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608026

REPORTS

FROM

COMMISSIONERS, INSPECTORS,

AND OTHERS:

TWENTY-SEVEN VOLUMES.

—(27.)—

SPONTANEOUS COMBUSTION OF COAL IN SHIPS; VIVISECTION.

Session

8 *February* —— 15 *August* 1876.

VOL. XLI.

1876.

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FROM

COMMISSIONERS, INSPECTORS,

AND OTHERS:

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TWENTY-SEVEN VOLUMES:—CONTENTS OF THE

TWENTY-SEVENTH VOLUME.

N.B.—*THE* Figures *at the* beginning of the line, *correspond with the* N° *at the foot of each* Report; *and the* Figures *at the* end of the line, *refer to the* MS. Paging *of the Volumes arranged for The House of Commons.*

SPONTANEOUS COMBUSTION OF COAL IN SHIPS:

[c. 1586—I.] Report of the Royal Commissioners appointed to inquire into the Spontaneous Combustion of Coal in Ships, together with Appendix, Minutes of Evidence, and Index - - - p. 1

VIVISECTION:

[c. 1397.] Report of the Royal Commission on the practice of subjecting Live Animals to Experiments for Scientific Purposes, with Minutes of Evidence and Appendix - - - - 277

[c. 1397—I.] Digest of the Evidence, with an Alphabetical List of Witnesses, 689

Vol. XLI.—Sess. 1876.

REPORT

OF THE

ROYAL COMMISSIONERS

APPOINTED TO INQUIRE INTO THE

SPONTANEOUS COMBUSTION OF COAL IN SHIPS,

TOGETHER WITH

APPENDIX, MINUTES OF EVIDENCE, AND INDEX.

Presented to both Houses of Parliament by Command of Her Majesty.

LONDON:
PRINTED BY GEORGE EDWARD EYRE AND WILLIAM SPOTTISWOODE,
PRINTERS TO THE QUEEN'S MOST EXCELLENT MAJESTY.
FOR HER MAJESTY'S STATIONERY OFFICE.

1876.

[C.—1586.-I.] *Price 3s.*

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COMMISSION.

VICTORIA R.

Victoria, by the Grace of God of the United Kingdom of Great Britain and Ireland, Queen, Defender of the Faith.

To Our right trusty and well-beloved Councillor Hugh Culling Eardley Childers, Our trusty and well-beloved Sir George Elliot, Baronet, Our trusty and well-beloved Henry Hussey Vivian, Esquire, Our trusty and well-beloved Frederick Augustus Abel, Esquire, Fellow of the Royal Society, Our trusty and well-beloved Richard Cory, Esquire, Our trusty and well-beloved George Duncan, Esquire, Member of the Committee of Lloyd's Register, Our trusty and well-beloved John Fenwick, Esquire, one of the Elder Brethren of the ancient Corporation of the Trinity House, Our trusty and well-beloved John Ferguson, Esquire, Our trusty and well-beloved Charles Icely, Esquire, Our trusty and well-beloved John Percy, Esquire, Fellow of the Royal Society, and Our trusty and well-beloved William Young, Esquire, greeting.

Whereas We have deemed it expedient for divers good causes and considerations that a Commission should forthwith issue to make inquiry with regard to the causes of the spontaneous combustion of coal in ships, and the remedies which it may be possible to adopt for preventing and guarding against the same.

Now know ye, that We, reposing great trust and confidence in your ability and discretion, have nominated, constituted, and appointed, and do by these presents nominate, constitute, and appoint you the said Hugh Culling Eardley Childers, Sir George Elliot, Henry Hussey Vivian, Frederick Augustus Abel, Richard Cory, George Duncan, John Fenwick, John Ferguson, Charles Icely, John Percy, and William Young, to be Our Commissioners for the purposes of the said Inquiry.

And for the better enabling you Our said Commissioners to make the said inquiries, We do by these Presents authorize and empower you, or any five or more of you, to call before you, or any five or more of you, such persons as you may judge necessary by whom you may be the better informed of the matters herein submitted for your consideration and every matter connected therewith, and to inquire of and concerning the premises by all other lawful ways and means whatsoever.

And also to call for and examine all such books, documents, papers, or records as you shall judge likely to afford you the fullest information on the subject of this Our Commission, and to inquire of and concerning the premises by all other lawful ways and means whatsoever.

And We do further by these presents authorize and empower you, or any five or more of you, to visit and personally inspect such places in Our United Kingdom as you may deem expedient for the more effectual carrying out of the purposes aforesaid, and to employ such persons as you may think fit to assist you in conducting any inquiry which you may hold.

And Our further will and pleasure is, that you, or any five or more of you, do, with as little delay as possible, report to Us, under your hands and seals, upon the matter referred to you as aforesaid, and that you may have power to certify unto Us from time to time your several proceedings in respect of the matter aforesaid if it may seem expedient for you so to do.

And We do further will and command, and by these presents ordain, that this Our Commission shall continue in full force and virtue, and that you Our said Commissioners, or any five or more of you, shall and may from time to time proceed in the execution thereof, and of every matter and thing therein contained, although the same be not continued from time to time by adjournment.

And for your assistance in the execution of these presents, We do hereby authorize and empower you to appoint a Secretary to this Our Commission to attend you, whose services and assistance We require you to use from time to time as occasion may require.

Given at Our Court at Saint James's, the Second Day of April 1875, in the Thirty-eighth year of Our Reign.

By Her Majesty's Command.

RICH^{D} ASSHETON CROSS.

REPORT.

TO THE QUEEN'S MOST EXCELLENT MAJESTY.

We, Your Majesty's Commissioners, appointed "to make inquiry with regard to the "causes of the spontaneous combustion of coal in ships, and the remedies which it "may be possible to adopt for preventing and guarding against the same," humbly submit to Your Majesty, in accordance with Your Majesty's commands, this our Report on the matters referred to us.

Origin of the Inquiry.

Correspondence between Board of Trade, Lloyd's, and London Salvage Association.

On entering upon this inquiry, a correspondence, commencing in April 1874, was laid before us by the Secretary to the Board of Trade, between that Department, the Committee of Lloyd's, and the London Salvage Association.

From this correspondence, which is appended to our Report, it appears that in April 1874, the Board of Trade decided to hold no more inquiries, under the Merchant Shipping Act, into casualties caused by explosions or fire in coal-laden vessels, for the reason that the finding of their courts, which invariably took the form of an exoneration of the ship's officers and a recommendation in favour of better ventilation, appeared to be entirely disregarded both by shipowning and underwriting interests.

Later in the year the Committee of Lloyd's, having furnished to the Board of Trade a return of the casualties to coal-laden vessels during the preceding 16 months, drew attention to the serious increase in the risk to life that appeared to be involved in the coal trade, and urged renewed investigation into such casualties, in order that the causes of these spontaneous combustions, hitherto so variously accounted for, having been ascertained and made public, such conditions in coal cargoes as were shown to be dangerous might, as far as possible, be provided against.

Unsatisfactory result of statutory inquiries held under the Board of Trade.

The Board of Trade still demurred to a renewal of their inquiries on the ground that no new facts were ever elicited, and that the conclusions, to which the courts had for years invariably arrived, had apparently been productive of no practical results. They submitted that there was no guarantee that the shipping interests would, in the future, pay more regard to the recommendations of the Courts of Inquiry than they had done in the past; and they suggested that some serious steps should be taken by those who own, manage, and insure these ships and cargoes, to profit by the facts already proved, and to secure the safety of their vessels by better ventilation, by the provision of means for extinguishing fire, and by caution in the description and condition of the coal shipped.

To this the Salvage Association replied by pointing out that the Courts of Inquiry hitherto held were not so constituted as to command the confidence of those concerned in the coal trade; that inquiries conducted, perhaps, by a justice of the peace and two assessors, were not calculated to elucidate a subject so difficult in itself, and rendered further obscure by the great conflict of opinions concerning it. They observed that the facts, which the Board of Trade alluded to as proved at these inquiries, were held generally to have been merely discussed, the conclusions arrived at by the court being of far too uncertain a character to be accepted or acted upon with decision. They therefore strongly urged that an inquiry should be held by a Commission consisting of men of high scientific attainments, and especially acquainted with the nature of coal in all its conditions, and of men acquainted practically with coal both at the pit and in the ship's hold. They pointed out that the result might be to fix with a certainty, absolute or qualified, the causes of this dangerous combustion of coal in ships, thereby attaching to the proper persons the responsibility for due precautions.

They expressed their confidence that the recommendations of such a Commission would receive attention and consideration from underwriters, shippers, and shipowners; and in support of this opinion they referred to a letter from Sir John Pirie & Co., strongly advocating an inquiry, so constituted, into the causes of spontaneous ignition, the best means for its prevention, and the most effective method for its extinction.

This proposal was referred by the Board of Trade in January 1875, to the Committee of Lloyd's, who expressed their hearty concurrence; and effect was given by Your

Majesty, to the suggestion,—so strongly urged in this correspondence on Your Majesty's Government,—by the appointment of a Royal Commission.

General Condition of the Export Coal Trade and the Extent and Circumstances of the Casualties from Spontaneous Combustion.

Information collected.

In the first instance it appeared to us desirable to collect information bearing on the general condition of the export coal trade, the methods of shipment employed at the various ports, the descriptions of coal exported, and the fittings employed in coal ships either for the ventilation or preservation of the cargoes. We also endeavoured to obtain statistics showing, as far as possible, the exact extent of the evil we were to deal with, and the circumstances in which it generally occurred.

Return of casualties, 1874 and 1875.

For this purpose we first obtained from the Committee of Lloyd's a list of the coal-laden ships which had been subject to casualties from fire in the year 1874, and by individual application to the owners and masters of these vessels we procured, so far as practicable, particulars in each case of the tonnage, class, size, build, age, and destination of the vessel, and of the name, tonnage, condition as to moisture and ventilation, and method of loading, of the coal carried, together with the circumstances attending the combustion.

A tabulated analysis of this return, which was subsequently continued for the year 1875, will be found in the Appendix.

Evidence, oral and documentary.

2ndly. We addressed a circular letter to the chambers of commerce at the principal coal shipping ports of Great Britain, requesting them to furnish us with lists of such shipowners, coal-shippers, masters, engineers, staithmen, and others, as they considered it would promote our inquiry to consult, either orally or by letter. The chambers of commerce responded with readiness to this request, and enabled us to obtain information by means of series of questions, copies of which are appended, from a large number of the most important interests connected with the coal trade. We examined some of the witnesses recommended to us by the Board of Trade, Chambers of Commerce, and others whose opinions were likely to be valuable, and we have further received evidence from officers, sent to us by the Admiralty, with regard to cases of spontaneous combustion and of explosion that have occurred in the coal bunkers of Your Majesty's ships, and in certain of the Government coal stores. The minutes of evidence are appended.

Lloyd's return of coal shipments for 1874, showing tonnage, destination, and casualties.

3rdly. We requested the Committee of Lloyd's to prepare for us a return which would show the number of coal shipments, and the quantities of coal shipped from British to foreign ports in 1874, dividing the shipments according to their destinations, classifying them according to the tonnage of the cargoes, and assigning to each group of shipments the casualties that had arisen from spontaneous combustion. This valuable return, the material for which was obtained from the "Bill of Entry Offices" at the ports, entailed great labour on those offices, and still greater on the clerical staff of Lloyd's, and we wish to take this opportunity of acknowledging the valuable assistance we have obtained from that Corporation in the statistical branch of our inquiry.

Scientific paper by Dr. Percy and Professor Abel.

4thly. We requested Dr. Percy and Professor Abel, two members of our Commission, possessing the requisite scientific knowledge, to draw up a paper which should describe, simply and clearly, the chemical conditions which tend to originate and develope spontaneous combustion and those which produce explosion. This paper, which we consider of great value in connection with our inquiry, is annexed to the Report.

General conditions of export coal trade.

From the information collected from these various sources it appears that the export of coal from the United Kingdom is steadily on the increase. In 1873, 12 million tons were sent out of the country; in 1874 nearly 13½ million; and, notwithstanding this exceptionally large increase, last year shows a further advance of half a million, or a total export of nearly 14 million tons.

But of these large quantities of coal which annually leave the country more than three-fourths are destined for European ports, and the returns for 1875 show that the increase in the export for that year is wholly in this direction.

Shipment of coal to European, as com-

How far the unusually large number of fires which occurred in 1874 in coal ships bound to distant and tropical ports, may, by alarming shippers and underwriters, have influenced the shipment of coal on long voyages, we cannot say; but it is remarkable that the export to Africa, Australia, the East and West Indies, and North and South America, which increased by 340,000 tons or over 14 per cent. in 1874, as compared with 1873, show a decrease in 1875 as compared with 1874 of upwards of

190,000 tons, or nearly 7 per cent.; while the export to Europe which in 1874 increased at a lower rate than that to more distant ports (namely at not quite 10 per cent.) still shows an increase of $7\frac{1}{2}$ per cent. in 1875, or a total of 11,400,000 tons as compared with 10,600,000 in 1874. Setting against this the decrease to distant ports we find an increase last year on the total export of coal from the country of over half a million tons. Of this increase more than half went to France which is by far our largest customer.

pared with more distant ports.

Comparative export of coal from the chief coal shipping ports of United Kingdom.

The coal thus annually exported from the United Kingdom is shipped chiefly from the Tyne, the Wear, the Mersey, and the Bristol Channel. The Scotch and Yorkshire ports also sending out a considerable amount.

In round numbers, the following was the export from these ports in the years:

——	1873.	1874.	1875.
From the Tyne -	Over 3 million tons	Over 3 million tons	Over $3\frac{1}{2}$ million tons
" Wear -	Under $1\frac{1}{2}$ million "	Under $1\frac{1}{2}$ million "	Nearly $1\frac{1}{2}$ million "
" Bristol Channel -	Over $3\frac{1}{2}$ million "	Nearly 4 million "	About $3\frac{3}{4}$ million "
" Mersey -	Over $\frac{1}{2}$ million "	Nearly $\frac{3}{4}$ million "	Under $\frac{3}{4}$ million "
" Scotch ports -	Over 1 million "	Over $1\frac{1}{2}$ million "	Over 2 million "
" Yorkshire ports -	Over $\frac{1}{2}$ million "	Over $\frac{3}{4}$ million "	Over $\frac{3}{4}$ million "

Turning from the extent and conditions of the coal shipments for foreign export, we come to consider the extent and circumstances of the casualties from fire, occurring among them, which form the subject of our inquiry.

Extent and circumstances of the casualties in 1874.

From the return prepared by Lloyd's to which allusion has already been made (and which is tabulated in the Appendix) it appears that among 31,116 shipments in 1874, amounting to 13,369,012 tons of coal, the casualties were 70. But of these shipments, 26,631, amounting to 10,513,181 tons of coal, were to European* ports, and only suffered from combustion in 10 cases. This leaves 60 casualties among only 4,485 shipments, amounting to 2,855,831 tons of coal to Asia, Africa, and America.

Again this return shows, to a startling extent, that the proportion of casualties traceable to spontaneous combustion increases *pari passu* with the tonnage of the cargoes. This becomes still more apparent when the European trade is deducted. The return then shows that there were, in 1874:—

2,109 shipments, with cargoes under 500 tons, in which 5 casualties occurred,—or under $\frac{1}{4}$ per cent.;
1,501 shipments, with cargoes between 500 and 1,000 tons, in which 17 casualties occurred, or over 1 per cent.;
490 shipments, with cargoes between 1,000 and 1,500 tons, in which 17 casualties occurred, or $3\frac{1}{2}$ per cent.;
308 shipments, with cargoes between 1,500 and 2,000 tons, in which 14 casualties occurred, or over $4\frac{1}{2}$ per cent.; and
77 shipments, with cargoes over 2,000 tons, in which 7 casualties occurred, or 9 per cent.

The casualties in vessels bound to San Francisco were the most remarkable. Deducting vessels under 500 tons (in which no cases of spontaneous combustion were recorded), the return shows 9 casualties out of 54 shipments. These also increase in proportion to the tonnage of the cargoes, till we arrive at the alarming fact that out of the 5 ships with cargoes of over 2,000 tons sent to San Francisco in 1874, 2 suffered.

Thus it becomes evident that spontaneous combustion has occurred principally in vessels carrying over 500 ton cargoes, bound to the West Coast of South America, San Francisco, or to Asiatic ports beyond the Mediterranean and Black Seas. Of such shipments over 4 per cent. suffered in 1874; and though they only amounted to 1,181 out of the 31,116 shipments for the year, yet among them occurred 51 out of the 70 casualties.

In conducting our inquiry into the causes of spontaneous ignitions in coal cargoes, the chief points which we have found ourselves called upon to consider have been:—

Chief heads of inquiry.

1. Descriptions of coal dangerous for shipment on long voyages;
2. The breakage to which coal is subjected by the various methods of loading employed at the several ports;
3. The effect of moisture;
4. The through ventilation of coal cargoes;

---

* This includes all ports in the Mediterranean and Black Sea.

5. The providing of means for testing the temperature of coal during the voyage; and
6. The question of explosions in coal cargoes, which, though not specifically referred to us, we have ventured to consider within the terms of our inquiry.

Evidence as to dangerous descriptions of coal.

1. *Descriptions of Coal dangerous for shipment.*—This is a very difficult, and delicate, and at the same time a very important, branch of our inquiry. There can be no doubt that certain kinds of coal are of such a quality as to be entirely unsafe for shipment at all, while other kinds should only be shipped for short voyages and under circumstances of great precaution. We desire, as far as possible, to contribute, with regard to these coals, any information which may be valuable in averting danger from life and property, and we would therefore direct attention to the evidence, as to the character of certain coals, given by Mr. Cooper Rundell, Mr. Derry, Mr. Harwood, Mr. Beynon, Mr. Bladon, Capt. Steinson, Mr. Roberts, Mr. Scott, Capt. Cawkitt, and especially by Mr. Glasbrook. All coals mentioned in evidence before us will be found in their proper places in the Index.

Difficulties in obtaining complete information as to character of coal.

It would, however, be of course impossible for us to cite with completeness or accuracy the various coals to be avoided, or their relative susceptibilities to spontaneous heating. The difficulties are two-fold, first, because coals of very different character in this respect are produced from the same colliery; and secondly, because the conditions of shipment may be totally dissimilar. Nor can chemical analysis be relied upon, in every case, to furnish conclusive evidence as to the tendency to ignition. On this subject a letter from Mr. Ferguson, Professor of Chemistry at Glasgow University, will be found in the Appendix.

We are, however, inclined to think that the character in this respect of each class of coal is pretty generally known in the locality where it is worked (see Mr. Bladon's evidence, Q. 1,557, 1,558, and 1,560); and we consider that what is most needed, to avoid the shipment of dangerous coal, is thorough inquiry on the part of coal shippers, into the character of the coal they propose to ship, and subsequently a careful scrutiny of the coal before shipment in order to ensure that the coal contracted for has been actually supplied.

On this point the evidence of Mr. Roberts, Inspector for the Peninsular and Oriental Company at Cardiff, is worthy of attention, and we would refer especially to the answers given by him in evidence to Questions 2,000–2,005.

It appears to us, therefore, most desirable that facilities should be provided for tracing more certainly and easily where the coal, shipped in any particular vessel, comes from. Such means of information, in the case of cargoes which had heated, would enable coal shippers to learn from the accumulation of experience the character of the coals with which they had to deal.

As it is at present, a cargo of coal is constantly taken without even a name being given to it. If heating occurs in such a cargo, the dearly bought experience may prove valueless, there being no means of tracing the coal with which the vessel was loaded.

Provision recommended for securing records of the descriptions of coal exported.

It is of course impossible to insist by law that shippers should inquire into the character of the cargoes they carry, but we consider that the means of doing so, where a cargo has proved dangerous to human life, should be secured. We would suggest, therefore, that, on the "specification (in lieu of shipping bill or Customs bill of loading)" which every exporter has to deliver to the officers of Customs within six days from the time of the final clearance of the ship, as provided by the 23 Vict., cap. 22, he should be required to state the denomination of the coal carried and where it comes from, as well as its quantity and value. The form, as at present drawn up (of which specimen is appended), requires him to state the "quantity and *description* of goods," "but the goods need only be described in accordance with the denomination in the "official list of British and Irish produce and manufacture exported," and in the case of these cargoes the denomination at present given is simply "coal." These specifications are noted at the Custom House in London, and kept with the ship's papers. Were the exporter required to name the coal, or, in a mixed cargo, the several descriptions of coal, loaded in the vessel, and the same penalty extended to a false statement in this respect as now attaches to a false statement of quantity, facilities would be given for tracing to its source, and inquiring into the character of, any cargo of coal in which spontaneous combustion might occur. We would observe that the information we propose to make compulsory is already voluntarily given in the specifications by several coal shippers, especially at the Welsh ports.

Suggestion that Inspectors of

Having made this practice general, we would recommend that whenever a vessel loaded with coal from a British port is reported to have suffered from spontaneous combustion, reference should at once be made to the Customs authorities for the description of the coal given by the exporter, and that it should then be the duty of the Inspector of Mines for the district from which the coal was stated to have come to inquire into the case.

Mines should hold inquiries into all cases of combustion occurring in coal from their respective districts.

This inquiry would not be of the character of a marine inquiry, nor need it interfere with any investigation which the Board of Trade might think it desirable to make. The Inspector would be directed to ascertain as far as practicable the particular seam or seams from which the coal was wrought, the natural character of the coal, its condition at the time of the casualty, the manner and condition in which it was shipped, and other circumstances.

Legislation.

Some legislative provision may be requisite to enable the Departments of Government concerned to carry out these recommendations, but with this exception we advocate no legislation on the subject of our inquiry.

Shipment of small or inferior coal, or of coal insufficiently cleaned.

In connection with the subject of dangerous coal, we desire to draw attention to certain evidence given before us bearing on the shipment of small or inferior coal, and of coal insufficiently cleaned.

Mr. Cooper Rundell, accountant to the Liverpool Underwriters' Association, who had collected for his employers a variety of statistics relating to spontaneous combustion in coal ships, was of opinion that want of care in the selection of coal had a good deal to do with the increase of casualties in 1873 and 1874 (Q. 134). He considered that "the coals shipped in 1874 were most probably not so free from pyrites and small " coal as in the case of the coals shipped in the preceding years" (Q. 135,) and that "the coal agents found a market for the inferior coal they were able to sell and get rid of" (Q. 136). He also pointed out that in consequence of the Mines Regulation Act, which came into force in 1873 and restricted the labour of children, a great many of the boys, who were formerly employed in cleaning and selecting the coal, were no longer available (Q. 137). He adds, "There has been a great demand " for coal, and a ready sale has been found for inferior and unclean sorts, which " would not formerly have been marketable. I have been told this, that formerly " men were fined for sending up bad coal, but that they are not fined for doing so " now. The pitmen, it is stated, would have been fined for sending up so much " bad stuff, and the stony and metallic part of the coal which did come up would be " carefully separated at the pit's mouth. Now the coal agent, in order to meet the " demand, appears to accept anything passed to him." (Q. 137 and 139.)

On the effect of the Mines Regulation Act, Captain Steinson was subsequently examined as follows: *Q.* "You mentioned that the boys who formerly picked out " the pieces of brassy coal no longer do so?—*A.* Yes." *Q.* "At what date was " the practice of employing boys to pick out the brassy pieces of coal discontinued?— " *A.* It has gradually been discontinued since the Mines Regulation Act came into " force." *Q.* "When did the Mines Regulation Act come into force?—"Within the " last two or three years." *Q.* "Those accidents have been more rife since the " cleaning of the coal has been neglected?—*A.* The men have been getting more " careless in every way since then; since they get the big wages they have been getting " more careless." *Q.* Are you of opinion that the coal has been much more carelessly " cleaned since 1872 than previously?—*A.* I will not say much more carelessly; it is " not so carefully cleaned." *Q.* "Have the places of these boys been filled by anyone " else?—*A.* There is a screener at each side of the screen; he is supposed to pick out " the brasses." *Q.* "Did the boys supplement those men?—*A.* The boys supplemented those men." *Q.* "Therefore there was a double check, now there is only a " single one?—*A.* Only a single one." (*Q.* 1736–1743.)

On the subject of the shipment of small coal, Mr. John Bladon, a coal shipper at Newport, said, "I think that the shipment of small coal at Newport in large quantities " is rather a new trade. I think that formerly the small coal shipped at Newport was " generally considered smith's coal, and it was generally sent to the various ports " round the coast, but the shipment of small coal for foreign districts is somewhat of " a new trade." (Q. 1559.) He further said, "There is no doubt that many fresh coals have come into the market within the last few years." (Q. 1560.)

Systems of loading in relation to breakage of coal.

2. *Breakage of Coal.*—This seems generally to be recognised in the trade as an evil to be guarded against; and in advocating the various systems of loading employed at the several ports, their relative merit in avoiding breakage has been always accepted by witnesses as the gauge of their efficiency. It is also important to observe that the experience, in this respect, of shipowners, masters, and shipping agents is strongly

confirmed by the conclusions laid down in the paper drawn up by the scientific members of the Commission to which we desire to draw particular attention.

Visits of Commission to coal shipping ports.

So important did this question of the breakage of coal in loading seem to us, that we thought it desirable to visit the chief coal shipping ports, and inspect for ourselves the working of the various systems.

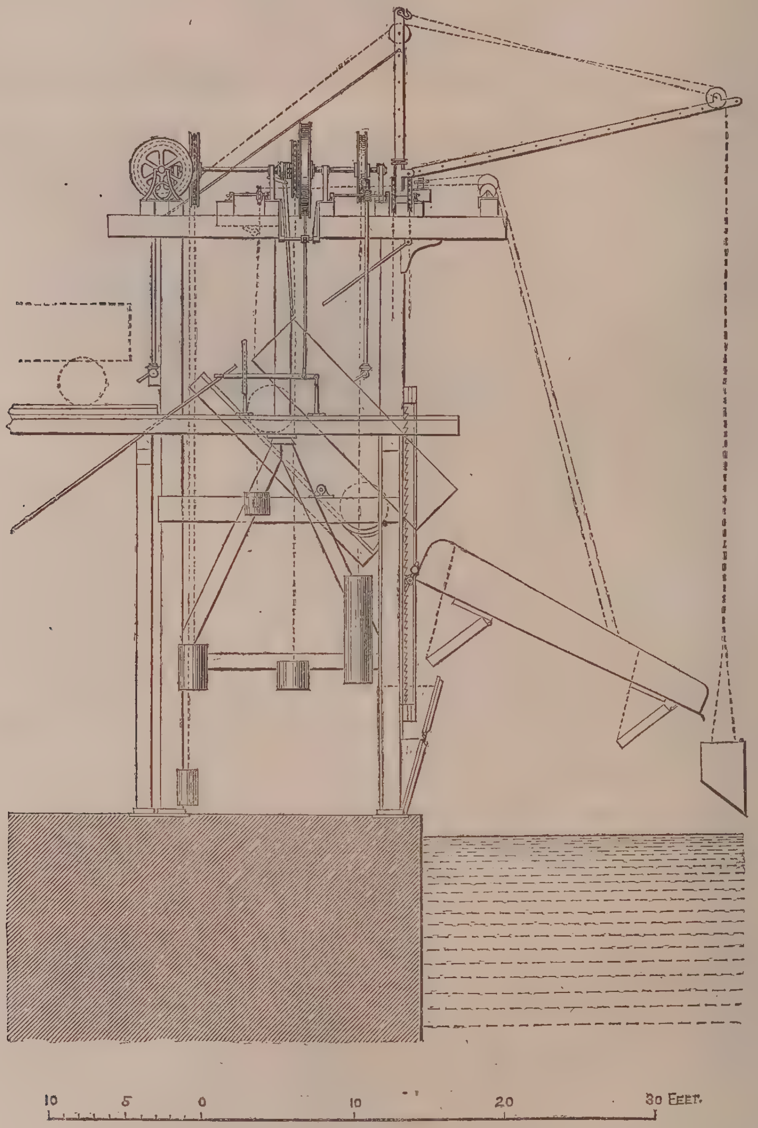
System of loading in South Wales.

Taking first the ports in South Wales, we found at Cardiff that, at the west and east docks, the coal is loaded from a high level by balance tips (also called drops), but that at the new basin the low level system, with hydraulic tips, has been adopted.

Balance tips or drops.

In the balance tips, of which the following are sketches,* the waggon runs from a high level of rails, 22 to 28 feet above the dock coping, on to a platform or cradle. This platform, in the tips of earlier construction, as shown in Fig. 1, is supported by balance weights sufficient to raise it when carrying an empty waggon but unequal to the weight of a loaded one. These weights are connected to the platform by chains passing over pulleys at the top of the framing, upon which are breaks for controlling the motion of the platform. When a full waggon has been run on to the platform a suspended chain is hooked on to its tail, and the platform is lowered by means of the breaks until the waggon is tipped sufficiently. The coal is then discharged through an end door into an inclined shoot which reaches the ship's hatchway. The break is then released, and the platform, with the empty waggon, rises again to the level of the line of rails, and the waggon is run off into a siding, leaving the platform ready for a fresh load. The shoot down which the coal passes to the ship can also be adjusted to any height and at any angle, and has screens at

No. 1.



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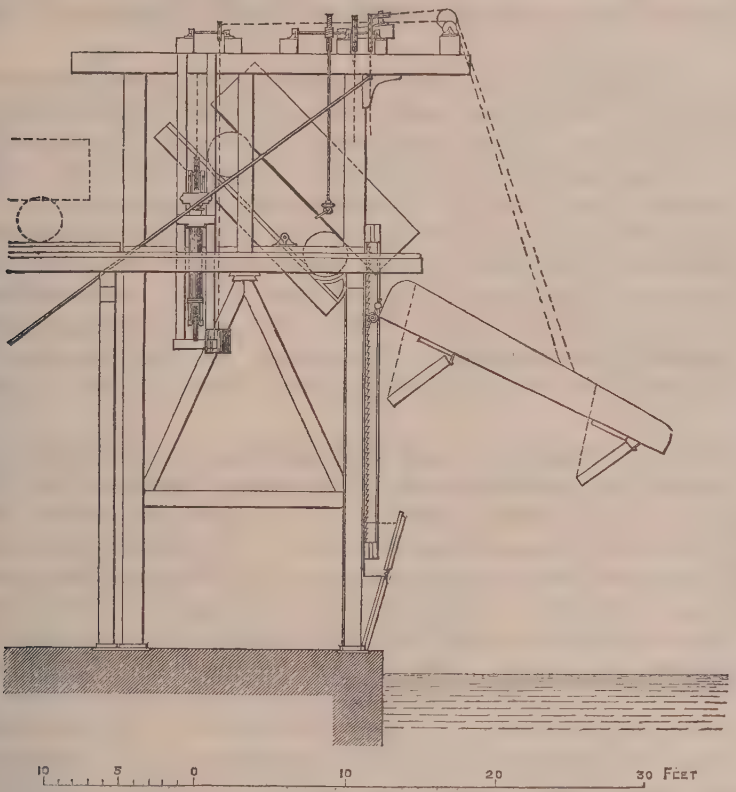
* For the particulars and sketches of these systems of loading in use at the South Welsh ports, we are greatly indebted to a paper by Mr. John McConnochie, of Cardiff, engineer to the Bute Harbour Trust.

the bottom for separating the small coal, but these are not always left open. The shoot tapers towards the end, and is generally furnished with a door, so as to check the too rapid descent of the coal.

In the tips more recently constructed this system of tipping, by securing the tail end of the platform with a chain and lowering the other end to the shoot, has been superseded by the introduction of a hydraulic lift, which raises the tail end of the platform until the waggon is tipped to the desired angle of incline, as in Fig. 2.

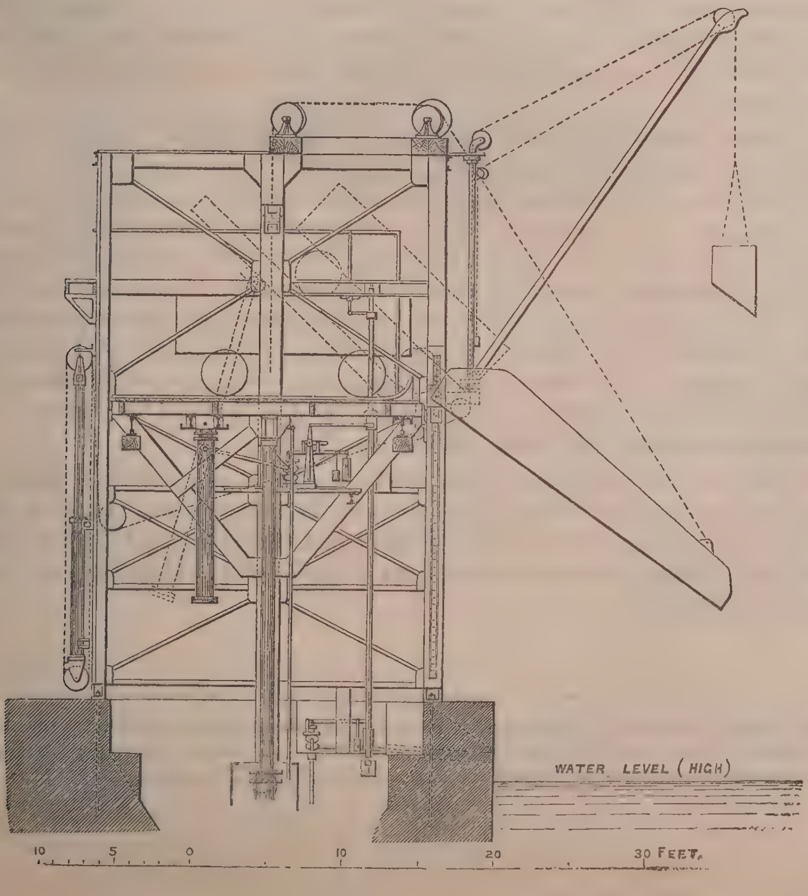
No. 2.



At the new basin, served by the Great Western Railway, which comes into Cardiff on the low level, hydraulic tips have been fitted, of which the following is a sketch :—

Hydraulic tips.

No. 3.



By this method the waggon of coal runs on to a platform level with the quay, and is raised by a hydraulic lift to the height required to tip the coal down the shoot into the hold of the vessel, the waggon being tipped by hydraulic pressure as in the case of the high level balance tips. At low water it is unnecessary to raise the platform, and the cradle, with the rails upon which the waggon rests, is simply tipped, the tail end being raised by hydraulic pressure.

There is little to choose between the hydraulic and balance tips either in facility or rapidity of loading, each tip being capable of shipping about 560 tons of coal a day of 10 hours, and of course, as regards breakage of coal, the effect is the same in both cases.

Antibreakage boxes.

In any system of loading by tips this breakage necessarily occurs to the greatest extent in shipping the first waggon loads, when the coal has to fall from the mouth of the shoot to the bottom of the ship's hold. To meet this serious evil a machine called the anti-breakage box is much used at the south Welsh ports. (For illustration, see preceding sketches, Nos. 1 and 3.) It consists of a hopper-shaped iron box with a hinged flap for discharging at the bottom, and it is suspended from a crane close under the mouth of the shoot. The coal is then lowered by it from the shoot, a ton at a time, to the bottom of the ship's hold, until a cone of coal is formed under the hatchway, the top of which is level with the deck. The box is then removed and the coal allowed to run directly from the shoot. The use of the box, however, seriously delays the loading of the coal in its earlier stages, and unless insisted upon is liable to be neglected.

Box system of loading.

The same systems of loading in use at Cardiff are also prevalent at Newport and Swansea. At Cardiff and at Swansea the box system, introduced by Mr. Brunel, also exists, though to a very limited extent. At Cardiff we believe that the Powell Duffryn Co. are the only shippers who still use it. By this system the coal is loaded, at the collieries, into iron boxes, each containing 2½ tons, and the boxes are conveyed, two on a waggon, to hydraulic hoists fitted on the quay. Each box is constructed with double doors at the bottom, and has three chains, one attached to the box itself and the two others to the doors. The waggon having been run under the hoist each box is lifted off it, and lowered through the ship's hatchway until the bottom of the box is close to the ship's bottom, or upon the surface of the coal already shipped. The doors are then opened by means of the chains and the contents let out. This system has fallen into disuse owing principally to the change on the railway from broad to narrow gauge. It is also less expeditious than the tips, and is generally considered a more expensive method of loading, but it clearly breaks the coal to a far less extent.

Systems of loading at Liverpool.

On the Mersey coal ships are generally loaded by the appliances of the Mersey Docks and Harbour Board. At Liverpool, at the high level railway, the coal is mostly put on board either by cranes, or by hand tips. The following are sketches* of these two methods (page 9).

The crane.

The crane, which loaded 363,000 tons in 1875 as against 73,000 loaded by hand tips, is worked by hydraulic pressure, and lowers the waggon-load of coal to the hatchway of the vessel, when the bottom of the waggon is opened and the coal is poured into the ship. The waggon cannot be lowered through the hatchway, as in the box system, so that the initial fall of the coal is the whole depth from the hatchway to the bottom of the ship, this fall decreasing as a cone is gradually formed, until the top of the heap is level with the lowest hatchway. It is then trimmed until the lowest compartment is filled, when the process is repeated. All the coal therefore in the lowest division of the vessel, supposing her to be a two or a three decker, must fall at least the distance from the uppermost to the lowest hatchway. Working at the quickest speed a crane can load 100 tons an hour.

The hand tips at the high level are used for shipping coal into flats; the coal by this method has a fall first from the waggon into the shoot, then a fall of about 6 feet at the turn of the shoot, and another fall from the end of the shoot into the vessel. The box system, as used at Swansea and Cardiff, exists also at Liverpool, but it is not largely resorted to.

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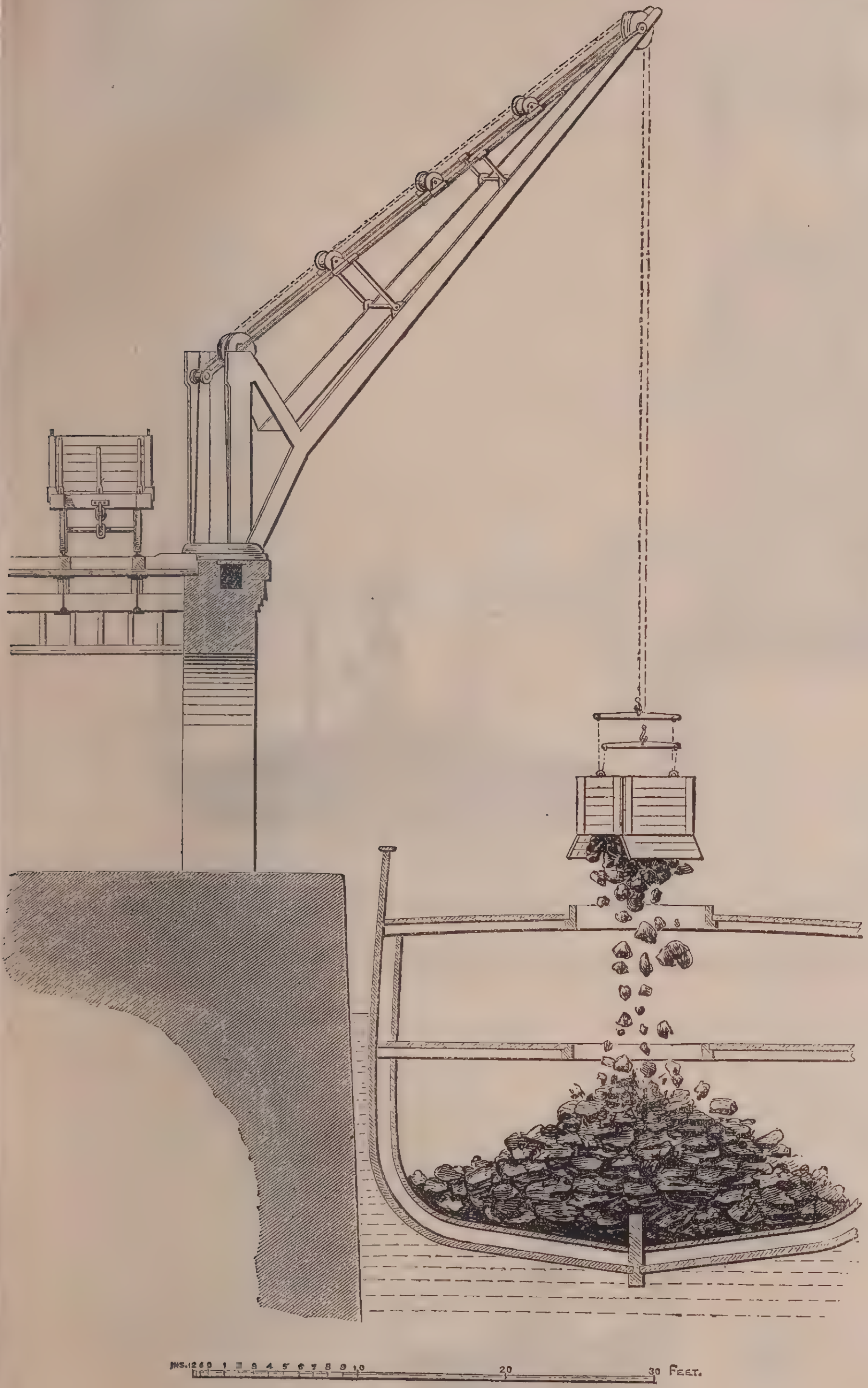
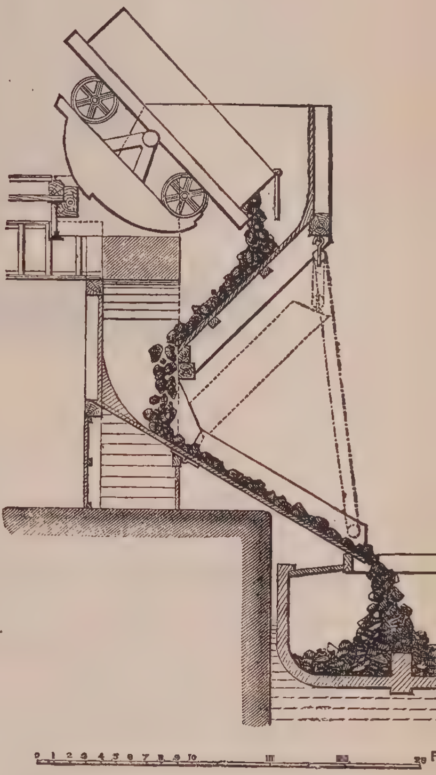
* These sketches, and those of the systems in use at Birkenhead, have been kindly forwarded to us by the secretary to the Mersey Docks and Harbour Board. Also some interesting statistics, relating to the shipment of coals from the port of Liverpool, which will be found in the Appendix, page 118.

No. 4.



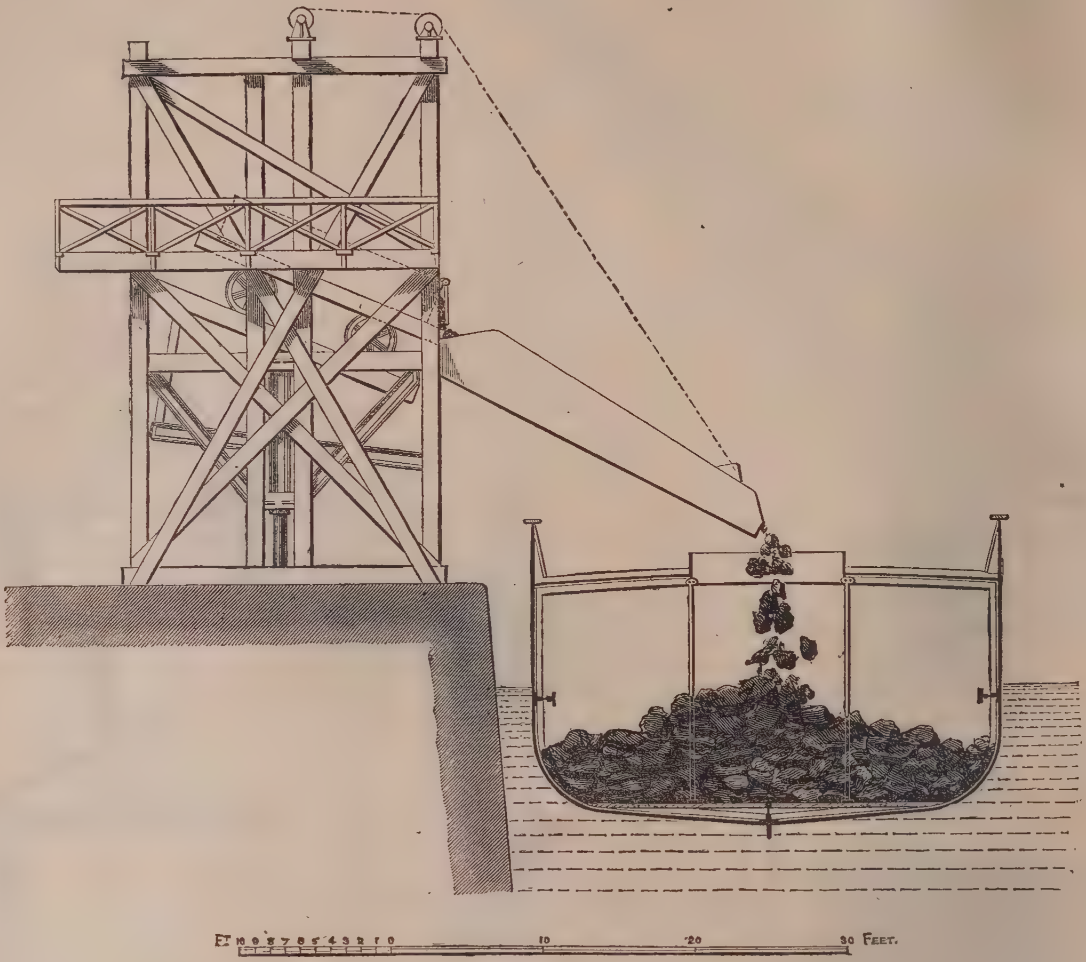
No. 5.



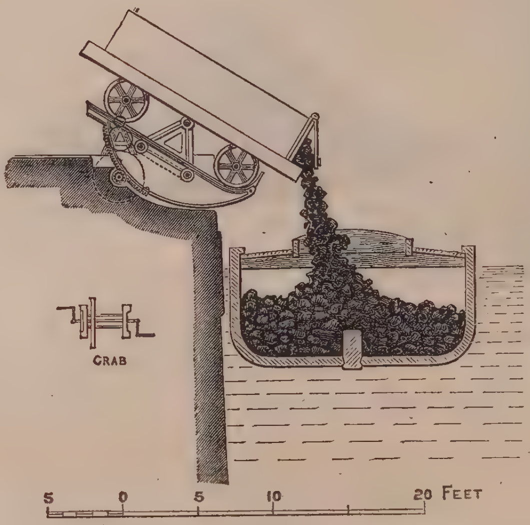
Systems of loading at Birkenhead.

At Birkenhead where the lines are on the low level, the Mersey Docks and Harbour Board appliances consist of hydraulic hoists and hand tips, of which the following are sketches:—

No. 6.



No. 7.



Hydraulic hoist.

The hydraulic hoist is somewhat similar in principle to that used in the Bristol Channel, but without the anti-breakage boxes. Only 77,000 tons were loaded by this method last year at Birkenhead, as against 363,000 shipped by the hydraulic cranes at Liverpool.

Hand tips.

The low level hand tips are only used for flats and lighters.

Loading by barrow.

Besides these methods upwards of 254,000 tons of coal were loaded during 1875 into sailing vessels by barrows.

By this method the coal is taken from the waggon in barrows which are lowered into the ship's hold, and there wheeled along planks to the place where the coal should be discharged, thus avoiding all the disturbance consequent on trimming and all serious fall of the coal. This is, however, by far the slowest process of loading, and we found that, to save the time and trouble required to lower the barrows into the hold, the coal was often discharged from the barrow down the hatchway.

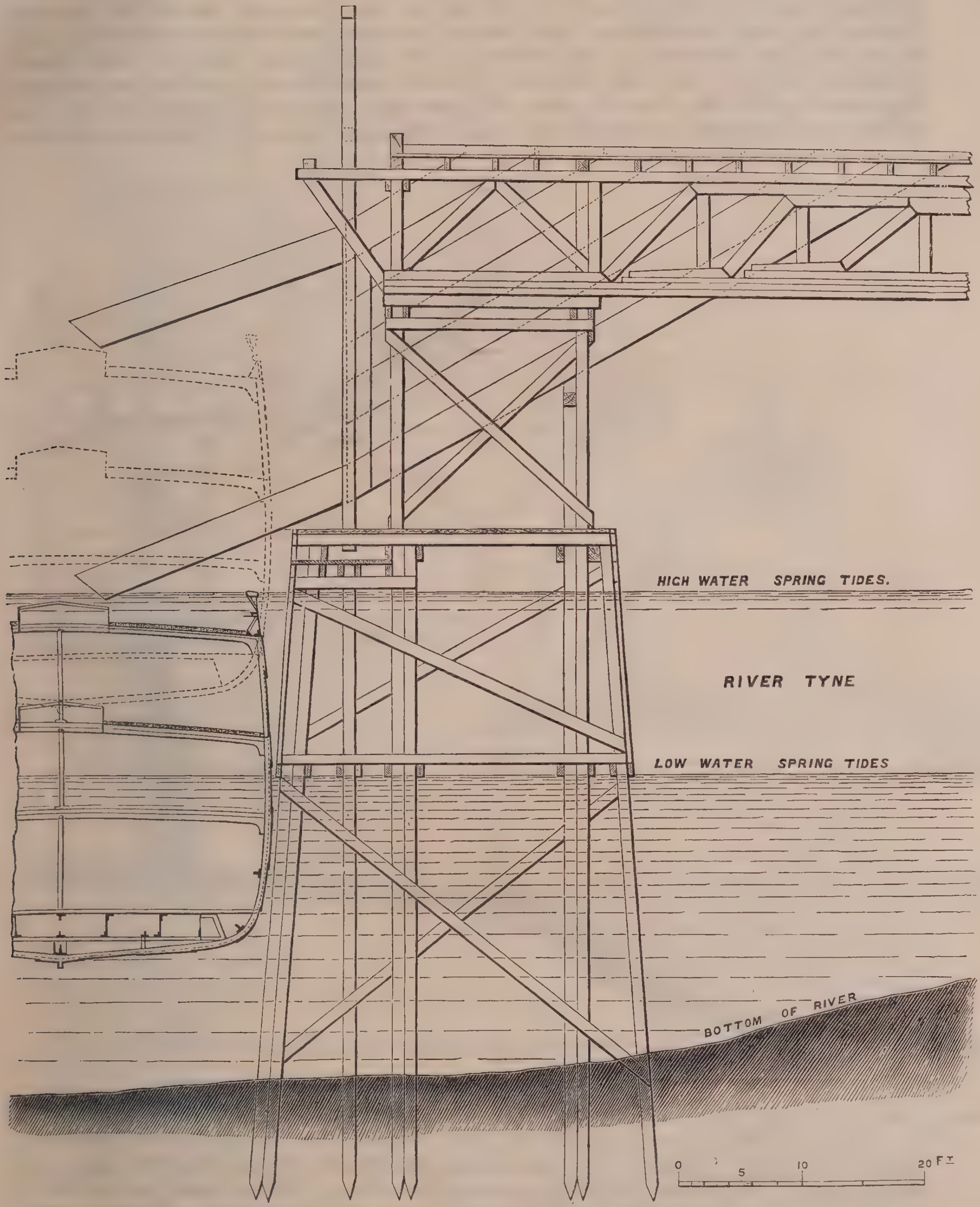
Systems of loading on the Tyne.

On the Tyne, the two systems in use are called the "spout" and the "drop."

The "spout.

The following is a sketch* of the coal staiths of the River Tyne Improvement Commissioners at Whitehill Point, and illustrates the "spout" system :—

No. 8.



By this method a sliding shoot is fitted to the mouth of whichever of the seven spouts, represented in the sketch, the height of the tide, and the depth of the vessel in the water, renders most suitable for bringing the end of the shoot directly over the ship's hatchway. The waggon is then run over the entrance to the spout, and its bottom opened. The coal falls some three to five feet into the spout, slides down the incline and along the shoot a distance, varying according to the spout used, from 35 to 75 feet, and then drops from the end of the shoot into the ship's hold. The height from the line of rails on which the waggon stand to the ship's hatchway would vary according to tide from about 15 to 35 feet. The coal is not trimmed until a cone is formed reaching to the hatchway, when the fall from the end of the shoot becomes reduced to two or three feet. The trimmers then spread the coal as it is shipped, assisted by smooth steel plates, which they place on the sides of the cone. The anti-breakage boxes do not appear to be in use on the Tyne, and the fall of the coal forming this cone, especially of that forming its base, must be very considerable, the depth from the hatchway to the bottom of the hold being often in large vessels upwards of 20 feet. As in the shoots at the Welsh ports the end is in the first instance kept closed until the shoot is quite full, and then the coal is allowed to run out, sliding down the spout in a continuous mass at the same rate as it is filled in at the top.

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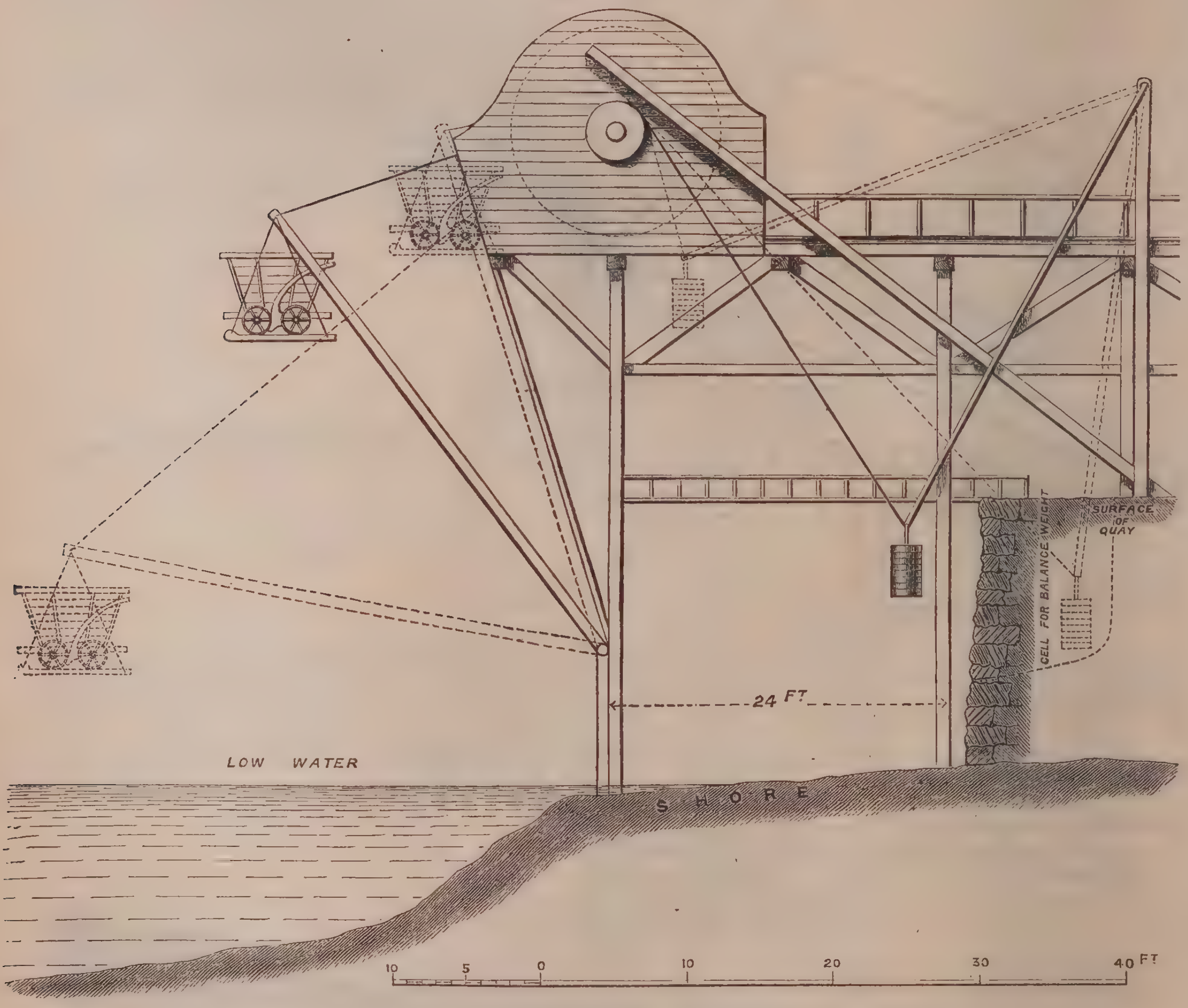
* The sketches of the systems of loading on the Tyne were kindly forwarded to us by Mr. Philip Messent, engineer to the River Tyne Improvement Commission.

No. 9.



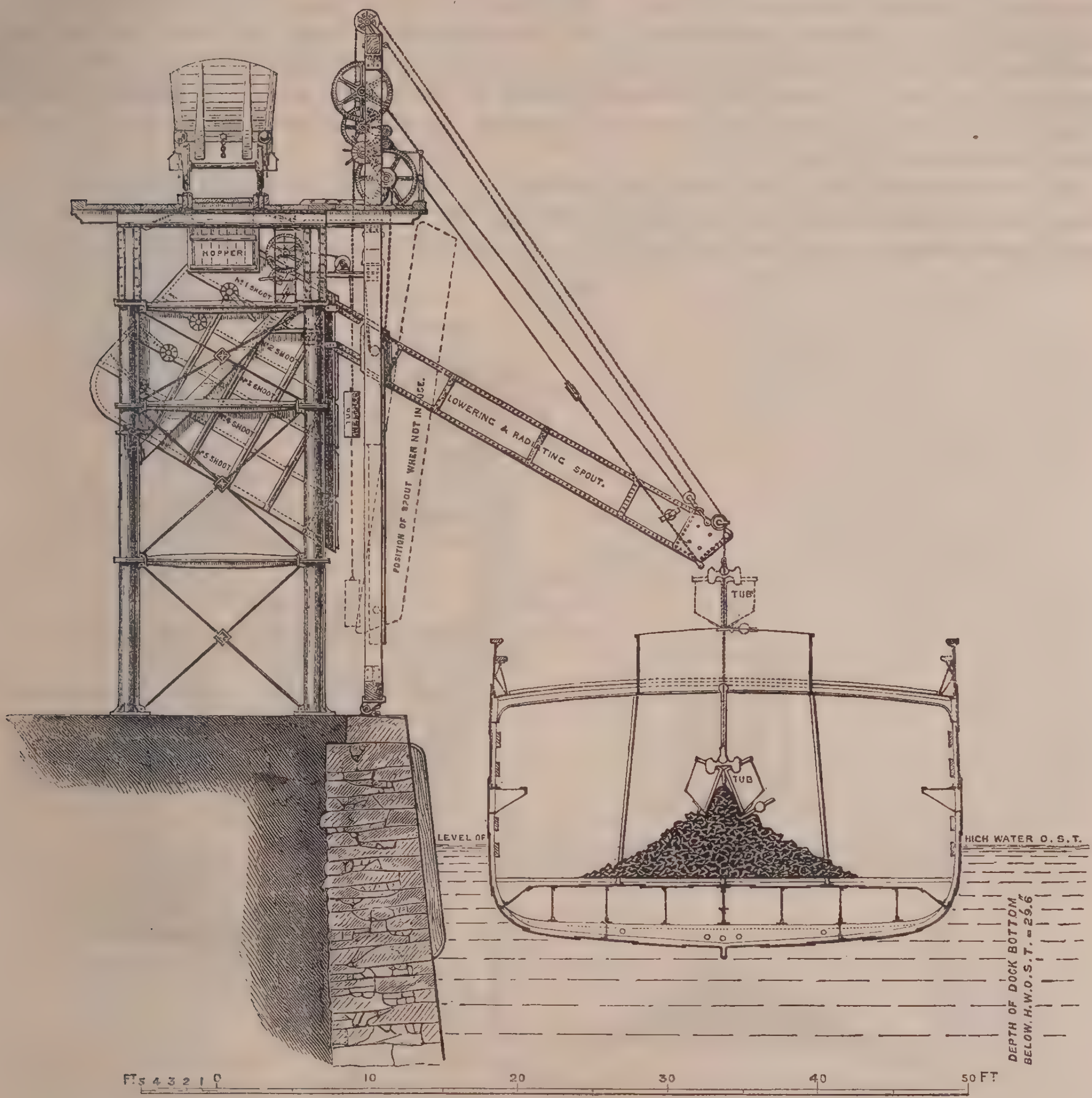
The "Drop." Of the "drop" system, illustrated above, there are only six or eight specimens on the river, and they are only adapted for small vessels. The system is being gradually superseded by the "spout." The fall of the coal from the waggon into the spout, and its subsequent disturbance during its passage down the spout and shoot, is avoided by the "drop," which lowers the loaded waggon to the ship's hatchway. In this system, as with the spout, the coal is not trimmed until a cone is formed rising to the level of the ship's deck; but, as the waggons cannot be lowered through the hatchways, the initial fall of the coal forming this cone is represented, as in the case of the spout system, by the distance from the coaming of the hatchway to the bottom of the empty ship.

Captain Steinson,* coal inspector on the Tyne and Wear for the Peninsular and Oriental Co., and for the Admiralty, India Council, and Crown Colonies, described a lowering apparatus formerly used at Newcastle as satisfactory in result, but it had to be given up on account of the aversion of the men to working it, there being some risk of accident unless it were carefully handled. Captain Steinson had since adopted a system of platform on the lower beams in order to break the fall of the coal.

Systems of loading on the Wear. The "Spout.

On the Wear the systems employed are the "Spout" and the "Gooseneck Drop." The following is a sketch† of the spouts at the new staith erected at Hendon dock.

No. 10.


HOPPER
No 1 SHOOT
No 2 SHOOT
No 3 SHOOT
No 4 SHOOT
No 5 SHOOT
TUB WEIGHT
POSITION OF SPOUT WHEN NOT IN USE.
LOWERING & RADIATING SPOUT.
TUB
TUB
LEVEL OF HIGH WATER O. S. T.
DEPTH OF DOCK BOTTOM BELOW H.W.O.S.T. = 26.6
FT 5 4 3 2 1 0
10
20
30
40
50 FT

It is approached by heavy concrete retaining walls, nearly 600 feet long and about 40 high. From the retaining wall the staith is carried on iron columns decked with rolled iron and creosoted timber. The kip of the staith consists of two lines of rails which provide standing for 350 tons of coal. The lines for the loaded waggons have a fall towards the first spout, where they join, and then rise as a single line to the end of the gearing. The line for the return of the empty waggons is a single line with a return fall, so that the waggons are worked entirely by gravitation. There are two spouts, each with five shoots, to allow for the shipment of coals at various heights, according either to the rise of tide or the size of the vessel. The centres of the two sets of shoots are 50 feet apart, but, by a simple arrangement, the two spouts into which the shoots discharge can be moved in a lateral direction either to the right or left, so that the distance between the mouths of the spouts may be varied from 30 to 70 feet, in order to meet the requirements of any ship's hatches. The height of the rails above the high-water level of the Hendon dock is 43 feet, the height of the opmost shoot is 31 feet 6 inches above the same level, the height of the lowest shoot being 18 feet 9 inches, the dock having a depth of water of 29 feet 6 inches. During shipment, a set of eight waggons containing 32 tons of coal are brought down to the spouts, and the contents of two waggons are discharged simultaneously into the two hoppers, so that 16 tons of coal are run down each spout at one and the same time. By this means the spouts are capable of shipping at the rate of 500 tons per hour.

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* Captain Steinson has been coal inspector for the P. & O. Co. for over 14 years, and in 1864, 1865, and 1866 he inspected for the Egyptian Government. His evidence appeared to the Commission to result from long and valuable experience, and to deserve especial consideration.

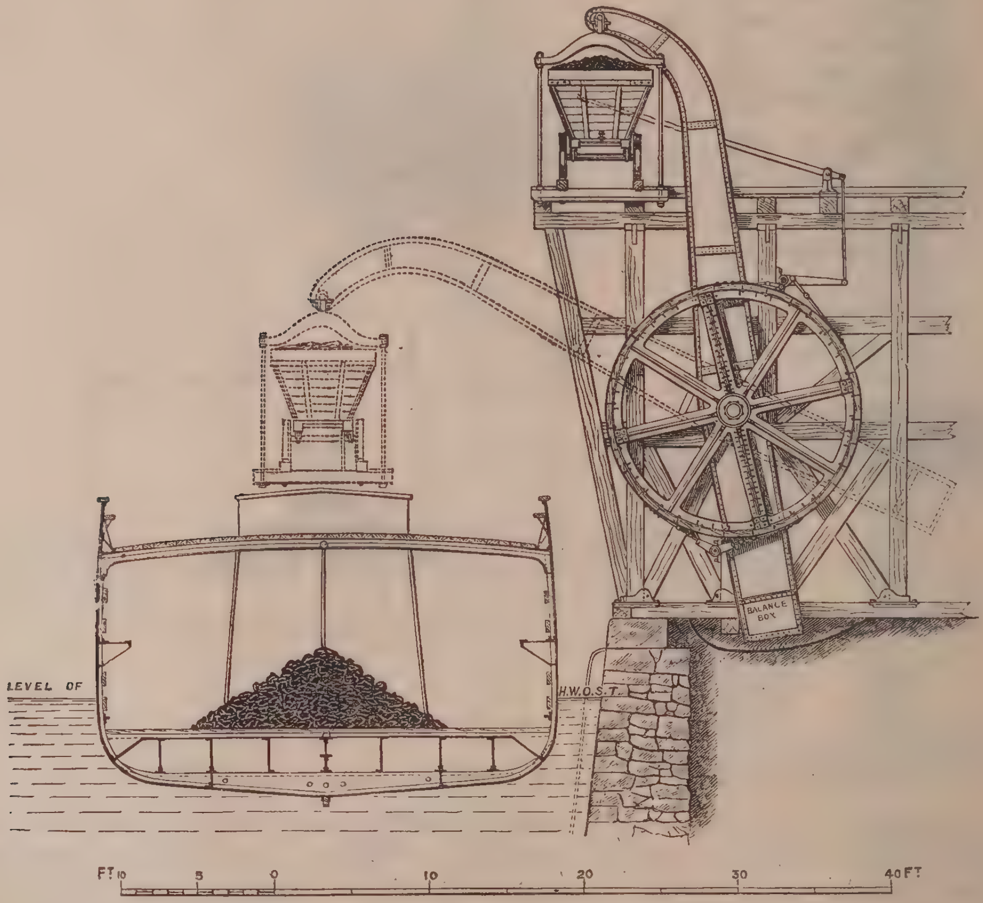
† We are indebted to Mr. Wake, engineer to the River Weir Commission, for the sketches of the systems of loading in use at Sunderland.

The breakage of coal by this system of loading must be very considerable, indeed greater even than by the tips, for when the waggon is tipped the coal runs with less force and fall from the end of the waggon into the spout or shoot than when it is allowed to drop in a mass through the bottom of the waggon into the spout. At Sunderland the fall from the waggon into the first shoot must be over six feet, and from the bottom of the waggon to the lowest shoot must be about 20 feet. The anti-breakage box or tub is used, as in South Wales, in the earlier stages of loading, for the formation of a cone of coal to lessen the breakage in the fall from the mouth of the spout into the ship's hold.

The "Gooseneck drop."

The "Gooseneck drop," of which the following is a sketch, is worked simply by the break wheels, and lever break, the weight of the loaded waggon taking down the drop to the distance required, where it is retained, by means of the break, until the waggon is emptied. The lower ends of the lever are weighted so as to be a little heavier than the empty waggon, which by this means is brought up again to the level of the rails. The capability of a drop is said to be about the third of that of a spout for the shipment of coal. Its effect as regards breakage would be the same as that of the drops in use on the Tyne.

No. 11.



Comparison of systems with regard to the breakage of coal.

Of all these methods, the evidence tends distinctly to show that the "tips" and "spouts" conduce most to the breakage of the coal. In the system of loading by hoist or by crane the fall from the bottom or end of the waggon into the ship's hold is as great as from the mouth of the shoot or spout; but the coal, until released from the waggon, is literally undisturbed, and the fall from the bottom or end of the waggon, as the case may be, into the spout, together with the concussion of the coal in its passage down the spout, is avoided.

Whatever additional breakage, however, has to be debited to the "tip" and "spout" systems on these accounts happens before the coal reaches the vessel, and is equal in its effect on the whole cargo, and though reducing the value of the coal, is of small importance with regard to spontaneous combustion, in comparison with the final fall of the coal into the ship's hold, an evil which attaches equally to both systems of loading, whether by spout or by crane.

In each system the fall of that portion of the coal which is first put on board, especially in large and therefore deep ships, is very considerable, and the consequent accumulation of small and crushed coal under the hatchways has, we believe, in many cases promoted spontaneous combustion.

Danger of accumulation of small coal.

It was to obviate, as far as possible, the breakage consequent on this fall that the anti-breakage boxes were introduced, and witness after witness advocated building a cone of large coal up to the hatchways, either by boxes or by baskets, in order to break the fall of the coal, and disperse the small coal as it comes down the spout. But even where a cone of average size coal is thus made, rising to the level of the highest hatchway, (which in a two or three decker seems hardly practicable,) still the continuous fall on this heap of the rest of the cargo, sometimes amounting to over 1,000 tons, gradually crushes or pulverises it. Moreover, as each load falls on this central heap, it is the large coal which runs down the sides and is trimmed away to the ends, while the smallest coal, fine slack and dust, clings to the heap where it falls, or gradually accumulates in a compressed mass under the hatchway. It is here that experience shows that spontaneous combustion generally originates; witness the cases of the "Oliver Cromwell," "Calcutta" "Euxine," "Pocahontas," and others.

Speaking of the "Oliver Cromwell," Mr. Price, a large shipowner, said that the coal fell "a very considerable height," and "was very much broken indeed." He continued: "I went into the hold of the 'Oliver Cromwell' when she was about half "loaded, and I noticed in the main hatchway, where the bulk of the cargo was coming "down, a great accumulation of small coal in a pyramidical shape, and I said to the "captain as I came up, 'I am thankful that I am not the owner of this cargo; "'there seems to be so much small in it.'" (Q. 1151). He added that the "Calcutta" and the "Euxine" (which were also burned) were loaded in exactly the same way.

In the case of the "L'Aqouhanna," loaded at Birkenhead, the captain said (Q. 2539), "The tip lifts a truck about 15 or 20 feet above the ship's deck; the coal runs in an "iron shoot, and by the time it gets into the hold it is smashed up, and a large body "of small coal always lies where it falls." Speaking of the cargo of the "L'Aqouhama" he said the coal having to fall a great height there was a great accumulation of small coal round the hatchways (Q. 2561).

Captain Cawkitt, the manager of the Liverpool Salvage Association said, "Most "of the burnings in the ships we have heard of have been under the main hatch, "and the bulk of the burnings have been in vessels that have been loaded under the "tips where the coal is broken, and where there has been a lot of small coal accumu-"lating round the main hatchway, and I believe in most instances the fire has been "proved to have originated about the main hatchway."

The evil of this accumulation of small coal was insisted on by many witnesses.

Mr. Cooper Rundell, accountant to the Liverpool Underwriters Association, stated, quoting from "Stevens on Stowage," that in South Wales it is the practice to dig out a quantity of small coal from the hatchway, after the vessel has completed her loading, and to replace it by larger coal, showing that there is a feeling that danger may arise from too large a surface of small coal.

Mr. Price, to whose evidence we have already referred, when asked whether he had any reason to believe that spontaneous combustion was more likely to occur in an accumulation of fine coal in coal cargoes, not only referred again to his own experience, but said that a shipmaster who had been in ships that had been on fire, and had attended three surveys of ships, that either came into Bombay on fire, or had taken fire while lying there with coal, told him that the fire in each case occurred about 5 feet from the skin of the ships in the region of the main hatchway, where there was an accumulation of fine slack to which he attributed the fires (Q. 1228, 1229); and though he acknowledges that the liability to breakage was to a certain extent got over, when once a cone was formed, he added, "I have always seen it "come up in a pinnacle of small coal to the very top, and as long as there is room "for it to run clear of the hatchways the big lumps of coal run down, and the small "coal generally culminates." (Q. 1290–1292.)

Mr. Hancock, of Bristol, said, "I think the danger of spontaneous combustion "arises from the small coal. I think the fire generally takes place in the hatchway, "underneath the drop, where the small coal is likely to accumulate" (Q. 1342 and 1353).

Mr. Scott, of the firm of Gillespy and Scott, read the following extract from a letter written by the master of the "Pocahontas," lost by combustion in 1874. "Four

"Boston ships have been burnt this season within two months of each other, and "the accounts given are about alike as to the cause of the fire. In most every case "ships have taken fire in the hatchways, where the fine coal is. If ships could be "loaded so as to avoid the fine coals collecting in the hatchways, and some means "taken to prevent the coals breaking up, I do not think fire would occur." (Q. 2093.)

This serious danger, of an accumulation of small coal under the hatchways, which attaches both to the "tip" ("or spout") and "crane," is avoided if the vessel is loaded either by the box system or by barrow.

Accumulation of small coal avoided in loading by box system or by barrow.

The box system was strongly advocated by Mr. John Glasbrook, of Swansea (Q. 2212–2218), and was also commended by Mr. Robert Hancock, shipping manager to Messrs. Charles Hill and Sons, of Bristol and Cardiff (Q. 1331–1335). And speaking of the barrow system, Mr. Cooper Rundell believed in an entire immunity from combustion in vessels so loaded (Q. 116 and 117), whilst he gave some significant statistics as to the increased use of the "tip" at Liverpool, and of the subsequent increase of casualties in shipments from that side of the Mersey (Q. 76).*

Comparing the system of barrows with that of tips, Mr. Price said, "I have loaded "vessels on two or three occasions from Birkenhead by tips before I became alive to "the advantage of the better mode of loading," (Q. 1211) and when asked what he considered the advantage of loading by barrow, said, "Hitherto I have not done it "voluntarily, I have done it because it was a stipulation in my charters that that "mode of loading should be adopted, in future I shall always adopt it, because I "think the cargo is more equally distributed, the small coal does not get into a mass "in the region of the main hatchways, which is the locality where according to my "experience fires usually take place" (Q. 1214). He admitted, however, that the system was a very slow and expensive one, enabling, even with plenty of hands, only 250 tons a day to be put on board, whereas by tips 1,800 tons could be put on board in 48 hours (Q. 1205 & 1210).

Rapidity in loading.

But rapidity in loading is of course an important consideration, not only as a question of time, but also of expense, as shown by Mr. Cooper Rundell (Q. 157 to 163).

Breakage of coals deteriorating to its value as well as conducive to combustion.

That the box and barrow systems are stipulated for in charters, and adopted by many shippers, although the process is in both cases much slower and generally more expensive than that of the tip or the crane, proves that the coal is far less subject to breakage when thus lowered into the ship. It is true that the Peninsular and Oriental Co., and others who use these methods, desire to minimize the breakage of the coal, not so much from any apprehension of spontaneous combustion, as for the purpose of maintaining the value of the coal. But in so doing it is clear from the evidence both of experience and of science that they also diminish the risk of casualty to their cargo from spontaneous combustion, since, apart from any accumulation of small coal, the less broken the coal is generally, the less liable it is to heat.

We are therefore of opinion, with reference to the subject of our inquiry, that, in all methods of loading, the chief aims should be to subject the coal to as little breakage as possible, and to avoid any accumulation of small coal in the cargoes.

Tendency to overrate injurious effect of wet on coal cargoes.

3. *Moisture*.—Most of the witnesses who have come before us have strongly condemned the shipment, or carriage, of coal in a wet condition, and their experience, in this respect, is, to a certain extent, supported by the scientific evidence to which we would direct very careful attention. It would appear that the wetting of certain kinds of coal, more especially those containing pyrites, is active in promoting spontaneous combustion; but the statements of some of the witnesses lead to the impression that moisture has been credited with cases of combustion beyond the sphere of its operation; while, from other passages of the evidence, it appears to us that the influence of the water, existing to a greater or less extent, in all coal before it has been brought to the surface, has not been sufficiently recognised.

4. *Through Ventilation*.—If this had been the only question referred to us, the urgent need for this inquiry would have been completely demonstrated by the result.

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* From the statistics forwarded to the Commission by Mr. Cooper Rundell, which will be found in the Appendix, page 127, it appears that of the shipments from the port of Liverpool with 500 tons of coal and upwards, on voyages across the Equator or through the Suez Canal, in the years 1873, 1874, and 1875, 348 were loaded by tips, and 608 by hand. Among the 348 loaded by tips 21 casualties occurred, while only 4 casualties are recorded among the 608 loaded by hand.

Conflict of evidence as to through ventilation.

The evidence before us of those connected with the shipment of coal, either as shipowners, shippers, or masters, shows a decided want of information as to the elementary causes of spontaneous combustion, and therefore of the real source of danger in through ventilation as explained in the scientific paper attached to this report; hence arises a considerable conflict of opinion.

Real danger of ventilation not understood by its opponents.

Even those who from extensive personal experience, had arrived at the knowledge that ventilation through the body of the coal was, in effect, rather injurious than otherwise, based their opposition to it solely on the grounds that it "fanned the flames" after ignition had taken place, or that the wood, of which the ventilators were generally made, ignited at a lower temperature than the coal, and thus hastened the catastrophe; but they always expressed their conviction that such ventilation could not but tend to prevent heating in the first instance.

Advocates of ventilation.

On the other hand, some thorough advocates of ventilation have given evidence before us.

When their views were based on personal experience, we think that the evidence of these witnesses showed a tendency to credit all cases of immunity from fire to the ventilation, and to attribute all cases of casualty in ventilated ships either to other causes, or to the inadequacy of the ventilation itself, not because they realised the fact that partial ventilation was an element of danger, but because they considered that the amount of ventilation, though beneficial as far as it went, had not been sufficient to keep the temperature low enough.

Spontaneous combustion and explosion confused.

Where ventilation was advocated on less practical grounds than those of experience, we observed a strong tendency (and this remark applies to the bulk of the evidence on this subject) to confuse spontaneous combustion and explosion. Many of the witnesses were under the impression that both results were produced by the same causes. For instance, Mr. William Darley, a gentleman employed by the Board of Trade as assessor in the cases of the "Zeno," "Berwick," "Marie Lee," and "Herbert Graham," on being asked (Q. 2812) "As to green coal, how does that produce *spontaneous combustion*?" replied, "When it is wrought from the pit it is full of gas; if it " is exposed to the atmosphere previously to being shipped, some of that gas is " emitted that would be otherwise retained, but when confined it becomes dangerous, " and ultimately heats."

We believe that this confusion of ideas may have caused many witnesses to treat ventilation as a safeguard against spontaneous combustion, having had reason to regard it as a safeguard against explosion; and a want of knowledge of the conditions productive of explosion, lead them, as in the case of Mr. Darley, to consider that this ventilation, even to guard against explosion, should be carried down through the body of the cargo.

Ventilation generally considered harmless, if not beneficial.

But between the thorough opponents and the consistent advocates of ventilation, the evidence shows a certain proportion of shipowners, masters, and coal shippers, who express themselves as having "no faith in ventilation," but as considering that it " could not do any harm." Many of the witnesses representing this class informed the Commission that they only ventilated their ships to satisfy other interests concerned. Thus, Captain Scurr, of the "Brenhilda," a thoroughly ventilated ship, said (Q. 308 and 309), "I would as soon go to sea with a ship that was not furnished with " ventilators as with a ship that had them."

Captain Kennedy when questioned about ventilation said, "I do not think it does " much good" (Q. 2546); and again, "I doubt the practical worth of ventilation at " all" (Q. 2554), nevertheless he habitually ventilated under advice from his owners " who considered it beneficial."

Again, Mr. Price, a large shipowner, when asked (Q. 1235) "Would the object of " adopting that mode of ventilation be to guard against fire, or to guard against explo- " sion," replied, "It would be rather to satisfy my underwriters than to guard against " anything; for I tell you candidly, that having been free from casualties, I simply " adopted that which the underwriters surveyors, and such like people, recommended " without looking at it critically or carefully at all. The first thing an underwriter " says to you when you are insuring a cargo of coals is, "Will your friends see, or " will you see, to the ventilation? They make a special point of seeing to the " ventilation."

Mr. Thomas Beynon, shipowner and coal merchant at Newport, said, "Personally " I have no faith at all in the ventilation of smelting coal," (Q. 1395); but when was suggested to him that he had subsequently in his evidence recommended a particular system of ventilation, he replied, "No; I stated the system I adopted, and I think in ships which I loaded at Liverpool we were obliged to adopt that plan by the Board of Trade," (Q. 1495.) In this, howerer, he was mistaken, the Board of Trade having laid down no absolute rule on the subject.

Captain Steinson, coal inspector at Newcastle for the Admiralty, India Council, Crown Colonies, and the Peninsular and Oriental Company, stated "that all Liverpool ships must be ventilated." (Q. 1598.) Later he said, "It is a curious thing that " none of the North country ships that I have had to do with, that have not been " ventilated, have been burnt. I was talking to an old captain who told me that he " had never had a casualty happen to a ship except in one case, and that was the " only time he ever ventilated the ship." (Q. 1698.)

Referring to the case of the "Oliver Cromwell," which was burnt, Captain Steinson said, "The 'Oliver Cromwell' was ventilated. The captain came and asked me how " I wanted the coal ventilated, and I said 'I do not want the coal ventilated at all, " 'it is no business of mine;' on that he said, he was obliged to ventilate by the " charterparty. I said, 'Then ventilate according to the charterparty, but as far as " 'my own opinion goes I would not ventilate at all.' He further said, 'The owners " 'say she must be ventilated.' 'Then,' I said, 'Ventilate according to the owners' " instructions.' So he did ventilate according to the owner's instructions with shafts " up and down each hatchway, connected with fore and aft shafts." (Q. 1632.)

Further, Mr. Cooper Rundell, of the Underwriters' Association of Liverpool, gave evidence that "The Underwriters' surveyors at Liverpool have recommended a special " system of ventilation," and that "These surveyors go round the docks and give " information, amongst other things, to the underwriters, respecting the mode of " ventilation adopted." (Q. 91.)

Captain Sandeman, formerly surveyor at Liverpool to the London Salvage Association, and now surveyor under the Marine Department of the Board of Trade, said, with regard to ventilation, "I should say that under all circumstances it is desirable." Q. 2654.) And having explained in detail the various systems employed at Liverpool, he said, "Many shipowners in Liverpool hold that ventilation is of no advantage " whatever, and it was only under pressure that they were disposed to adopt it." (Q. 2647.)

The evidence given by letters on this subject from all parts of the kingdom in answer to the printed circulars issued, of which an analysis is appended, also shows the same prevalence of the adoption of ventilation, often against the convictions of the shipowners and masters concerned.

Injurious effect of recommendations made by courts of inquiry held by Board of Trade.

Such evidence leads us to fear that the statutory inquiries held during the last few years under the Board of Trade, with uniform result in favour of ventilation, have not been so ineffectual as that Department believed, but that, on the contrary, they have proved powerfully influential in a dangerous direction.

The practice of ventilation has greatly increased during these years. Charterers, insurers, and surveyors on behalf of Government, or of underwriters associations, or of shipping companies, are the classes most likely to be influenced by the repeated verdicts of such courts of inquiry; and the evidence goes far to show that many vessels, which if left to their owners or masters would have probably gone to sea unventilated, have, under pressure from these quarters, adopted systems of ventilation that cannot but have added to the risks they incurred.

Evidence against ventilation furnished by the cases of vessels that have suffered combustion.

Turning from the evidence of opinion to the evidence of fact with regard to ventilation, we find that the scientific conclusions expressed by Dr. Percy and Professor Abel receive confirmation.

Of the 70 ships that suffered in 1874, none are reported as not having been ventilated, while 38 are distinctly stated to have been ventilated, 35 of them being vessels over 500 tons. But the most startling illustration is furnished by the cases of four vessels "the Euxine," "Oliver Cromwell," "Calcutta," and "Corah." These ships, to which reference was made by several of the witnesses who appeared before us, were loaded under the tips at Newcastle, at the same time, with the same coal, and from the same seam, sometimes one ship being under the tip and sometimes another. They were each carrying from 1,500 to 2,000 tons of coal. The "Euxine," "Oliver Cromwell," and "Calcutta" were bound for Aden, the "Corah" for Bombay. The first three ships were thoroughly ventilated, the fourth was not ventilated at all. The "Euxine," "Oliver Cromwell," and "Calcutta" were all totally lost from spontaneous combustion. The "Corah" carried her cargo safely to Bombay.

Since this Commission has been sitting further, evidence in this direction has been supplied by the vessels "Theresa" and "Anglia." The "Theresa" (704 tons) sailed from Birkenhead for Rangoon with 920 tons of coal in September 1875, and became a total loss from spontaneous combustion when she had been at sea a little over two months. She was very extensively ventilated, having square venetianed deal ventilators in each hatch to the keelson, and a trunk-way on each side of the keelson, and three rose mushroom ventilators in each hatch. The following is an extract from a letter from her owner, Mr. Henry French, of Liverpool:—"When I heard of the loss " of the 'Theresa' I had another vessel (the 'Anglia,' 525 tons reg., classed A.E. 1 " Eng. Lloyd's) beginning to load 630 tons of the same kind of coals for Havanah. " The captain decided he would have no ventilation, and went without any. She " sailed 29 Jan. /76, and was 69 days on the passage, being 28 days before getting " clear of this land. He reported his coals very hot when discharging, but no signs of " fire, and the top of the coals seemed to be coated with a kind of scum like sulphur. " During the voyage his hatches were kept off when practicable. I mention the " above on account of the coals being from same pit, and being just the same time on " board as the 'Theresa's' cargo. One ship was ventilated, the other not."

We have also personally inspected the methods of ventilation adopted at the various coal-shipping ports, and that inspection has further convinced us how feeble such systems are for any purpose, except for the introduction into the coal of one of the elements of spontaneous combustion.

Systems of ventilation inspected by Commissioners.

The most ordinary method of ventilation consists of shafts, about 2 ft. square, nearly always constructed of wood, and in the majority of cases venetianed, which pass down through the body of the coal from the hatchways to the bottom of the vessel, where they lead into a box shaft running along the keelson or into a space below the platform. Sometimes these shafts are carried up through the hatchways and fitted with cowls, so as to form an up-cast and a down-cast for the current of air passing through them, but they are often only brought to the level of the deck, and are therefore closed whenever the hatches are on.

Some valuable papers, forwarded with great courtesy to the Commission by Mr. Henry S. Poole, inspector of mines for Nova Scotia, on the subject of spontaneous combustion in the coals of that district, contain some striking and conclusive evidence on the injurious effect of ventilation as applied to coal on shore. We feel much indebted to Mr. Poole, both for the expression of his own views on this subject, and also for the trouble he has taken in collecting for us the experience of those whose papers he has inclosed. (See Appendix.)

Papers from Nova Scotia.

We are therefore fully of the opinion of Dr. Percy and Professor Abel that, "the " evidence which has been submitted to the Commission on the subject of ignition " of coal cargoes, bears out generally the conclusion that it is inadvisable to attempt " *through ventilation* of cargoes in coal-laden ships."

Through ventilation condemned by scientific evidence.

5. *Provision for testing Temperature of Coal Cargoes.*—The papers forwarded from Nova Scotia by Mr. Poole, the Inspector of Mines, to which allusion has already been made, advocate the use of thermometers, to be lowered, by means of metal tubes, passing into the body of the coal, in order to test and register its temperature; and by this means some interesting statistics, which he incloses, in connection with the heating of coal in store, were obtained. Mr. Wright, the engineer-in-chief to the Admiralty, described to us, in his evidence, a similar plan adopted by the Government in their coal stores at Jamaica, and in the coal bunkers of Her Majesty's ships.

The use of thermometers recommended for testing temperature of cargo.

Mr. Robert Hancock, shipping manager to Messrs. Charles Hill and Sons, of Bristol and Cardiff, in his evidence on the case of spontaneous combustion in "Her Royal Highness," said, "We always instruct the master to ascertain the heat of the hold with the thermometer" (*Q.* 1326.) A registration of the temperature by means of thermometers, was advocated by other witnesses, among whom was Captain Steinson, who stated that he was very careful, when commanding a coal-laden ship, to observe the temperature every day, but that it was not customary to do so among the masters of coal ships.

Papers from Nova Scotia Admiralty and other evidence.

6. *Explosions.*—We have not given any statistics on the subject of explosions,* because any return of explosions at sea would give a very inadequate idea of the extent of this evil, many, if not the majority, of these cases occurring in dock, or in harbour, sometimes even before the loading of the cargo is completed.

No reliable statistics as to explosions.

Mr. Beynon, in answer to the question,—

"Do you know any instance of explosion having occurred on a coal ship?" said,

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* Particulars however of certain cases of explosion, forwarded by the Board of Trade and by the Admiralty, will be found in Appendix II. and III., pages 4 and 60.

"I have known several in Cardiff, but I have not inquired into the particulars. The "mate of one ship, after the ship had been loaded, took a lighted candle into the "hold and an explosion occurred. I do not know the particulars. The ship had "been loaded four or six hours." (Q. 1491.)

And Mr. Charles Tully, a shipowner in Newcastle, when asked as to his experience of explosion from gas, replied :—

"I have known two or three instances on the Tyne, where after the gas coals had "been loaded and the hatches had been put down, immediately the vessel blew up "altogether, that is to say, within two or three hours afterwards. In those cases "the vessel had not begun her voyage, but the hatches had been put down, and the "coals being full of gas they caused an explosion." (Q. 444.)

The danger of explosion is much greater in some kinds of coal than in others, and there is more risk and injury to life from these catastrophes than from spontaneous combustion, because they occur too suddenly to admit of provision being made for personal safety.

But, on the other hand, it is much easier to provide against the occurrence of explosion than of combustion.

We have, under another head, shown how prevalent is a confusion of ideas between these two causes of casualty, and it cannot be too clearly understood that they originate from entirely distinct causes inherent in the coal.

It should also be borne in mind that the gas, which by its mixture with oxygen is productive of an explosive atmosphere, is a light gas, which will readily find its way to the surface of the coal.

Surface ventilation recommended to prevent explosion.

All it requires therefore, is to have free and constant egress from the surface of the coal into the open air, and the majority of the casualties seem to occur from making the gas dependent for this egress upon the hatchways, which, in the event of rain or heavy weather, are liable to be covered and battened down. The gas then accumulates between the surface of the coal and the deck, and, mixing with the air with which it is confined, forms an atmosphere which only needs the application of a flame to explode.

Every coal-laden ship should therefore be fitted with shafts or ventilators piercing the upper deck, but not carried down on to or through the coal, with cowls always trimmed so as to form a downcast and upcast for the current of air, which would then pass continuously, and in all weather, over the surface of the coal, carrying with it any explosive gas as fast as it is evolved.

Objections to carbonic acid gas as a means of extinguishing fire.

Steam or water recommended.

7. *Means for extinguishing Fire.*—Several methods for generating carbonic acid gas and applying it to the ignited portions of a coal cargo, have been proposed for our consideration. We consider, however, that although this gas might be useful, by excluding atmospheric air (which is essential to support combustion), yet it will not, as water does, exert any very sensible cooling effect; which is a point of vital importance in the case of a considerable mass of ignited coal. We are of opinion that water and steam are the only agents practically available for the purpose of extinguishing fire in coal cargoes.

Summary.

Having completed our inquiry into the various questions embraced by the terms of the reference to us, we beg to lay before Your Majesty a summary of the conclusions at which we have arrived :—

1. That certain descriptions of coal are intrinsically dangerous for shipment on long voyages.
2. That the breakage of coal in its transport from the pit to the ship's hold, the shipment of pyritic coal in a wet condition, and, *especially*, *ventilation through the body of coal cargoes*, conduce to spontaneous combustion, even though the coal may not be unfit for conveyance on long voyages.
3. That spontaneous combustion in coal cargoes would be less frequent, if regard were had by shipowners and underwriters to these facts.
4. That when coal is being carried on long voyages the temperature in the various portions of the cargo should be tested periodically by thermometer, and registered in the log.

5. That, with a view to guard against explosion, free and continuous egress to the open air, independently of the hatchways, should be provided for the explosive gases, by means of a system of *surface* ventilation, which would be effective in all circumstances of weather.

6. That in order to make known the descriptions of coal liable to combustion, the Inspectors of Mines should be instructed to hold inquiry into all cases of spontaneous combustion occurring in cargoes of coal taken from their respective districts; exporters being required always to record on their specifications the denomination of the coals forming the cargo.

7. That no additional legislation with reference to the conveyance of coal by sea is required, unless for the purpose of giving effect to our proposals with regard to the inquiries by Inspectors of Mines, and to the fuller specification of coal entered outward at Her Majesty's Customs.

(Signed) HUGH C. E. CHILDERS.
GEORGE ELLIOT.
HENRY HUSSEY VIVIAN.
F. A. ABEL.
RICHARD CORY.
GEORGE DUNCAN.
JOHN FENWICK.
JOHN FERGUSON.
CHAS. ICELY.
JOHN PERCY.
WM. YOUNG.

J. M. CARMICHAEL,
Secretary.
18th July 1876.

Paper by Frederick Augustus Abel, Esq., F.R.S., and John Percy, Esq., M.D., F.R.S.

The so-called spontaneous development of heat which occasionally takes place in coal, is due to chemical changes which certain substances occurring in it undergo through the agency of atmospheric oxygen, and which are liable to acceleration by some conditions attending the storage or transport of coal.

The substances, the oxidation of which is attended by the development of heat, are iron pyrites (and possibly some other combinations of sulphur), and some carbo-hydrogen compounds, forming part of the coal itself, which are comparatively readily oxidisable.

Iron pyrites is of almost universal occurrence in coal. The condition in which it exists differs considerably in different kinds of coal; in some it is distributed so as not to be detectable by ordinary inspection, in others it is in form of more or less conspicuous laminæ of a brass yellow colour; occasionally small distinct crystals of cubical form and bright metallic lustre are observed, and not unfrequently it is found in nodules (known as "brasses") and layers of considerable dimensions.

Iron pyrites, a compound of sulphur and iron known as disulphide of iron, occurs in the mineral kingdom in two crystalline forms, which are identical in chemical composition. Pyrites vary greatly as regards their stability or their liability to undergo oxidation by contact with the atmosphere. Some, after remaining unchanged during long periods of exposure, will undergo oxidation without any immediately apparent inciting cause. Others are perfectly stable, and with others oxidation follows with more or less rapidity upon their exposure to the atmosphere.

The presence of moisture in air promotes the oxidation of pyrites; it does so apparently by bringing the atmospheric oxygen into more intimate contact with the surfaces of the oxidisable material. Similarly, the absorption of moisture by mineral substances of laminated or porous structure, through which pyrites are disseminated, promotes the oxidation of the latter by bringing the atmospheric oxygen, which is dissolved by the water, into more intimate contact with the oxidisable material.

Thus, the oxidation of pyrites in alum schist is accelerated, in the manufacture of alum therefrom, by occasionally watering loose heaps of the mineral which are piled in such a manner that the access of air to the interior can be regulated.

The oxidation of pyrites (as in most instances of chemical action) is accompanied by the development of heat, which may accumulate to such an extent as to lead to the ignition of highly oxidisable constituents of minerals through which it is distributed. Heaps of alum schist exposed to the air and moistened will become heated to an extent to lead to the ignition of the carbonaceous constituents of that mineral, and similarly coal, through which pyrites are disseminated, may become heated to the point of ignition by the oxidation of pyrites disseminated through it.

Sulphur exists in coal in other forms of combination than that of iron pyrites. Thus sulphate of lime is occasionally disseminated through coal; the existence of sulphur in this form cannot give rise to the development of heat, as it is already in the completely oxidised or burnt condition; however, so far as is known they have not been found to possess any liability to spontaneous heating by oxidation. Some descriptions of coal or shale have been found to contain sulphur in somewhat considerable quantity in combination with carbon and hydrogen. An example of this is furnished by the so-called "resiniferous shale" occurring in Tasmania, which has been found to contain as much as five per cent. of sulphur in combination with carbon and hydrogen.

So far as is at present known, the spontaneous heating of coal is not ascribable to any tendency of such sulphur combinations to oxidation by atmospheric agency.

Pyrites appear therefore to be the only sulphur combinations occurring in coal which by their oxidation, promoted by the presence of moisture and by mechanical conditions favourable to the accumulation of the heat developed by such oxidation, are liable to give rise to the so-called *spontaneous ignition* of coal.

Coal varies considerably not only in chemical composition, but in structure, some varieties being comparatively open and porous, others compact or laminated, and very friable or readily broken up.

Carbon in a finely divided or porous condition has the property of absorbing and condensing within its pores large volumes of certain gases, among which is oxygen.

Thus, wood charcoal which has been recently produced, or which has been freshly heated sufficiently to expel moisture and gases which it may have absorbed, will condense within its pores more than nine times its own volume of oxygen, and seven times its own volume of nitrogen, which it is capable of absorbing by exposure to the air.

The condensation of a gas by a porous body is attended by the development of heat, proportionate to the extent of that condensation. Moreover, the tendency to oxidation which carbon, and certain carbon-compounds existing in such a substance as charcoal, possess, is favoured by the condensation of oxygen within its pores, whereby the very intimate contact between the carbon and oxygen particles is promoted. Hence the development of heat and the establishment of oxidation occur simultaneously; the latter is accelerated as heat accumulates, and chemical action is thus promoted and may in course of time proceed so energetically that the carbon or carbo-hydrogen particles may be heated to igniting point.

A good illustration of this action is afforded by the very porous charcoal prepared for the manufacture of gunpowder. It has to be protected against rapid access of air for some considerable time after it has been removed from the carbonising retorts (by transferring it at once from the retorts to metal receptacles which are kept tightly closed for a number of days), otherwise the charcoal, though it may have been allowed to cool down perfectly, will speedily become heated upon exposure to air, by the action above explained, to such an extent as to ignite spontaneously. Even after the stick charcoal no longer exhibits any tendency to heating when exposed, its pulverisation by grinding has still to be delayed; otherwise, when the absorption and condensation of oxygen by the internal portions of the pieces of charcoal become favoured by the reduction of these to a fine state of division and the consequent exposure of large surfaces to the air, ignition of the charcoal will take place. (Instances of such ignition upon the mill beds at powder works are not unfrequent.)

The spontaneous ignition of oiled cotton or silk waste, or cloths, which is of not unfrequent occurrence, affords another illustration of the manner in which the heat developed by the action of atmospheric oxygen upon readily oxidisable substances, consisting chiefly of carbon and hydrogen, which are exposed to that action under conditions very favourable to it, will readily accumulate to an extent sufficient to give rise to ignition. The porous cotton or silk waste serve, in such cases of spontaneous ignition, as vehicles for exposing the readily oxidisable oil or grease in a finely divided condition to the air, and thus favouring its rapid oxidation.

The foregoing explanation of the manner in which the spontaneous heating and eventual ignition of certain carbonaceous substances by the action of atmospheric oxygen are brought about, has a direct bearing upon the spontaneous ignition of coal. The more porous and readily oxidisable portions of coal, which are known to be more or less largely disseminated through seams from different localities, undergo oxidation by absorption of atmospheric oxygen, and by the exposure of large surfaces to its action, and the heat developed by that action will accumulate under favourable conditions to such an extent as soon to hasten the oxidation and the consequent elevation of temperature, until some of the most finely divided and readily inflammable portions actually become ignited.

The breaking up of the coal which occurs for a more or less considerable extent before and during its shipment obviously favours the absorption of oxygen, and consequently increases a tendency to heating by the action above described.

In a series of valuable experiments on the weathering of coal, by Richters and others, it has been demonstrated that a considerable quantity of oxygen is absorbed by freshly gotten coal, and that carbonic acid and water are evolved from such coal after absorption of oxygen, while a portion of the oxygen absorbed is fixed by the formation of some solid oxygenated compound in the coal. Spontaneous oxidation of coal by the oxygen absorbed from the air is therefore satisfactorily established and is unquestionably one, if not the chief, cause of the spontaneous heating of coal.

Spontaneous ignition of coal, when due to the oxidation of the porous and readily oxidisable carbonaceous substances occurring in coal does not appear to be favoured by the presence of water in the coal or by its access to a cargo; on the contrary, these portions, by becoming wet, would have their pores more or less filled with water, and their power of absorbing oxygen would be proportionately diminished; hence the presence of water must be antagonistic to the oxidising action of the latter in many instances, though, when iron pyrites is present, it may promote or accelerate spontaneous heating as already pointed out.

The gases which are occluded (or confined in a more or less condensed condition) in coal, vary considerably in quantity and composition in different kinds of coal, and they also gradually undergo various changes in composition by exposure or keeping of the coal.

Their chief inflammable constituent is marsh gas, and it is to the admixture of this gas with a *considerable* volume of air that *explosions* are due in freights or stores. In pits where explosions are liable to occur, the gas escapes either with more or less rapidity and force from fissures ("blowers"), or gradually from the freshly exposed surfaces of coal seams. When coal from such pits is brought to the surface it continues slowly to evolve inflammable gas for some time afterwards, especially if the coal be in large masses or stored in compact heaps.

If facility is not afforded for the ready escape or removal into the open air of the inflammable gas emitted from the coal composing a cargo or contained in bunkers on board ship, the spaces between the masses of coal, or any partially confined spaces not occupied by the coal but in close proximity to or communication with it, will in time become filled with a mixture of the gas with the air in those spaces, which, unless the former be present in very small proportions, would explode on the approach of a flame to it, and with a violence depending upon the proportion which the air bears to the inflammable gas which has become diffused through it.

As the application of flame (or of a body raised to a high red heat) is indispensable to the ignition of mixtures of air with the inflammable gas evolved from coal, it is obvious that *explosions* cannot occur spontaneously from this cause on board ship, but must be brought about by the accidental or incautious approach of a light to localities where the coal is stored, or where the explosive mixture is likely to penetrate.

If coal, from seams which are charged with marsh gas, is placed on board ship shortly after being raised from the pit, there is obviously great liability to the formation of an explosive atmosphere in the hold or bunkers, or spaces communicating with them, and every possible means should in such cases be had recourse to for facilitating the escape of gas from the coal into the open air.

But, as the gas requires a large admixture of air to render it violently explosive, it is obvious that any attempt to ventilate the coal by passing or drawing air into the *body* of the freight would be most likely to favour the production of a violently explosive mixture of gas and air. The only useful application which might be made of any special means of ventilation with a view to diminish the risk of *explosions* would be to pass a current of air *over* the coal and immediately into the open air, so as to accelerate the escape and removal of the inflammable gas.

As regards the application of ventilation with a view to reduce the liability to spontaneous *ignition* of a cargo of coal, the only useful object which could *possibly* be aimed at by ventilation would be the rapid abstraction of heat developed in the coal (by chemical changes set up as described) by causing cool air to circulate freely and rapidly throughout the *body* of a mass of coal. The attainment of such a result, even by powerful means of artificial ventilation, elaborately applied, and with the coal in the mechanical condition most favourable to its free permeation by air, appears, to say the least, very doubtful, and there can be no question that any system of ventilation practically applicable on board ship would fail to attain such a result, even disregarding the fact that the mechanical (finely divided) condition of much of the coal constituting a cargo is quite antagonistic to the free passage of air through its mass.

It does not appear practicable, therefore, to apply ventilation with any prospect of guarding against the accumulation of heat in some portion of a cargo of coal.

Such circulation of air as may be established even in the less compact portion of the cargo is not likely to have any valuable cooling effect, and the circulation, if there be any, must be very feeble among the more closely lying masses of small coal, so that heat, if developed in these, will accumulate undisturbed. Indeed, its development would possibly be favoured by the fresh supply of oxygen which a gradual replacement of the air surrounding those parts would convey, so that a period would be reached sooner or later when the development of heat would be most seriously promoted by ventilation, some time before actual ignition demanded the total exclusion of fresh air.

The evidence which has been submitted to the Committee on the subject of the ignition of coal cargoes bears out generally the conclusion to which the above considerations lead, namely, that it is inadvisable to attempt *through ventilation* of cargoes in coal-laden ships.

(Signed) F. A. ABEL.
JOHN PERCY.

APPENDIX TO REPORT

OF THE

ROYAL COMMISSIONERS

APPOINTED TO INQUIRE INTO THE

SPONTANEOUS COMBUSTION OF COAL IN SHIPS.

APPENDIX I.

Correspondence between Board of Trade, Lloyd's, and London Salvage Association.

Board of Trade to Salvage Association and Lloyd's.

Board of Trade, Whitehall Gardens,
April 22nd, 1874.

Sir,

I am directed by the Board of Trade to transmit to you for the information of your Association the accompanying copy of a statement of inquiries, held under the Merchant Shipping Act, into casualties caused by explosion or fire to coal-laden vessels in 1873.

The result of these inquiries is always the same, viz., the officers are exonerated, the court recommends better ventilation; and as the Board of Trade have no reason to believe that the recommendations of the courts have any effect in inducing owners to adopt a better system of ventilation, or render underwriters unwilling to insure vessels of this description, they have decided not to order inquiries in such cases in future.

I am, &c.
(Signed) Thomas Gray.

J. A. W. Harper, Esq.,
Salvage Association.

Inquiries held in the year 1873 into Casualties occurring to Vessels through Combustion of Cargo (Coals), with Suggestions of Courts.

"Zeno," steamship.	Lost in the Bay of Biscay, on the 13th May, through explosion of coal gas.	Inquiry held at Hull 11th June; master acquitted; Court suggested the adoption of stringent measures for proper ventilation of coal-laden ships.
"Berwick," steamship.	Explosion of coal gas on the 9th of May, off the Dudgeon.	Inquiry held at Greenwich on the 26th September; master exonerated; means recommended for ventilating holds of coal-laden vessels.
"Herbert Graham," barque.	Destroyed by fire off Magellan Straits. Spontaneous combustion of cargo of coals.	Inquiry held at Newport (Mon.); master exonerated; means of ventilation suggested.
"Minnie Graham," barque.	Destroyed by fire in the South Pacific, through combustion of cargo of coals.	Inquiry held at Newport; master exonerated; means of ventilation suggested.

A similar letter was addressed by the Board of Trade to the Secretary of Lloyds'.

Salvage Association to Board of Trade.

16, Cornhill,
April 23rd, 1874.

Sir,

I beg to acknowledge the receipt of your letter of the 22nd instant, in which you desire me to inform the Committee that the result of inquiries into casualties caused by explosion or fire in coal-laden vessels held by the Board of Trade is invariably the same, viz., that the officers of the ships are exonerated, that the Court recommends better ventilation; and as the Board of Trade have no reason to believe that the recommendations of the Courts have any effect in inducing owners to adopt a better system of ventilation, or render underwriters unwilling to insure vessels of this description, the Board of Trade have decided not to order inquiries in such cases in future.

The Committee will, no doubt, order me to circulate notice of the resolution of the Board of Trade and the reasons of it, as widely as possible. But, before any steps are taken with that object, will you allow me to ask you if the matter will not admit of a representation of the views of the Board of Trade, something as follow:

(I.) That, as the Courts appointed by the Board of Trade and their assessors acquit the officers of the ships of all blame for explosions, with which the officers have nothing to do, but abstain from finding that the vessels wherein such explosions occur through the absence of proper ventilation are unseaworthy, the Board of Trade have come to a resolution to hold no more such inquiries.

(II.) That they are confirmed in this resolution not to hold inquiries because the owners of these ships are determined that their ships shall continue to go to sea in the unseaworthy condition, which the Board of Trade inquiries apparently ought to have found them to be in.

(III.) Because underwriters, who, at the time when risks are taken on these ships are altogether without means of ascertaining whether or not the ships are unseaworthy in the particular matter of due ventilation, continue to take risks.

I am, &c.
(Signed) J. A. W. Harper,
Secretary.

The Assistant Secretary,
Marine Department,
Board of Trade.

Board of Trade to Lloyd's.

Board of Trade, Whitehall Gardens,
September 29th, 1874.

Sir,

I am directed by the Board of Trade to request you to be so good as to move the Committee of Lloyd's to cause a table to be prepared for the Board of Trade of such coal-laden vessels as have been reported to them as lost or damaged through the spontaneous combustion of their cargo during the twelve months ending 30th June last.

I am to request that as far as possible the table should state the name, port, and tonnage of each vessel, her voyage, and whether steam or sailing, the date, place, and result of explosion, the number of lives lost, and whether the cargo was ventilated or not.

I am, &c.
(Signed) HENRY G. CALCRAFT.

The Secretary, Lloyd's.

LLOYD'S to BOARD OF TRADE.

Lloyd's, E.C., October 14th, 1874.

SIR,

I AM directed by the Committee of Lloyd's in reply to your letter of the 29th September to forward to you the enclosed two copies of the return of casualties to coal-laden vessels by fire, prepared by the Committee so far as it lies within their power to ascertain the losses or damages requested in your communication.*

I am, &c.
(Signed) HENRY M. HOZIER,
Secretary.

The Assistant Secretary,
Marine Department, Board of Trade.

LLOYD'S to BOARD OF TRADE.

Lloyd's, November 5th, 1874.

SIR,

WITH reference to my letter of the 14th October enclosing a return of coal-laden vessels reported in Lloyd's List as lost or damaged through fire during the twelve months ending 30th June 1874, I beg to enclose you a supplementary return showing the coal-laden vessels reported in Lloyd's List as lost or damaged through fire from 1st July to 31st October 1874.

I am, &c.
(Signed) HENRY M. HOZIER,
Secretary.

The Assistant Secretary,
Marine Department, Board of Trade, S.W.

P.S.—I also enclose a list of coal-laden vessels posted as missing from 1st July to 31st October 1874.

LLOYD'S to BOARD OF TRADE.

Lloyd's, November 7th, 1874.

SIR,

I AM directed by the Committee of Lloyd's to inform you that the Committee have learned with regret that the Board of Trade has abandoned holding inquiries in the case of fire to coal-laden ships. By the return of coal-laden ships to which casualties are reported to have occurred according to Lloyd's List that I sent to you in my letter of yesterday's date, you will perceive that of late there appear to have been a great number of such casualties, and I am directed to draw your attention to this fact with the hope that you may be able to move the President of the Board of Trade to allow some inquiry to be made into the circumstances connected with the casualties by fire to coal-laden ships.

I am, &c.
(Signed) HENRY M. HOZIER,
Secretary.

The Assistant Secretary,
Marine Department, Board of Trade.

BOARD of TRADE to LLOYD'S.

Board of Trade, Whitehall Gardens,
December 1st, 1874.

SIR,

WITH reference to your letter of the 7th instant, calling attention to the necessity for making inquiry into the circumstances connected with the casualties by fire to coal-laden ships, I am directed by the Board of Trade to state that in the year 1873 official inquiries were held into the cases of the steamers "Zeno" and "Berwick," and the barques "Herbert Graham" and "Minnie Graham" which had been lost through combustion of the cargo of coals.

In each of these cases the master was acquitted, and the Court suggested that means should be adopted for the proper ventilation of coal-laden vessels.

Looking therefore to the uniform result of the inquiries in these cases, the Board of Trade, before resuming them, would be glad to be informed what is the particular advantage which the Committee think is to be derived from them.

I am, &c.
(Signed) THOMAS GRAY.

The Secretary, Lloyd's.

LLOYD'S to BOARD of TRADE.

Lloyd's,
December 3rd, 1874.

SIR,

I AM directed by the Committee of Lloyd's to acknowledge the receipt of your letter of the 1st December, and in reply to inform you that several causes of the frequent occurrence of fires of late in coal-laden ships have been suggested, such as the excess of ventilation, the short time that is allowed between the extraction of the coal from the pit, and its stowage on board ship, the increasing inferiority of coal, and consequent more frequent prevalence of iron pyrites, and that all these and other causes have been supposed to lead to the great risk of loss of life which is involved in the combustion or abandonment of coal-laden vessels. Under these circumstances it appears to the Committee that the object of the inquiry should be to illustrate facts, and to publish them for the information of the general public, with a view to the prevention of the great risk of loss of life which now appears to be involved in the coal trade, and which, according to the tables which I lately forwarded for your perusal, appears to be seriously on the increase.

I am, &c.
(Signed) HENRY M. HOZIER,
Secretary.

The Assistant-Secretary,
Marine Department, Board of Trade, S.W.

SALVAGE ASSOCIATION to BOARD of TRADE.

16, Cornhill, E.C.
December 21st, 1874.

SIR,

ON Tuesday last I laid before the Committee of this Association a letter addressed to the Committee by Sir John Pirie & Co., extensive coal shippers on the subject of coal combustion in ships.

The Committee consider that letter and the whole subject so important that they have desired me to transmit the letter to you, and to express their hope that the suggestions of Sir John Pirie & Co. will receive the very careful attention of Her Majesty's Government.

The Committee are sensible that the subject is one of singular difficulty and extreme obscurity; but a commission of inquiry, composed of scientific and practical men, might possibly discover the causes of active combustion of coal in ships, and so arrive at some practical remedy.

I am, &c.
(Signed) J. A. W. HARPER,
Secretary.

The Assistant Secretary,
Marine Department, Board of Trade.

London,
December 11th, 1874.

GENTLEMEN,

BEING largely interested in the trade, we beg to call your attention to the unusual number of instances lately reported (as per list herewith) of spontaneous combustion of coal laden on board vessels on their way to distant ports.

It is of the greatest importance to all concerned that—

1. The cause of such ignitions shall be investigated.
2. The best means be ascertained for preventing them, when possible, by ventilation or otherwise.
3. Steps be taken to discover the best mode of extinguishing fire when it does occur on board ship.

We take the liberty, therefore, of proposing that the Board of Trade be requested by you to undertake such an investigation, appointing qualified scientific experts to carry out a thorough inquiry.

The present plan of using water or scuttling ships (when practicable) appears to us a most wanton and destructive mode of proceeding, and surely some apparatus may be applied so as to extinguish fire by carbonic acid gas, where the hatches can be battened down.

We are, &c.
(Signed) JNO. PIRIE & Co.

The Salvage Association,
16, Cornhill, E.C.

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* The Returns enclosed in this and the subsequent letter will be found in an extended form, Appendix V., page 101.

Board of Trade to Secretary, Lloyd's.

Board of Trade, Whitehall Gardens,
Sir, December 23rd, 1874.

I am directed by the Board of Trade to acknowledge the receipt of your letter of the 3rd instant, in which you state that various causes have been suggested to account for the frequent fires on board coal-laden vessels, and that the Committee of Lloyd's think that the object of inquiries to be in future held by order of the Board of Trade should be to illustrate facts, and to publish them for the information of the general public, with a view to the prevention of the great risk of loss of life which now appears to be involved in the coal trade.

In reply I am to request that you will state to the Committee of Lloyd's that the Board of Trade have made many inquiries into cases of fire and explosion on board coal-laden vessels, and these inquiries have shown that the various causes enumerated in your letter are fruitful of great risk of loss of life on board coal-laden vessels. Copies of the reports of the three last inquiries are enclosed.

What remains to be done is to take warning by the facts proved at these inquiries, and to take such steps as may render coal-laden ships safe by better ventilation, by providing means for stifling flames, and by caution as to the description of coals shipped and their condition when shipped, and this involves primarily the interests and responsibility of owners and underwriters.

Some serious steps should be taken by those who own, manage, and insure these ships and their cargoes, and what the Board can properly do to assist such endeavours shall not be left undone.

They, however, are unable to see any use in holding inquiries which seem to lead to no practical result, and they rather wait to see that their report should receive some consideration and attention than propose to themselves a fruitless continuance of disregarded information.

The Board of Trade, however, will not fail to insure full investigation of any special cases brought to their notice likely to bring out any further facts.

I am to add that the Board of Trade learn that the Board of Underwriters in Boston have had their attention directed to the subject, and intend to frame rules showing under what conditions they are prepared for the future to undertake risks on coal-laden vessels.

I am, &c.
(Signed) Thomas Gray.

The Secretary, Lloyd's.

Board of Trade to Salvage Association.

Board of Trade, Whitehall Gardens,
Sir, December 31st, 1874.

I am directed by the Board of Trade to acknowledge the receipt of your letter of the 21st instant and its inclosure from Sir John Pirie & Co., suggesting, that a commission of inquiry should be instituted into the causes of spontaneous combustion on board coal-laden ships.

With reference thereto I am to inclose a copy of a recent letter (23rd December 1874—M. 16285) from this Board to the Committee for managing the affairs of Lloyd's on the same subject.

The Board of Trade will be glad to hear what action has been taken by owners and underwriters in consequence of the numerous reports after the official inquiries already instituted by this Board, what steps, if any, have been taken by shipowners, shippers, and underwriters themselves to ascertain the causes of explosions on board coal-laden ships, or to ensure the proper stowing and ventilating of coal cargoes on board ship, and, further, whether there is any guarantee that if the Board of Trade takes any steps in the direction indicated in your letter either by re-opening inquiries in the case of casualties to coal-laden ships, or otherwise, the result will receive attention or consideration on the part of underwriters, shippers, and shipowners.

I am, &c.
(Signed) Thomas Gray.

J. A. W. Harper, Esq.

Salvage Association to Board of Trade.

Coal-laden Ships.

16, Cornhill, E.C.
Sir, January 1st, 1875.

I am desired by the Committee of this Association to acknowledge with their thanks receipt of your letter of December 31st, 1874, enclosing a copy of a letter from you to the Committee of Lloyd's dated 23rd December 1874, M. 16285.

In reply to your letter to this Committee, the Committee consider that there is in their letter on the subject and in the accompanying letter from Messrs. John Pirie & Co., extensive coal shippers, some guarantee that if the Board of Trade should think proper to take any steps in the direction indicated in a letter of the Committee, that is, in the direction of a combined, scientific and practical, inquiry, the result of that inquiry will receive attention and consideration on the part of underwriters, shippers and shipowners.

With respect to your letter to Lloyd's Committee, they desire me to observe that the Committee were not aware that any inquiries, that is inquiries of the nature suggested by this Committee, viz., by scientific and practical men combined directed especially to the subject of the real causes of combustion of coals in ships, and possibly the means of avoiding them, had ever, up to the date of their letter, been held.

The Committee need not point out to you that an ordinary inquiry, conducted perhaps by a justice of the peace and two assessors, whose principal, if not exclusive function, and principal if not exclusive qualification for the office, are their knowledge of ships and navigation, is not an inquiry calculated to elucidate a subject so difficult and so obscure in itself, and rendered further obscure by the great conflict of opinion.

The Committee presume that these are not the inquiries which you allude to as having led to no practical result.

The Committee are afraid that the facts which you mention as having been proved at such inquiries are generally considered as only discussed, and the real facts remain too uncertain for it to be possible that the Committee should hope they will constitute the warning you desire.

If an investigation were held such as the Committee venture to suggest, one by men of high scientific attainments, and especially scientific acquaintance with coal in its nature and in all its applications of whom the name of more than one will immediately suggest itself to you, combined with persons practically acquainted with coals in the pit and in the ship's hold, the result might be to fix with a certainty, absolute or qualified, what the causes of this dangerous combustion of coals in ships are, and then this great advantage would be gained that the responsibilities would be fixed upon the proper person, and the clear knowledge of those responsibilities would be likely to operate to cure the mischief.

With respect to the projected combination of underwriters in Boston, to model their conditions of insurance to the dangers of combustion, the Committee beg to observe that underwriters are neither coal owners nor shipowners, and are very likely to be wholly unversed in subjects such as that of the combustion of coals, and that it is not improbable that the underwriters in Boston may be found to jump at conclusions which a more thorough knowledge of the subject might discover to be entirely a mistake. And on the whole subject of the intervention of underwriters, the Committee beg to point out that such a remedy is very much too circuitous, indirect, and uncertain to be worth considering on such a critical question as the combustion of coal in ships; and further that a combination of the comparatively few underwriting companies existing in Boston is probably an easy thing. In the underwriting community of London and especially Liverpool, a very difficult, if not an utterly impracticable one.

And as to merchants and shipowners providing a remedy the Committee beg to point out again that at present the facts are not ascertained with sufficient certainty and with sufficient authority to justify any expectation of an effectual remedy in those quarters.

I am, &c.
(Signed) J. A. W. Harper,
Secretary.

The Assistant Secretary,
Marine Department,
Board of Trade.

Board of Trade to Secretary Lloyd's.

Casualties.

Board of Trade, Whitehall Gardens,
Sir, January 14th, 1875.

With reference to previous correspondence upon the subject of inquiries into cases of casualty by fire to coal-laden vessels, I am directed by the Board of Trade to state that the Salvage Association, who have also communicated with this Board upon the subject, have suggested that inquiries should be held into such cases by scientific and practical men combined, and directed especially to the subject of the real causes of combustion of coals in shipping, and possibly the means of avoiding them.

This suggests the appointment of a Scientific Commission, and the Board of Trade would be glad to learn whether this arrangement would meet the views of your Committee.

I am, &c.
(Signed) HENRY G. CALCRAFT.
The Secretary, Lloyd's.

LLOYD'S to BOARD OF TRADE.

SIR, Lloyd's, January 20th, 1875.

I AM directed by the Committee of Lloyd's to acknowledge the receipt of your letter of the 14th January, and to inform you that the Committee consider that an inquiry should be held into cases of casualty by fire to coal-laden vessels by scientific and practical men combined.

I am, &c.
(Signed) HENRY M. HOZIER,
Secretary.

APPENDIX II.

STATUTORY INQUIRIES held by BOARD OF TRADE, and Reports of other Cases received from that Department.

REPORTS of STATUTORY INQUIRIES held under the BOARD OF TRADE into cases of SPONTANEOUS COMBUSTION in COAL-LADEN VESSELS, from 1868 to 1874, inclusive.

"CATTOFIELD" Barque, of Aberdeen, of 450 tons register.

Port Elizabeth,
17th September 1868.

Inquiry into the circumstances connected wtth the loss of the barque "Cattofield," destroyed by fire near the island of Tristan d'Acunha, and abandoned on the 25th August 1868.

Appeared WILLIAM ROBERTSON, who, being duly sworn, deposes:—

I was chief officer of the barque "Cattofield," bound from Leith to Penang, laden with coals. We sailed from Leith on the 22nd June last. Everything went on right until the 24th August, when just after dinner we observed smoke coming out of the after cabin, which I reported to the captain, who said he could hardly believe such, and that I must go and examine. I did so, and found the smoke ascending through the after hatch. I took the hatch off, and found the coals very heated. I reported such to the captain, who examined it himself. The captain held a consultation as to what was best to be done, and all were of opinion there was no immediate danger, and so we secured all the hatches and bore away for the nearest land, which was Tristan d'Acunha. By 4 or 5 o'clock that same night the smoke increased so much as to prevent the men going down the forecastle. We made preparations to leave the ship in the event of the fire breaking out before morning. Early the next morning an explosion took place, blowing off the fore hatches, previous to which the decks had become so hot as to melt the pitch. About two hours afterwards another explosion took place, blowing off the lazarette hatches. In the cabin the smoke now became so dense that we were unable to go below. The vessel was still sailing towards the island, but no land visible from the masthead. We then put some clothing in the longboat and some bread and water, ready to leave at a moment's notice. We did not try to extinguish the fire by pouring water down, because the smoke was issuing so dense that we could not reach the hatches. At 10 a.m. that morning, the 25th, we sighted a vessel and made signals of distress and steered towards her, and she bore down to us and lowered a boat and sent us assistance. We put some more clothing in that boat, lowered our own, and some of the crew left the ship. The captain, the carpenter, myself, and one or two others remained on board our vessel; but the heat and smoke became so bad that we were compelled to abandon the vessel, and just as we were leaving another explosion took place, blowing off the main hatches, the captain being the last man to leave the ship. It was the "Glenlyon," bound to Bombay, that rendered us assistance. The carpenter scuttled the vessel before leaving her. We all went on board the "Glenlyon" and sailed her course, and on the 10th we were put on board the barque "Aletheia" off Cape St. Francis, and landed in Algoa Bay on the 13th instant. The vessel was still visible when we abandoned her, but dense smoke issuing from all parts. Our hatches were properly battened down fore and aft. I considered the fire had gained too much hold of the cargo when we first discovered it to enable us to extinguish it by pouring water on it. The cargo seemed to be generally on fire. The ship was not supplied with a fire engine. I was not present when the cargo was shipped. The ship had not strained at all. The object of scuttling the ship was to prevent collisions with other vessels. She was scuttled from the outside.

Cross-examined.

I first discovered the smoke issuing from the after cabin, and the fore hatch blew off first. I thought the whole cargo was on fire and so general that we did not know where to commence throwing water down. No one could have worked in the hold, from the dense heat, smoke, and sulphur that arose. We battened the hatches down with a view of smothering the fire by preventing access of the air. I should think we were 250 miles from land. I have never heard of coal fires being extinguished. We found it impossible to remain any longer on board. We transferred some of the clothing we had placed in our longboat to that of the "Glenlyon."

WILLIAM ROBERTSON,
Chief Officer.

Before us, the 18th September 1868,
JOHN CAMPBELL, R.M.,
F. S. READ, Assessor.

HENRY JOHN BASTARD, duly sworn, deposes:

I was second officer on board the barque "Cattofield," and remember the chief officer reporting smoke was issuing from the after cabin. I went with him to examine it, when we found smoke coming up the main and after hatches. We took the main hatch off, examined the coals, which we found very hot, and smoke coming up the ventilators through the main hatchway. We thereupon battened the hatches down fore and aft, and steered for the island Tristan d'Acunha. About 8 o'clock that same night the crew came aft and reported the smoke to be so dense that they could not stop in the forecastle. We got the longboat ready for launching and put some water and provisions in it in case the fire should break out and be compelled to abandon the vessel. An explosion took place the following morning, blowing off the fore hatches. The decks had become very hot. Some time after another explosion took place, blowing off the lazarette hatches in the cabin. The smoke increased, but no flames issued. We sighted a sail, which bore down to us and sent us assistance. The smoke and heat had increased so much that we could remain no longer on board, and we all left the ship and went on board the other vessel, being the "Glenlyon." Before leaving our vessel the carpenter scuttled her. An explosion blew off the main hatch just as we were leaving her. On first discovering the fire we had no hopes of extinguishing it. I saw the cargo shipped. The coals were perfectly dry, the hatches were properly battened down, and the ship was tight. From what I could judge the fire seemed to be general.

HENRY JOHN BASTARD.

Before us, this 18th September 1868,
JOHN CAMPBELL, R.M.,
F. S. READ, Assessor.

JOHN MASSON, duly sworn, deposes:

I was carpenter of the barque "Cattofield," and remembered when the vessel was first discovered on fire, on the 24th August. I examined the coals, and found them very heated; smoke was issuing from them. We battened down the hatches. The smoke increased to such an extent that the men could not remain in the forecastle. The following morning, between 5 and 6 o'clock, an explosion took place, blowing off the fore hatch. Some time after another took place, blowing off the lazarette hatches. The smoke and heat had so much increased that we could not go below. During the day we sighted a vessel, and sent us assistance on board. The smoke had become so dense and the heat so great that we could no longer remain on board, and we abandoned the vessel and went on board the other, which proved to be the "Glenlyon." Just as we were leaving the ship another explosion took place, blowing off the main hatch. After we got into the boat I scuttled the vessel by cutting a hole with an adze at midships, just above the copper line. We then sailed in the "Glenlyon." I think it was then about 3 o'clock in the afternoon. I saw the vessel about 5 o'clock. There was smoke issuing from all parts, but no flames were visible. The ship made no water; she was quite tight. I did not see the cargo shipped.

JOHN MASSON,
Carpenter.

Before us, this 18th September 1868,
JOHN CAMPBELL, R.M.,
F. S. READ, Assessor.

Statement of SAMUEL E. JANE, late master of the barque "Cattofield," of 450 tons, burnt at sea.

On the 24th June 1868 I sailed in command of the barque "Cattofield," of Aberdeen, from Leith, laden with a cargo of coals at that port, and bound for Penang. On the following day, 25th June, anchored in Aberdeen Bay to land a sick man. At 9 p.m. of same day got under weigh and proceeded on the voyage.

Nothing of import occurred until the 24th August, when the ship had reached the latitude of 34° 24′ S., longitude 15° 40′ W.

About 1 p.m. on that day smoke was observed issuing from the after cabins. I immediately examined the hold, and found smoke ascending up through the coals and ventilators, and the heat very great a little below the surface. The coals had evidently ignited. I then had all the hatches closed down to prevent flame from bursting out, and bore away for the islands of Tristan d'Acunha, distant about 250 miles in S.S.E. direction.

At 9 p.m. the smoke became so dense it was impossible to remain below, the fire increasing rapidly and smoke issuing from every aperture. Fearing it would be impossible to reach the islands, at midnight got the boats ready for launching, placing a little provisions and water in each, in order to leave the ship at any moment, should the flames break out.

At daylight on the 25th, nothing was visible from the mastheads; we continued running towards the islands, the wind blowing strong from the N.E. with a rough sea. About this time an explosion took place, blowing off the forward hatch.

About 9 a.m. a sail was descried ahead from the main topgallant yard. I immediately steered for her with distress signals flying, and about noon we were near enough to exchange signals. She proved to be the ship "Glenlyon," of Quebec, from Liverpool, bound to Bombay, commanded by Captain McIntyre, who on learning our condition at once hove to, and sent a boat alongside to render us any assistance needed.

The wind freshening and the sea rising, no time was to be lost in leaving the burning ship, it being now utterly impossible to get below.

We then launched the boats, and putting what clothing could be got into them, abandoned the ill-fated "Cattofield" about 3 p.m. on the 25th August last, scuttling her before doing so.

Previously to the last boat leaving the ship another explosion took place, blowing off the main hatches. The smoke rushed up in clouds. I immediately jumped into the boat and shoved off. We were very kindly received by Captain McIntyre, of the "Glenlyon," and every attention was paid to our comfort while on board his ship. When the "Cattofield" was last seen, smoke was pouring in volumes from every hatchway and skylights, and she could not possibly last for many hours.

And I must here express my gratitude to Captain McIntyre, who, under Providence, was the means of rescuing us from the horrors of a burning ship.

We remained on board the "Glenlyon" until the 10th September, when a part of the crew were transferred to the "Aletheia," bound for this port; the weather becoming bad, the remainder of the crew remained on board the "Glenlyon" until the following morning, when they were all transferred to the "Aletheia," and were safely landed here on Monday the 14th instant, all in good health, viz., myself and 15 men.

I cannot let the opportunity pass without thanking Captain Daniels, of the "Aletheia," on behalf of myself and crew, for the promptness with which he consented to receive us on board his ship (already full with passengers), and the trouble taken by him and uniform kindness evinced towards us while on board.

S. E. JANE,
Port Elizabeth, late Master of the "Cattofield."
17th September 1868.

Judgment of the Court.

The Court is of opinion that the abandonment of the barque "Cattofield" was not caused by the wrongful act or default of the captain, but the cause of her loss was spontaneous ignition of her cargo of coals.

JOHN CAMPBELL, Resident Magistrate
F. S. READ, Assessor.

"PERSIA,"* of Glasgow, a wooden ship of 1,289 tons register, four years old, classed A 1 for seven years.

The Mauritius Marine Board met on Friday, 27th November 1868, for the purpose of inquiring into the loss by fire of the ship "Persia," of the burthen of 1,289 tons, James Clark, master, which took place in lat. 40 South, long. 21 East, or thereabouts, on the 2nd instant.

Members of the Board present:

D. WALES, Harbour Master and President.
A. W. BARCLAY, Surveyor and Merchant.
J. COWIN, Surveyor.

Appeared before the Board for examination, and were duly sworn:

James Clark, master; Peter Hutchinson, mate; Hugh Conville, 2nd mate; together with several of the crew of the late ship "Persia."

The master and the two mates were examined on oath; their evidence was clear and precise. It was not considered necessary to call in any of the crew.

It appeared from the evidence, and also from the log book, that the "Persia" sailed from Greenock on the 31st August 1868, laden with coals and bound to Bombay; she was tight and staunch when she left, and continued so to the time of her loss. The voyage was prosecuted successfully till the 1st of November, when gas and smoke were observed rising from the coals about the midship part of the vessel, and on examination it was evident from the heated temperature that combustion was going on. This was met and combated by every available means; water was freely poured over the part that appeared most heated, the deck above being scuttled for that purpose; the force pump was worked by a portion of the crew and directed to the same spot. The water in the well soon increased to 3 feet, and the vessel's pumps were set on and the water as it rose conducted on the burning coal. Coals were dug out and thrown overboard in the hope of getting at the real seat of the fire, and a party of men remained at that work till driven on deck by the rapidly increasing volume of gas and smoke. The "Black Watch" hove in sight at this critical time; it was now evening; she laid by the "Persia" during the night, and on the morning of the 2nd November, the captain, officers, and crew abandoned the ill-fated vessel, from every hatchway of which columns of dark, black smoke, with occasional flashes of flame, were issuing. Mr. Kerruish, the master of the "Black Watch," fully confirmed this part of the statement.

There was in fact so little time to spare that hardly any of the effects of the crew were brought away, and the master and mates lost everything, certificates included. There is no doubt in the opinion of the Board that every possible exertion was made to save vessel and cargo.

In reference to this loss the board would remark that in their opinion the burning of coal-laden vessels demands the serious investigation of scientific men. These fearful accidents are increasing in frequency, and spontaneous combustion is hardly a satisfactory conclusion to arrive at. Why is there spontaneous combustion? Why does one ship burn from that cause, and dozens of others laden from the same pit, at nearly the same time, arrive safely at their destination? Is ventilation of the coal advisable or dangerous? Ought hatches to be kept on or off? Could not inflammable gases be pumped out of a ship's hold? Is there not a method of creating gases in an inexpensive manner on board ship that would smother and extinguish any amount of fire in a vessel's hold? Such measures are spoken of commonly enough but are apparently never carried into practice. Three large coal-laden vessels, the "Pernix," "Volante," and the "Persia" have been burnt at sea, and their crews arrived in this colony during the last year. What destruction of property? What risk to life? What privation and suffering do these burnings entail? At this moment there is a valuable vessel in this port, coal-laden, which narrowly escaped the same fate as the "Persia," the coals having been smouldering many days before they could be extinguished.

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* For the correspondence, arising out of this case, between the Board of Trade and the Local Marine Boards in the United Kingdom, *see* page 25.

The Board submit these remarks for consideration, and hope that the very important subject they refer to may be thought worthy the attention of competent persons. Certainly any one who will point out the way in which coals may be conveyed to distant ports without greater risk than the generality of cargoes will have deserved well of owners, sailors, underwriters, and last not least, of humanity.

D. Wales,
Harbour Master, President.
A. W. Barclay,
Marine Surveyor.
John Cowin,
Marine Surveyor.

"L'AGOUHANNA," of Glasgow, a Quebec-built ship of 1,103 tons register.

Bombay Fort Police Court,
Tuesday, 16th February, 1869.

Pursuant to the Government notification, Marine Department, dated the 10th instant, appointing John Connon, Esquire, Acting Chief Magistrate of Police, and Lieutenant G. O'Brien Carew, late Indian Navy, to conduct an investigation into the circumstances connected with the loss by fire of the ship "L'Agouhanna" on Tuesday, the 2nd instant, about 30 miles west of Hurnee, and 100 miles south-west of Bombay, the Court assemble this day at half-past 2 o'clock p.m. for the purpose stated.

Mr. Hearn, solicitor to Government, appears to watch the case on behalf of Government.

William Kennedy sworn; examined by Mr. Hearn.

I was captain of the ship "L'Agouhanna." I hold a master's certificate from the Board of Trade, No. 28,697, dated 14 July 1863.

She was Quebec-built and 1,103 tons burthen. We were bound from Liverpool for Bombay with a cargo of coal. The coal was shipped in Birkenhead Docks. It was called West Hartley coal or North Wales coal. It came by railway as far as I could tell direct from the pits. We came out of the docks on the 30th of September last and started direct on the voyage. Everything went right up to the 4th of February. On the 2nd of this month my second officer who has been acting for the chief officer, Mr. McKee, came and reported to me, about 7 o'clock in the evening, that there was a smell of something like naphtha or coal tar, and that the paint was a little stained just by the forehatch. I went to investigate this, but I could find no greater heat than I had noticed during the rest of the voyage. There was a little steam, but nothing more than I noticed three days after we left Liverpool. I sent the second officer down below the same evening. He reported that there was no more heat below than there was on the forehatch, next morning a little after 4 o'clock, when I went on deck, the boatswain came and said he thought there was fire in the ship. I sent for the carpenter and sent him below. He came up in about ten minutes and reported the ship was on fire, and that the smoke was coming up strong by the stanchions, and said there was every appearance of fire amongst the coal. I immediately ordered the force pump to be got out and applied water to every available place in the hold. We continued throwing water with buckets as well as the pumps till 8 p.m. The smoke was at that time so strong that the men could not stop below for more than a few minutes at a time. The smoke was coming up through all the hatches and through the ventilators besides; of these we had one upright down each hatch. There was an upright ventilator connected with others that went fore and aft both above and below the beam. At this time we were in 30 fathoms water and about 30 miles from the land, as far as I can judge south-west of Hurnee. The wind was about north-north-west. The ship was under sail, making the best we could for the land, and so we continued until 4 o'clock next morning, the weather being at that time very fine. At 4 o'clock on Wednesday morning, 3rd February, I called the hands aft and told them my opinion about the ship being on fire, and ordered the boats to be got ready and provisioned in order to be prepared for the worst. That was all done by daylight; water was still being thrown below by the hose. The men had been obliged to leave the forecastle at 3 o'clock. In the course of the night I tacked the ship to the westward fearing to get too near the land; at 3 o'clock I tacked again to the eastward to get nearer the land. About 7 or 8 o'clock, as near as I can remember, the smoke became so strong we could not stop on deck and we had to take to the boats and about a quarter of an hour after that we saw the flames coming up through the after hatch, the cabin skylight, and the stern windows. About an hour after this, when we were pulling for the land, the main and mizzen masts fell, and between 11 and 12 the foremast fell. I reached land with my boat about 10 at night. One or two of the other boats had reached from 4 to 7. We made out the glare of the burning ship until about 12, when it disappeared.

All the crew came safe to land. I saved my chronometer, part of my clothing, and sextant. I lost all my charts and nautical books with the rest of my library.

This is the ship's third voyage to Bombay with coal. I never experienced an alarm of fire before. I noticed no particular difference between the coal on this occasion; in fact there was less steam on this occasion from the coal than there was from the coal on the first voyage.

I can only attribute the fire to some inflammable quality in the coal, and I think the fire first took place in the bottom of the ship. According to my experience coal is always shipped or stated to be shipped direct from the pits, and according to my experience coal ships now-a-days catch fire more frequently that they formerly used to do.

By the Court:

We were about nine days in loading, no fresh water got into the cargo then, nor afterwards. It was showery when we were loading, but we stopped loading during the showers. It was about eight and twenty hours when we took to the boats after the fire was discovered. I always thought we should be able to drown out the fire till we were compelled to take to the boats. We could not get the ship nearer the land because there was no wind from midnight of Tuesday until the time we left the ship with the boats. I cannot say whether the coal on this occasion was from the same pit as the coal we carried on previous occasions.

In this ship or any other ship I know of laden with coals, if a fire were to break out in the bottom of the hold, there are no means of immediately finding it out.

William Kennedy.

Taken on oath before us,
John Connon,
Acting Chief Magistrate of Police,
G. O'B. Carew,
Lieutenant, late Indian Navy.

Robert McKee sworn; examined by Mr. Hearn.

I was second officer of the ship "L'Agouhanna." I have been doing duty as chief officer for the last two months and half, during the illness of the chief officer. I hold a certificate from the Board of Trade as second officer. I did not the see coal taken on board. I only joined the ship on the 28th September. There was no appearance of fire until the 2nd February, on which day, at 6 o'clock in the evening, I called the captain's attention to a smell. I did not think it was fire; I thought it might be a little extra steam. The captain at once sent the carpenter below and I went down myself. I discovered no extra heat. I first discovered the ship was on fire at 4 o'clock on Tuesday morning, and we continued pumping and throwing water into the hold all that day and night till about half-past 3 o'clock on Wednesday morning. I thought all Tuesday that we should get the fire under. On Tuesday night, between 7 and 8, the smoke became so suffocating that we could not stay below. I went down the main hatch about every ten minutes and shifted the hose on to the hottest place.

The hands were pumping and carrying water in buckets all through the night. They were driven from the forecastle about half-past 3, and we then found it was a hopeless case. The boats were then all got ready, watered and provisioned, and dropped astern, and we were obliged to take to them about 8 o'clock. The wind was about north-north-west on Tuesday night. It was fresh during part of the night. It fell light about 12 o'clock, I reached the shore about half past 6 o'clock on Wednesday night. I could see the glare of the fire at that time. The ship's hands could not have worked harder, and nothing more have been done than was done to put out the fire.

Robert McKee.

Taken on oath before us,
John Connon,
Acting Chief Magistrate of Police,
G. O'B. Carew,
Lieutenant, late Indian Navy.

Adjourned.

Bombay Fort Police Court,
Friday, 19th February 1869.

Mr. Hearn appears to watch the case on behalf of Government.

Alexander Smith sworn; examined by Mr. Hearn.

I am the carpenter of the "L'Agouhanna." This is my third voyage in her. I saw her take in coal at Liverpool. I was by her all the time. It was in the month of September. It was not much wet. There were summer showers. The coals were quite dry when they were shipped. They came by railway. The coal was in good order. In my opinion no water, either salt or fresh, could have got to the coals after they were shipped, from the precautions I took. The ship is very tight; she did not make an inch of water in 24 hours. The first thing that came to my notice on Monday, the 2nd February, was that the paint at the fore hatch was discoloured, but I thought there was nothing unusual in this, and did not think it was necessary to to mention it to the captain. Next morning at 5 a.m., the captain sent me below to see if there was any heat among the coals. I went below, half way between the fore and main hatch by the stanchions. I saw the smoke coming up by the stanchions. I moved a quantity of coal, and noticed the smoke coming up much stronger, and there was a smell like coal-tar or naphtha. This was about the centre of the ship. I tried the same experiment towards the sides and forward, but there was no heat whatever. I went on deck again and told the captain the coal was on fire. He got a force-pump to work at once among the coals. It was a good and powerful pump. The pumping went on from Tuesday morning until we left the ship on Wednesday. The fire got worse, and the men could not stop below. They could only go down for a minute to shift the hose. The fire spread aft first, about 6 o'clock on Tuesday evening. About 5 o'clock on Wednesday morning we found it was a hopeless case. The men worked as hard as they could, and everyone did his best. The captain then gave orders to get the boats ready, and to have them provisioned. About 8 or 9 o'clock we took to the boats. The fire was actually blazing before we left the ship. The weather was fine on Tuesday, with a light breeze, and the ship was going slowly through the water. At midnight it fell calm. I cannot form any opinion as to how long the coal had been on fire. It might have been smouldering for a long time. I was never in a coal ship before, when the coal took fire. I cannot give any opinion as to the origin of the fire.

By the Court:

We had no naphtha or coal-tar on board; we had no cargo except coal.

Alexander Smith.

Taken on oath before us,
John Connon,
Acting Chief Magistrate of Police,
G. O'B. Carew,
Lieutenant, late Indian Navy.

Isaac Newton sworn; examined by Mr. Hearn.

I was boatswain on board the "L'Agouhanna." I saw all the coal shipped at Birkenhead. It was dry weather all the time we were taking in the coal. I noticed nothing unusual about the coal until Monday morning, the 2nd February. I thought there was more smell or dry heat. I had noticed a dry heat for several days, and not so much steam as on former occasions. On Tuesday morning about 4 o'clock I told the captain I thought there was more smoke coming up than there was yesterday. Upon this the captain told me to take the watch below and examine the coal, which I did as far as I could. I found the coals very hot, and a kind of vapoury smoke coming through. I went up and reported this, and the captain ordered the hose to be got out, and we kept playing water on the coals all day and all night. The fire continued to get worse and stronger, notwithstanding that everyone was doing his uttermost to get the fire under. On Wednesday morning the ship was too hot to hold us any longer, and we took to the boats. I reached the shore about sundown. I saw the main and mizzen masts fall soon after we left. I know of nothing inflammable on board except the coal.

Isaac Newton.

Taken on oath before us,
John Connon,
Acting Chief Magistrate of Police,
G. O'B. Carew,
Lieutenant, late Indian Navy.

Decision of the Court.

Bombay Fort Police Court,
Wednesday, 3rd March 1869.

Pursuant to the Government notification, Marine Department, dated the 10th February last, appointing John Connon, Esquire, Acting Chief Magistrate of Police, and Lieutenant G. O'Brien Carew, late Indian Navy, to conduct an investigation into the circumstances connected with the loss by fire of the ship "L'Agouhanna" on Wednesday the 3rd idem, about 30 miles west of Hurnee, and 100 miles south-west of Bombay, the court assembled on the days mentioned in the proceedings for the purpose stated.

From the evidence taken before us with regard to the destruction of the "L'Agouhanna," from Birkenhead, when approaching the port of Bombay, by fire, on the 3rd February last, we have arrived at the conclusion that spontaneous combustion in a cargo of coals was the cause of this accident.

It appears also from the evidence that the weather was showery, while the coals were being taken on board in the latter end of September last, or when they were conveyed from the pits, and this may perhaps have been the primary cause of the coals subsequently taking fire.

The captain in his evidence says all went well till the 1st February; on the next day, however, there was indication of fire among the coals, and the second mate was sent below to examine the state of the cargo. The next morning (3rd February), the boatswain expressed an opinion that there was fire in the ship's hold, and on the carpenter being sent for, he (after 10 minutes examination) reported there was fire among the coals, and at 8 p.m. of the same day the smoke was so strong that the men could not remain below more than a few minutes at a time.

On that day the captain called the crew aft and informed them that it was necessary to be prepared for the worst, and gave orders for the boats to be lowered and provisioned. On the following morning all hope of saving the ship being at an end, the captain and crew left in the ship's boats in good order and discipline, reaching the shore at Hurnee in the course of the same day. The ship seems to have gone down very soon after being thus abandoned.

We are of opinion under these circumstances that the ship should at once have been headed for the shore (at that time 30 miles off), where she might have been bilged without causing her total destruction; water would by that means have been admitted in at the seat of the fire, and with assistance obtained from the shore the bulk of the cargo as well as the ship might have been saved, as the captain believes (and we think correctly) that the seat of the fire was in the lowest portion of the cargo.

We are therefore of opinion that the captain acted injudiciously in standing to the westward, even if he did so for a short time, only with a view to the chance of meeting passing vessels and thus obtaining assistance.

The ship's log shows that fresh and moderate breezes prevailed, and there is no mention of calms during the time alluded to, the wind being north-north-west, and the port of Hurnee bearing about north-east, so that on a taut bowline and flood tide that port could have been made and the distance accomplished in eight hours, supposing the ship not to be making more than four knots an hour.

When the fire had got the upper hand and the captain saw that it was beyond the control of water applied, he should, we think, have adopted a plan universally followed by the chimney sweeps in England, by which (in case of a chimney being on fire) they exclude all air and smother the fire out by its own smoke.

It moreover appears from the evidence that there was an elaborate system of ventilation through the cargo. This ventilation, we are of opinion, should have been closed when it was discovered that the fire could not be controlled, and the excess of smoke mentioned by the captain and others might have smothered the flame, or at least given them ample time to have reached the shore.

In making these remarks with regard to what appear to us to be omissions on the part of the captain of the "L'Agouhanna," we fully recognise that he acted promptly and to the best of his judgment under the circumstances, and that every exertion was most creditably made on the part of both officers and crew, to carry out his instructions; nevertheless we conceive that he was wanting in resource in failing to adopt the precautions afore mentioned, although we are satisfied that according to his own conviction he made every effort to save the ship.

In connexion with this inquiry we would respectfully notice the frequent destruction by fire of coal-laden ships. A great number of such vessels have been destroyed by the coals taking fire within the last year or two.

During the last twelve months five vessels (the names of which are given in the margin) laden with coals have been discovered on fire in the port of Bombay, as reported by the master attendant of this port, and a number of others have been burnt on their outward voyage to India. It is, we would respectfully submit, a matter of great importance that means to detect any tendency to fire on board these ships, and of promptly meeting it when it exists, should be adopted with the least possible delay.

"Bell Isle,"
one year ago.
"H. E. Richardson,"
"Royal Albert,"
"Glenlyon,"
"Hannah Law,"
within the last six months.

In view to the adoption of such measures we offer for the consideration of Government and others interested the following suggestion:—

In the case of all ships laden with coal a cylinder of iron, three inches in diameter perforated its whole length and reaching down to the bottom of the cargo, should be placed in the centre of each hatchway, a rod of wood to fit easy being introduced to prevent the coal dust filling it through the holes while loading; when the loading is completed the wooden rod should be replaced with an inch iron one of the same length; at short intervals of time these should be carefully examined; to render such examination easy an eye should be formed at the head of the iron rod, to which can be attached a whip from aloft, and the rods drawn and examined in a few minutes; even while in the act of withdrawing the hand can examine them, and having done so to within a foot of the end they can be again lowered.

Should the rod be considered not sufficiently sensitive to detect through the cylinder a slight increase of heat, a thermometer lowered by a line could be used, and it should also be used occasionally to ascertain the mean heat; any great increase would give the necessary warning.

In the event of fire the hose of the pumps can be introduced into the cylinders and water thrown to the bottom of the cargo, and disseminated by means of the holes to all parts of it.

With these means there would be little or no danger of fire getting the upper hand, as the only question on the occurrence of fire would be a sufficient supply of water, and with good force pumps that could be easily obtained.

We do not consider that the captain deserves any punishment, beyond the slight censure implied in the foregoing finding; and the officers and crew of the ship seem to have behaved well throughout.

John Connon,
Acting Chief Magistrate of Police.
G. O'B. Carew,
Lieutenant, late Indian Navy.

Confirmed,
W. R. S. FitzGerald,
Bombay Fort Police Court,
3rd March 1869.

"COMMODORE PERRY," of Newcastle, wooden vessel of 1,978 tons register, 14 years old, classed at American Lloyds A1½ for 3 years in January 1869.

Bombay Fort Police Court,
Monday, 6th September 1869.

Pursuant to the Government notification, Marine Department, dated the 1st instant, appointing John Connon, Esq., Senior Magistrate of Police, and Lieutenant G. O'Brien Carew, Indian Navy, to conduct an investigation into the circumstances connected with the loss by fire of the ship "Commodore Perry" in the Bombay Harbour on the night of the 28th August last, the court assemble this day at half-past 2 o'clock, for the purpose stated.

Mr. Hearn, solicitor to Government, appears to watch the case on behalf of Government.

Thomas Black sworn; examined by Mr. Hearn.

I was the master of the ship "Commodore Perry." I have received a copy of the report on which this investigation is ordered. The "Commodore Perry" was ship-rigged, and a wooden ship 1,978 tons register; she belonged to Messrs. Thompson and Harper, of Newcastle-on-Tyne. I am not certain, but I think she was 14 years old; she was classed American Lloyds A1⅓ for three years in January last. I was on a voyage from Newcastle to Point de Galle for orders, with a cargo of coals. We had 2,560 tons of coal on board. It was Davidson's West Hartley coal. I saw part of the coal put on board, but not the whole. Part was shipped by wagons and about 250 tons out of lighters, from about 2nd February to 23rd March last. This is the second voyage I have taken her with coals. The coals were loaded dry. The ship had three decks. The lower hold was nearly full, the first deck two thirds, and the next deck a little over one third. The coal was stacked. Between the coal and the decks there was a current of air above and on the sides of the coal. Besides this the coal was ventilated by the fore and after hatches. A ventilator about four feet square ran from the poop down to the lower hold. I left Newcastle on the 8th March and arrived at Galle on the 7th July last; but having to put out again on account of the current, I did not get back again till 23rd of the same month, and left the following day for Bombay. On the 25th August a report was made to me that there was a large quantity of steam in the fore hatchway. I could not make out what it was at first, but on Friday, the 27th, I made out that the ship was on fire. There was a great deal of smoke in the latter part of the afternoon of that day. We made Kennery Light at dusk on Saturday evening the 28th. The pilot came on board about 9 p.m., and shortly afterwards there was an explosion which blew the fore hatches up, killing or suffocating the steward. The ship was immediately anchored. There was no flame visible at this time. The smoke was nearly all coming out of the fore hatch. The cabin was filled with smoke. I got the boats out as speedily as possible, being afraid that the ship might blow up in the main hatchway. After I got the boats out I went on shore for assistance. I first went on board Her Majesty's overland transport "Euphrates." I gave orders to the chief officer what should be done during my absence. I got back to the ship at about a quarter-past 5. She was then on Karanja shoal, she was a little below her loaded mark; tide ebbing at the time. She was burning very much forward at that time. Two water boats at that time were playing on her. They continued playing on her for the most part of that day. I went on shore about 6 o'clock to procure lighters to save the ship's stores. I went back towards the ship at half-past 5 in the afternoon, and found every one had left the ship. I returned to the shore in the Anchor bay. I gave up all idea of getting the fire under. The ship continued to burn from that time till she was consumed. I have been back to her three times. The second time I saw she was burnt down to the level of midships and below the loading mark at either end. I attribute the fire to spontaneous combustion of the coal. I can form no opinion why this cargo of coal caught fire while the other did not. I should have thought the other cargo of coal was more likely to take fire, as I am generally suspicious of Welsh coals. I have had a great deal of experience of coals, and as regards this cargo it was shipped under unusually favourable circumstances. It was fine weather, and the coal was shipped very dry. I produce the certificate I hold from the Board of Trade as Master, dated 4th March 1852, and No. 3,632.

By the Court.

We had rain several times on the voyage. The hatches were ordered to be put on whenever rain came. The ship did not make much water during the voyage; she was usually tight; she made more water just before we reached Bombay in two hours than she had previously done in 12 hours. I attribute this to the burning of the ship. I think the fire must have been in the lower 'tween decks, between the main and fore hatch. I left my ship for the purpose of obtaining assistance. I considered the ship was safe, as she was then at anchor, and therefore left myself in order to obtain assistance as speedily as possible. I have not heard of ventilator tubes being put through the cargo at different times in order to ascertain the heat. I never heard of that system prevailing in the Royal Navy. Some people think ventilators are of no use. I cannot give any opinion on this point myself, not having tried ventilators. I do not think the ship was fully insured. I know nothing about the cargo.

Thomas Black.

Taken on oath before us,
John Connon,
Senior Magistrate of Police.
G. O'B. Carew,
Lieutenant, late Indian Navy.

William Wright Sinclair sworn; examined by Mr. Hearn.

I was chief officer of the "Commodore Perry." I hold a master's certificate from the Board of Trade, and also a master's certificate from the Bombay Government; the Bombay certificate is dated June 1860, and the English certificate, 14th February 1862. The first time we saw any steam coming up was on the 25th August. On that day, as usual, I was up and down the hold two or three times. I did not notice any particular difference in the coals on that day; on the next day they were a little warm towards midships, and I fancy the fire must have broken out rather forward of the main hatch. Smoke was first visible on the 26th. I could not see any particular spot where it was coming from. The coal was quite dry. I saw the best part of the coals laden. They were very dry. The only opinion I can form as to the cause of this fire is that it must have originated from spontaneous combustion.

W. W. Sinclair.

John Connon,
Senior Chief Magistrate of Police.
G. O'B. Carew,
Lieutenant, late Indian Navy.

The other witnesses summoned in this inquiry are asked if they have anything to say to the court with regard to the destruction of the "Commodore Perry" by fire, and they reply that they are unable to say how the fire occurred.

The court adjourn until Monday next, the 13th instant, at half-past 2 p.m., to consider their decision.

John Connon,
Senior Magistrate of Police.
G. O'B. Carew,
late Indian Navy.

Decision of the Court.

Bombay Fort Police Court,
Monday, 13th September 1869.

Pursuant to the Government notification, dated the 1st instant, appointing us to conduct an investigation into the circumstances connected with the loss by fire of the British ship "Commodore Perry" in the Bombay Harbour, on the night of the 28th August last, we beg to report that the court met for this purpose on Monday the 6th instant.

From the evidence before us it appears the—

"Commodore Perry" was a wooden ship, and was 1,978 tons register. She was 14 years old; but the captain, Mr. Black, states she was classed in January last American Lloyd A1½ for three years. She left Newcastle in March last for Point de Galle, and was loaded with 2,560 tons of West Hartley coal.

Nothing particular appears to have occurred during the voyage. The ship arrived at Galle on the 7th July last, but had to put out again an account of the current, and did not get back till the 23rd of the same month. She left on the following day for Bombay. On the 25th a great quantity of steam was observed in the fore-hatchway. The captain states he could not at first make out what it was, but the next day (Friday) he found the ship was on fire. The ship kept on her course, and Kennery Light was made out at dusk on the evening of Saturday the 28th. She was boarded by the pilot at 9 o'clock that night, and shortly afterwards there was an explosion, by which the steward met his death. The ship was immediately anchored, and the master left her in order to obtain assistance from the shore. When he returned next morning about a quarter-past 5 o'clock, he found the ship on Karanja shoal. All efforts to extinguish the fire were unsuccessful, and the ship continued to burn till she was consumed.

We are of opinion, after careful consideration of the evidence brought before us, that spontaneous combustion among the coals was the cause of the loss of the ship.

No precaution was neglected by the captain or officers. The holds were examined, and the hatches on the occurrence of rain were attended to.

The court considers no blame can be attached to the officers or anyone on board. All appear to have behaved well in working their vessel into port, knowing her to be on fire, and liable at any hour to explode or burst into flames, when they would have little or no time to take to the boats.

That all on board were in great danger is sufficiently evident by the fact of explosions having occurred shortly after the vessel was anchored, resulting in the loss of one life, and it is also stated that another man was injured.

The court considers that the captain's certificate should be returned to him, and it is returned accordingly.

John Connon,
Senior Magistrate of Police,
G. O'B. Carew,
Lieutenant, late Indian Navy.

Confirmed,
W. R. S. FitzGerald,
Governor.
Bombay Fort Police Court, 13th September, 1869.

"BEACON LIGHT," of St. John's, New Brunswick, of 916 tons register. She was launched at New Brunswick on the 8th September 1869, and classed in the French Lloyds for seven years.

A Marine Court of Inquiry will assemble at the Police Court, Steamer Point, to-morrow morning, the 26th instant, at 7, to investigate and report on the circumstances attendant upon the burning of the ship "Beacon Light."

President: Lieutenant Prideaux, Assistant Resident.
Member: W. K. Thyne, Esq., Harbour Master, and Conservator of the Port.

Aden Residency, 25th January 1871.

The court having assembled pursuant to orders, proceed to read aloud a report handed in by the master of the "Beacon Light," in presence of the officers and crew of that vessel:—

Report of ship "Beacon Light," 916 tons register, of St. Johns, N.B., Fry, master, from Greenock to Rangoon, coals.

September 1st. We towed from tail of bank off Greenoc to Lamlash, and left there on the 2nd. We had light winds and a tedious passage to the Equator; nothing whatever occurring more than is common in any oversea voyage until November 3rd, when Gustav Stoltenhoff, O.S., fell from the cross-jack yard to the deck and was hurt, but no bones broken. On November 8th, at 5.30 p.m., smoke was seen coming up the fore hatch. Master and first mate went into the hold immediately and saw smoke coming up through the cargo at the afterpart of the deck-house on the port side, not far from wood ventilator that went down into the cargo. Immediately took off main hatches and hove coals overboard, so as to be able to get to play water. The force pump was also at once put to work and continued to play water during the night. The main hatch beams and two beams forward were cleared to our view by coals being discharged. The coals and beams on the beams on the port side from main hatch forward to some distance were hot, and it was here the smoke came from. We played great quantities of water from main and force pumps.

November 10th, 6 p.m. We had strong hopes that we were conquerors, but to our utter astonishment, about 3 a.m., on the 11th, great quantities of smoke came from the port side, not allowing any one to go into the hold. We considered it best to batten down main hatches and cut holes in the deck to admit water from main pumps with a hose. In the afternoon removed the long boat from the foreward house to after beams, some hands employed getting sails ready for boats and others pumping.

6.30 p.m. Crew all on deck, some pumping, others in the act of getting sails and painters ready for boats.

With a loud report like a cannon and a flash like lightning all hatches were blown up, fore and aft, carrying the second mate overboard with main hatches, who was seen no more, as night was coming on and great quantities of smoke coming up fore and main hatches, a heavy sea running at the time. The master ordered the boats to be got out for the preservation of life, as there was no chance of saving the ship. The long boat was got out with much difficulty. In getting out the pinnace she was capsized alongside, and John Tompson, seaman, drowned. Everybody, from master to ordinary seaman, used every effort night and day to save the ship. The ship was being steered S.E. by E. in hopes of getting to Tristan d'Acunha. About 9.30 p.m., as near as we could judge, we parted from the ship. Great quantities of smoke were coming from all her hatches. We steered for Tristan, and on Sunday evening saw the land. On the following morning we were met by the kind settlers of that island who had both bread and milk with them in their boat. They joyfully welcomed us to their homes. We were in the boat three nights and two days, in all 61 hours. The boat is left for the good of the settlement on the island. Lat. 35° 31′ S. long. 18° 31′ W. at noon of November 11th, ship run 52 miles S.E. by E., which made us about 274 miles from Tristan when we left the ship.

We, the undersigned members of the crew, declare this to be a true and faithful report of the said ship and her loss.

Edwin Fry, Master.
George Stout, First Mate.
Albert Jackson, Steward.
William Gilchrist.
Johan Peter Engströn.
T. Jansen.
H. P. Oalson.
William South.
Jacob Goldhamer.
F. Kramer.
G. Stoltenhoff.
Robie South.
J. Wright.
J. A. Nilsen.

Ship "Northfleet," January 24, 1871.

Statement of Edwin Fry, taken upon oath this 26th day of January 1871, which said deponent saith:

The statement presented by me to the court is a true and accurate account of the circumstances attending the burning of the ship "Beacon Light," 916 tons, of New Brunswick (St. John's).

She was launched on the 8th September 1869, at New Brunswick, and was registered in the French Lloyds for seven (7) years. Master's certificate of competency, No. 20,707.

1,450 tons of coal, as per bill of lading, were shipped at Greenock, consigned to Messrs. Tod, Finlay, & Co., of Rangoon. The coal was hard steam coal from different mines. I was by the ship all the time she was being loaded, with the exception of two days, when I was absent at Liverpool. There were two ventilators, one close to the main hatch, and the other by the after hatch. The ventilators were placed just outside of the coombings of the hatches. The after hatches were nearly always off, and so were the forward hatches. Three ordinary ventilators led to the hold from the deck. They were fixtures. The hatches were not battened down till we met with some bad weather in the Bay of Biscay. Then they were taken off, coming through the N.E. trades, and put on while the rain lasted at the Equator, and the main hatches were not taken off till they were lifted to discharge coal overboard. The after and fore hatches were kept off on every possible occasion. There was very little coal in the fore hold. Three 'tween deck beams in fore hold abaft the foremast were visible.

I heard that an inspector attended the ship at the time of loading, I believe on the part of the shipper of the coal; and I was told he expressed himself satisfied with the ventilators. I do know his name.

The ship was insured last year for 6,000*l.* (30,000 dols.) in America. The policy expired in October 1870, and I am not aware to what amount (if at all) she is insured at present. I am a part owner in the ship to a three-sixteenth share. The only insurance I know is 1,100*l.* advanced on the freight of the vessel for the voyage in which she was lost.

EDWIN FRY.

Before me, this 26th day of January 1871,
W. F. PRIDEAUX,
Asst. Res. and Magistrate.

Re-examined on his former oath.

I have been engaged in carrying coal for several years in one ship, the "Evening Star." I carried six cargoes to the West Indies without a ventilator of any description.

I consider the means of carrying coal as employed in vessels going round the Cape as quite insufficient, for the following reasons:—

1st. That they have not, as a rule, 'tween decks laid fore and aft, so as to separate the cargo. The "Beacon Light" had no 'tween decks.

2ndly. Because different kinds of coal are shipped on board the same vessel. I consider that only coal from one mine should be shipped on one vessel, as different kinds of coal generate different gases.

3rdly. Without fore and aft ventilation I do not consider it safe to carry coal on a long voyage.

I have arrived at these conclusions since the loss of the vessel and in consultation with the master of the ship "Northfleet."

The name of the second mate who was drowned is Joseph Dixon, of Liverpool, England, and of the seaman John Thomson, a Norwegian.

Of the remainder of the crew August F. Rabé, carpenter, of Germany, and Andrew Wilson, able-bodied seaman, of Norway, went on board the American whaling barque "Xantho," of New Bedford, U.S., Captain Lavars, on November 21, 1870.

There were three boats on board at time of explosion—a longboat, pinnace, and gig. The longboat was sufficient to carry crew. At time of casualty all plain sail to topgallant sails was set.

EDWIN FRY.

Statement of GEORGE STOUT, taken on oath the 26th day of January 1871 which said deponent saith:

I hold a certificate of competency as first mate, No. 87,137, and served as chief officer on board the "Beacon Light."

The statement handed in by the captain, which was read out, is correct.

I joined the "Beacon Light" as first mate about a week before she sailed.

I corroborate as correct the evidence given by the captain with regard to the ventilators and hatches which is now read over to me.

The last of the coals were being taken on board when I joined the ship.

In my opinion, nothing could have been done otherwise than what was done by the master. If the accident occurred over again, I cannot suggest any other means for extinguishing such a fire.

GEORGE STOUT.

Before me, this 26th day of January 1871.
W. F. PRIDEAUX,
Assistant Resident.

Statement of ALBERT JACKSON, taken on oath this 26th day of January 1871, which said deponent saith:

I was steward on board the ship "Beacon Light."

I have heard the statement made by the captain regarding the ventilators and hatches, and it is correct in every particular.

ALBERT JACKSON.

Statement of JAMES WEIGHT, taken on oath this 26th day of January 1871, which said deponent saith:

I was an A.B. seaman on board the ship "Beacon Light."

I have heard the statement made by the captain regarding the ventilators and hatches, and it is correct in every particular.

JAMES WRIGHT.

Statement of ROBIE SOUTH, taken on oath this 26th day of January 1871, which said deponent saith:

I am an ordinary seaman on board the ship "Beacon Light."

I have heard the statement of the captain regarding the ventilators and hatches, and it is correct in every particular.

ROBIE SOUTH.

Opinion of the Court.

The Court having considered the preceding evidence, are of opinion that the statement handed in by the captain and verified by him upon oath is a true and accurate account of the circumstances attending the loss of the ship "Beacon Light," and that the destruction of that vessel was caused by spontaneous combustion.

They consider that all necessary measures were taken by the surviving officers and crew for the preservation of life, that of property being out of the question, and that no blame is attributable to them in any respect.

The court are further of opinion that the remarks made by Captain Fry at the conclusion of his evidence are worthy the consideration of the Board of Trade, with a view to securing greater safety on board of coal vessels proceeding on long voyages.

The court desires, in conclusion, to express their appreciation of the humane conduct displayed by the master of the ship "Northfleet," in receiving the officers and crew of the lost vessel with such a long voyage before him, and beg leave to recommend him to the favourable consideration of the Board of Trade.

W. F. PRIDEAUX,
Asst. Resdt. and Magt., Aden.
W. K. THYNE,
Conservator of the Port, Aden.

Confirmed,
W. R. S. FITZGERALD,
Governor of Bombay.
Aden, 26th January 1871.

"BERNICE," of Liverpool, a sailing vessel of 1,455 tons register, nine years old, and classed A1 at Lloyds, which class would last until June (1873).

Bombay Fort Police Court,
Friday, 4th April 1873.

Pursuant to the Government notification, Marine Department, published in the Governmeut Gazette of the 3rd instant, appointing John Connon, Esquire, Senior Magistrate of Police, and J. E. Betham, Esquire, master of the ship "Princess Somerwatby," to conduct an investigation into the circumstances connected with the total loss by fire of the ship "Bernice," on Saturday, the 22nd March 1873, in latitude 18° 25′ North, longitude 72° 15′ East, the court assembles this day at 11 o'clock a.m., for the purpose stated.

Mr. PEILE, officiating solicitor to Government, appears to watch the case on behalf of Government.

JAMES STANNUS sworn; examined by Mr. PEILE.

I was master of the late ship "Bernice." I have received a copy of the report on which this investigation is held. I hold a certificate of competency as master, granted by the Board of Trade in 1853, No. 9,179. The ship was registered in the port of Liverpool. Her tonnage was 1,455 and 33. She was a sailing vessel, and was built at St. John's, New Brunswick, in 1864. We sailed from Shields on the 9th October 1872. We had coals on board weighing 1,894 tons and 15 cwt. She was classed A1 at Lloyds, which class would last until June next. I believe the ship was insured. I know nothing about the cargo.

All due and possible precaution was taken with reference to ventilation. There were trunk ventilators at each hatchway, which communicated with fore and aft trunks in the lower hold. I had eight deck ventilators. I have had much experience in hot climates in carrying coals. I never saw a cargo of coals better stowed than this. The coals were dry when they were taken on board, and it was dry weather when they were so taken on board. We had very light winds and hot weather, and made slow progress. Up to the 17th March last there had been no indication of any heating or fire among the coals, and the water in the tanks which were stowed amongst the coal in the after part of the ship remained quite cool. On the 17th March the ship was in latitude 15° 53′ North, longitude 72° 34′ East. At 6 o'clock that morning, in consequence of what the chief officer told me, I went into the fore hold with the chief mate. I cleared away some coal; smoke and vapour were coming out just abaft the foremast. I immediately commenced pouring water over the coals. I put the hatches on, and closed the ventilators in all places to exclude the air. We kept pouring water down night and day, till the flames drove the men away from the pumps. On the 19th March, about 4 o'clock in the morning, an explosion took place, blowing the decks up from the main hatch forward. It blew all the hatches and skylights up, and also the house forward occupied by the men. Several of the men were hurt, two rather severely. Two of the men are now in hospital, and are getting better. After the explosion a great deal of vapour was coming out of the fore part of the ship. The barque "Ocean Belle" was seen that morning, and I signalled to her to come up and remain by us. She did so till we left the ship. On the 22nd, about 6 o'clock in the morning, the flames broke out all over the ship, driving the men away from the pumps, and at 7 o'clock that morning the crew left the ship. I stopped there half-an-hour longer. The foremast had fallen before I left, aud about five minutes after I left the other two masts came down. The ship was all in flames fore aud aft. I and the the crew went on board the "Ocean Belle." The "Ocean Belle" remained near the wreck all day. In the middle of the day I went as close to the "Bernice" as I could safely get, and saw that she was a mere shell. There was a great mass of fire and very little smoke. From the time I discovered the fire I did my best to reach Bombay, hoping to keep the fire under till then. The coals belonged to the Peninsular and Oriental Company, and their own surveyor superintended the shipping of the coals in Shields.

By the Court.

I know the ship was insured, but I do not know anything about the cargo. I do not know the amount the ship was insured for; I got instructions about the insurance of the ship. The ship was drawing 22 feet 4½ inches aft, and 22 feet 2½ inches forward. In my judgment the ship was not overloaded; she would have carried 2,000 tons easily. She had 1,894 tons on board. I had every assistance and kindness from the "Ocean Belle." The crew were under proper control. I have been three years in the "Bernice;" I have seen her deeper loaded with salt. We had five boats on board; they were all in good order and ready for use. I saved four boats and sundry other articles belonging to the ship, which have been sold by auction. I saved my nautical instruments, but lost most of my effects.

James Stannus.

Taken on oath before us,
John Connon,
Senior Magistrate of Police,
J. E. Betham,
Captain of the ship "Princess Somerwatby."

By the Court.

The reason why I stopped the ventilation when the ship was on fire was because I thought and always understood that stopping the ventilation was the best way of preventing fire breaking out.

James Stannus.

Taken on oath before us,
John Connon,
Senior Magistrate of Police,
J. E. Betham,
Captain of the ship "Princess Somerwatby."

Philip Coutanche sworn; examined by Mr. Peile.

I was the chief officer of the ship "Bernice." I was on board when the cargo of coals was taken on board at Shields. The cargo was properly stowed, and every precaution was taken with regard to ventilation. We left Shields on the 8th or 9th October 1872. We had light and contrary winds and the weather was very hot. Nothing particular occurred on the voyage, and there was no indication of heat in the vessel or the coals being on fire until the 17th March. At daylight on that morning I noticed steam and smoke coming out of the fore hatchway, and I immediately informed the captain. The captain and I went below and examined the coals. We moved some of the coals and poured water down where we thought the fire was. After pouring a certain quantity of water on the coals, we put on all the hatches and stopped all ventilation except where we were pouring water down. We continued the pumping night and day, and never stopped. On the 19th, at 4 o'clock in the morning, there was an explosion. It blew the decks up in the fore part, also all the hatches and the forehouse. We continued pouring water down after the explosion. The ship was kept by the wind making the best of her way for Bombay. At 6 o'clock in the morning of the 22nd the flames broke out under the fore house, and then all over the ship. The "Ocean Belle" was near us at the time. When the sails forward took fire the crew left the ship and went on board the "Ocean Belle." I left the ship at half-past 7 that morning. The foremast was then over the side, and the mainmast was tottering and it came down about a quarter of an hour afterwards, and brought the mizzenmast down with it. The ship afterwards broke out in flames all over, fore and aft.

By the Court.

I joined the ship a month before we left Shields, and was on board all the time the coals were being taken on board. We were in the dock. The coals were quite dry. I have been in ships carrying coals before. I have been in two other ships bound for Bombay with coals. I think the ventilation on board the "Bernice" was the best I ever saw. There was no suspicion of fire till the morning of the 17th March. The crew behaved very well till the explosion occurred. They then seemed a little frightened. We were well received by the "Ocean Belle."

Philip Coutanche.

Taken on oath before us,
John Connon,
Senior Magistrate of Police,
J. E. Betham,
Captain of the ship "Princess Somerwatby."

Andrew Dryburgh sworn; examined by Mr. Peile.

I was carpenter on board the "Bernice." I was on board the ship when she was taking on board her cargo at Shields. The ventilating apparatus was all fitted before I went on board. I never saw a ship ventilated before. The other ships I have been on board of made short passages only. The ship was staunch and strong when she left Shields on the 9th October. She was tight all the passage. Up to the 17th March nothing particular happened. I was on board that morning, when smoke was seen coming out of the fore hatch. I and the chief mate went down the fore hold to try and find out where the fire was. We started the pumps pouring water over the coals about an hour afterwards. I think the hatches were put on the next day. We continued pouring water on the coals. The hatches were all put on. I remember the explosion taking place on the Thursday. Smoke was first observed on the Tuesday. The explosion occurred before daylight. The fore part of the ship was blown up. It was a loud explosion; several of the men were hurt. The boats were put out, and then we started pumping again. On the Sunday following flames burst out underneath the house in the fore part of the ship. That morning we left the ship and went on board the "Ocean Belle." Shortly after we left the ship the foremast fell, and about an hour afterwards the mainmast and mizzenmast fell. The flames broke out all over the ship after that.

By the Court.

The captain ordered the hatches to be put on. The ship made no water all the way out. I sounded the pumps every day, sometimes twice, all the way out. It is my practice to do so.

Andrew Dryburgh.

Taken on oath before us,
John Connon,
Senior Magistrate of Police.
J. E. Betham,
Captain of the ship "Princess Somerwatby."

William Elliot Jarvis sworn; examined by Mr. Peile.

I am the master of the "Ocean Belle," lately arrived in Bombay on a voyage from Swansea with coals. On a Thursday morning in March last, in consequence of signals, I passed close under the lee of the ship "Bernice." I was hailed by Captain Stannus, who told me his ship was on fire. I went on board. I found the decks were blown up from the main hatch forward, and the forehouse, and some of the men were lying about the deck injured. There was a little smoke coming out of the hold. I sent my second mate and three hands on board to assist. I could not spare any more hands, as mine is a small vessel. On the Saturday afternoon I took on board my ship the effects of the crew of the "Bernice," in order to encourage them to work. On the Sunday morning, about 6 o'clock or half-past 6, I saw the flames break out on board the "Bernice," in the forehouse. The crew of the "Bernice" then came on board the "Ocean Belle." At the time they came on board all the masts were gone on board the "Bernice." The master of the "Bernice" afterwards came on board. We remained by the ship allday, and about 12 o'clock in the day we went close past her, and I went up into the rigging to look at the "Bernice," and saw she was nothing but a shell and a mass of red hot cinders. All the time from the time I first went to the "Bernice" till the ship was burnt we were making the best of our way to Bombay.

By the Court.

All sail was set on the "Bernice" up to Sunday morning or until the flames broke out on board.

W. E. Jarvis.

Taken on oath before us,
John Connon,
Senior Magistrate of Police.
J. E. Betham,
Captain of the ship "Princess Somerwatby."

Report of the Court.

Bombay Fort Police Court,
Monday, 7th April 1873.

Pursuant to the Government resolution, Marine Department, and notification published in the Government Gazette of the 3rd instant, appointing John Connon, Esq., Senior Magistrate of Police, and J. E. Betham, Esq., master of the ship "Princess Somerwatby," to conduct an investigation into the circumstances connected with the total loss by fire of the ship "Bernice" on Saturday, the 22nd March 1873, in latitude 18° 25′ North, longitude 72° 15′ East, the court assembled for the purpose stated on the dates mentioned in the record of proceedings.

From the evidence adduced before us relative to the loss of the "Bernice" by fire, we find that nothing of any consequence appears to have occurred on board the ship till the morning of the 17th March last, when the ship was within 200 miles of Bombay.

At that time smoke and vapour were seen to be coming out of the hold just abaft the foremast.

The vessel appears to have been properly laden and well ventilated, and all due precautions seem to have been taken to guard against fire. The cargo consisted entirely of coal, of which there were 1,894 tons 15 cwts. on board. The registered tonnage of the ship was 1,455 tons, and the captain says that she was fit to carry 2,000 tons, which we believe. The ship left Shields bound for Bombay by way of the Cape of Good Hope on the 9th October 1872. The ship was thus over five months at sea when the fire was discovered among the coals. We are satisfied by the evidence that the ship was well found, well manned, and equipped, and the long passage is accounted for by light and contrary winds.

When the fire was discovered all possible means were, as appears to us, promptly adopted to extinguish it. But notwithstanding the efforts made by the master and by his officers and crew to save the ship from destruction, the fire made rapid progress, and on the morning of the 19th March, about 4 o'clock, an explosion took place, when the decks were blown up from the main hatch forward, also the hatches, skylight, and deck-house forward occupied by the crew. Several of the men were hurt, two severely.

After the explosion Captain Stannus states that he observed much vapour coming out of the fore part of the ship.

Fortunately the barque "Ocean Belle," an English ship, was seen in the morning the explosion occurred on board the "Bernice," and Captain Stannus signalled to the "Ocean Belle" for assistance, Captain Jarvis of the "Ocean Belle" immediately sent such assistance to the "Bernice" as he could. His men well assisted the crew of the ship in danger to work the pumps in the way of playing water on the cargo, keeping the decks flooded, &c., and we are of opinion that every effort which could have been made was made to put out the fire.

On the morning of the 22nd, however, about 6 o'clock, flames broke out all over the ship. The men were driven away from the pumps by the flames, and at 7 o'clock they were compelled to leave the ship, but they did not do so without the leave of the captain. The captain himself remained on board half an hour longer. Before he left the ship the foremast had fallen, and about five minutes after he left the ship the other two masts fell, the ship being then all in flames fore and aft.

The "Ocean Belle" remained near the wreck all day. In the middle of the day Captain Stannus and Captain Jarvis went as close to the "Bernice" as they could with safety, and saw that the vessel had been burnt down to a mere shell.

From the time the fire was discovered Captain Stannus made every effort to reach Bombay, thinking that he might possibly keep the fire under till then and be able to save his ship.

In view of all the facts as they have appeared before us in evidence, we consider that the destruction of the "Bernice" was caused by spontaneous combustion among the coal, which may have been owing in some measure to the long time the ship was at sea before she got near Bombay.

We are of opinion that no blame attaches to the master of the "Bernice," or to his officers or his crew. They have been the victims of a common misfortune, and all seem to have worked with judgment and care in their respective positions, and did all they could do to extinguish the fire and save the ship.

The captain of the "Bernice" holds a certificate of competency from the Board of Trade dated 20 years ago, and under the circumstances narrated in this report, and referring to the evidence taken before us, of which evidence we submit full notes, we have not thought it necessary to detain this certificate, though it was duly tendered us, and we have accordingly returned it to him.

We wish to add a word as to the captain of the "Ocean Belle." He seems to us to have behaved under the circumstances with most considerate humanity, and although that is not more than we should expect of any British seaman, we are of opinion that both before and after the disaster (as he brought the captain and crew of the lost ship all to Bombay in his own small ship and treated them well) he deserves honourable mention, that his conduct should be brought to the notice of the Government, as we now respectfully bring it, and that if the Government is so pleased, it should be brought to the notice of the Board of Trade as well.

John Connon,
Senior Magistrate of Police.
J. E. Betham,
Captain of the ship "Princess Somerwatby."

Bombay Fort Police Court,
7th April 1873.

Confirmed,
J. Wodehouse.
May 5th, 1873.

"COUNTY OF NAIRN," of Glasgow, of 990 tons register, four years old.

Report of the proceedings at a Court of Inquiry held at the office of H.B.M. Acting Vice-Consul at Sourabaya on the 23rd day of August 1873, for the purpose of investigating the circumstances attending the loss of the British ship "County of Nairn," of Glasgow, Richard McKenzie, master.

Present:

Alexander Hadaway, H.B.M. Acting Vice-Consul, President.
N. McVicar, partner of the firm of Messrs. Martin, Dyce, & Co.
James Greig, procuration holder of the firm of Messrs. Pitcairn, Syme, & Co.
Alexander McNab, master of the British ship "Daniel Rankin."

The president, having duly sworn the members of the court, explained to them the nature of the inquiry about to be instituted.

Captain McKenzie was then called and duly sworn, and in reply to questions put by the court, stated that he was 46 years of age. Had held a master's certificate for 16 years, and had been 6 years in the employ of the owners of the "County of Nairn." He once before had an accident with a vessel under his command; it was in 1859, and happened on the west coast of Scotland.

On being requested by the court to state the circumstances connected with the loss of the vessel, Captain McKenzie referred to his logbook, the entries in which from the 12th to the 16th August were then read out by the president as follows:—

Tuesday, the 12th August 1873.

Courses.	Winds.	
S.E. by E.	N.N.E.	a.m. Light wind and very
S.E. by E.	East.	variable. 2 a.m. steady; 8, tacked ship; 8.30, galley caught fire; it was soon put out, doing little harm; people variously employed.
S.S.E.	"	Noon, tacked ship; 1 tacked
S. by E. ½ E.	E. ½ S.	ship. p.m. Discovered smoke issuing from the coals on port side of main hatch; called all hands; passed pig iron on deck; removed boxes, crates, &c; from place around the smoke; played water on it.
E.S.E.	N.E.	5. Passed the powder on poop, and covered it the best we could with sails, tarpaulings, &c.
N.N.E.	—	Latter port, water kept playing on it to keep it down.
		11. Hard breeze came off, tacked ship to Eastward.

Wednesday, 13th August.

N.E. by E. ½ E.	S.E. ½ E.	a.m. Moderate breeze and clear weather. 4 a.m. do. do. 6 a.m. Crimon Java,
N.E. by E.	S.E. by E.	bore N.N.W. 4 miles; found the smoke coming
E.N.E.	S.E.	out all over in square of main hatch; cleared all
E. by N.	S.E. by S.	cargo out of the road, and kept water going on it;
S.W. by S.		it feels very warm; all hatches are on but one of the hatches at the main
South.	E.S.E.	for to get water down.
S. by E.	E. by S.	The smell of gas is so strong that it almost chokes the people in the hold. Noon, fine breeze; p.m., moderate breeze and fine clear weather.
		2 p.m. Tacked ship.
		Still playing water on the coals; they are getting much worse.
		11 p.m. Tacked ship.
		Still keeping going; smoke all over the deck; can't sleep in forecastle for gas.
		Midnight. Squally with rain; pumps attended to

Thursday, 14th August.

Courses.	Winds.	
S.E. ½ E.	S.S.W.	a.m. Fine breeze and clear weather. 4 a.m. Do. do. do.
S.S.E.	S.W.	There is smoke coming out all round the mainmast; took more pig iron.
S.E.	Variable.	On deck and shifted more cargo, keeping water on it.
		10 a.m. People cannot stop below, it is increasing so very fast. Smoke is like to suffocate us about main hatch; there is smoke coming out of all three lower masts heads, and
		p.m., now fearing that the coals are become ignited, judged it is prudent to make for the western channel to Sourabaya, to scuttle ship on bank, and get assistance. 6·46 p.m. Tacked ship to S.E.; at 9 p.m., sighted light-ship; at 9.30, wind hauled off the land, which taking the ship aback, obliged us to come to anchor in 7 fathoms water, with 30 fathoms chain on port.
		Light-ship bearing S.E., Panka Point S.W. and while in the act of clewing up the sail, flames burst up the main hatch; at once the order was given to heave gunpowder over board, and then to get out the three boats on beams aft; left the ship and lay by her; flames half up the mainmast.

Friday, 15th August.

		a.m. Flames stopping, judged it safe to board the ship; hove short and made sail ready to run the ship on the bank. 6 a.m. Spoke the schooner "Duke of Richmond," bound to Sourabaya; desired him to report ship on fire, and we did intend to scuttle her. Wind came in favour, weighed anchor, and proceeded towards the bank; wind light and coming right out, at the same time tide being against us we had to let go anchor again in 5 fathoms water; all keep playing water on fire. 11 a.m. Received on board a pilot; sea-breeze came strong, which caused the flames to increase very fast in the hold, but it keeps under hatches. 1 p.m. Tripped anchor and run the ship on shore, Panka Point.
		Bearing W. ½ S. ½ a mile; two anchors let go and bored holes to scuttle ship; while in the act of scuttling her, ship burst out in flames, forcing us to take to the boats as fast as we could assisted by two of the Panka pilot boats, saving but very little, amounting to gig, pinnace, and life-boat, with their gear, several bolts of canvas, chronometers, barometers, and compass landed with boats at Panka Point, ship being in one complete mass of flames fore and aft.

Saturday, 16th August.

		a.m. Daylight, all masts over the side, and giboom and hull of the ship burned to the water's edge; ship not being full of water to cover all the fire, made carpenter cut large hole to let water run in faster, which completely scuttled her; finding that nothing could be saved, got boats from Resident of Sedayoe, and proceeded to Sourabaya with crew, leaving the wreck of ship in charge of the Resident, with orders to keep a strict watch till further orders.

We, the undersigned, hereby certify that the above is a true and correct copy of the log-book of the "County of Nairn" from the 12th to the 16th August.

ALEX. HADAWAY,
H.B.M.'s Acting Vice-Consul.
NEIL MCVICAR,
Partner of the firm of Martin Dyce & Co.
JAMES GREIG,
Procuration holder of the firm of Pitcairn, Lyme, & Co.
ALEX. MCNAB,
Master of ship "Daniel Rankin."

CAPTAIN MCKENZIE stated further: Shortly before noon on the 12th, we lay a little to the west of the Island of Carimon, Java. I then steered for land due south, a light wind blowing from E. ½ S., but later on a land breeze springing up, I was forced to sail off land. Until the 13th, I did not look upon the fire as serious, and then from the position of the ship and the direction of the wind, it was easier to make for Panka Point than Samarang. When the log-book says the galley was on fire, it is incorrect, it should be a fire was in the galley. It was of a trifling nature, and was caused by the upsetting of some fat out of one of the pans into the fire. The woodwork over the fire was merely blistered; no sparks could possibly have come out of the galley and supposing a few had, it was impossible that they could have reached the hold. When the smoke first appeared, I accounted for it that the coals were either on fire or smouldering. I consider that there was sufficient ventilation for the coals on board. Altogether there were 600 tons, and they were stowed in two portions, one portion consisting of 400 tons was stowed directly under the main hatch and separated from the other portion, consisting of the remaining 200 tons, by a pile of crates. Both portions were about 13 to 14 feet deep at their deepest part, gradually tapering off towards the fore hatch and stern respectively. I am unable to say whether the coal was damp when taken in. When the smell of gas was first reported, I went below, and seeing nothing, ordered the hatches to be left open to carry away the smell. When we tacked at noon on the 12th, the hatches were still open, but they were afterwards closed. I have often carried the same quantity of coal with no better ventilation. I tried to get up a portion of the cargo, first passed up the gunpowder, after which we found it impossible to remain below. I then proceeded with all despatch to reach Panka Point, on purpose to scuttle the ship, which was the only way of saving either ship or cargo. I had pumps and a fire-engine with a long hose, all of which were in good order; we had also a good supply of buckets, which were used.

WILLIAM THIRD, first officer, was now called, and having been duly sworn, deposed as follows:—

I am 31 years of age. I have held a master's certificate for two years, Have been 17 months on board the "County of Nairn." The log-book was written by me, and I confirm its contents. When the fire in the galley took place, the cook, who acted also in the capacity of steward, was aft attending the captain and myself at breakfast. The paint in the galley was merely blistered, no other damage was done; and by the time the captain and I got forward, the fire had been put out. I am unable to account for the coals getting on fire, the ventilation was good, in fact better than in most vessels in which I have served.

He confirmed Captain McKenzie's statement as to the manner in which the coals were stowed.

The fire must have commenced in the coals stowed directly under the main hatch. The crates being stowed between the two lots of coal would assist the ventilation, the masts were ventilators, as they were of iron and had the usual holes, which I know were not plugged up, as when stowing the coal, I myself had them cleaned out. The coals were dry when taken in. I have often been in coal ships before, and in some without any ventilation whatever. While in Batavia the coals were never touched, neither was there any smell of gas detected.

He confirmed Captain McKenzie's statement as to the first discovery of smoke. When the pig iron, boxes, &c. had been cleared away, I distinctly saw smoke coming up through the coal. I have no reason to suppose that there was any carelessness on the part of the crew. It was impossible to scuttle the vessel before we did. The holes for scuttling were all bored from the outside. An unsuccessful attempt was made to save the sails, but it was impossible attempting to save any of the cargo. Between decks fore and aft she was one mass of flames. I cannot understand how it was she lasted as long as she did; and when the flames did burst out, I had to spring off the bows to gain the boats.

ROBERT BREMNER, second mate, was now called, and having been duly sworn, gave the following evidence:—

I am 25 years of age. I hold a first mate's certificate, and have served for about two years in this vessel. It was my watch when the smell of gas was first detected. It was reported to me by Collins, who had opened the fore hatch in order to go below for a small piece of wood. I tried to go down the fore hatch, but the smell of the gas almost choked me. I went and reported to the captain. The vessel was well ventilated. The masts were not plugged. I consider the mainmast was a good ventilator.

He confirmed Captain McKenzie and the first mate as to the stowing of the coal. The coal was dry when taken in; cannot say cause of fire. In Batavia, I was in the hold every day for 12 hours, and there was then no sign of anything going wrong.

When we tacked on the 12th, this being about the time the smoke was first discovered the hatches were open. There was no carelessness on the part of the crew which could have occasioned the fire. The fire in the galley was nothing; it was no fire at all.

COLLINS, A.B., was now called, and having been duly sworn, deposed as follows:—

I am a Canadian, and am 30 years of age. I went to the forehatch for the purpose of going below to get a piece of wood, but upon lifting the hatch, the smell of gas was so strong I could not descend. I then reported to the second mate, who went forward to see what was the matter. I was never before in a vessel laden with coals. From what we could see by digging in the coals through which the smoke was issuing, and which at first were not so hot as to prevent our standing on them, it is my opinion, and that of the rest of the sailors, that the fire was at the very bottom of the coals. I saw smoke coming out of the masts.

What Collins stated with respect to galley fire confirmed Captain McKenzie's statement.

JAMES LESLEY, carpenter, was now called, and having been duly sworn, stated as follows:—

I am 23 years of age, and have served three and a half months on board the "County of Nairn." I previously served on board a steamer. He confirmed Captain McKenzie's statement in regard to fire in the galley. When smoke was first reported I was at my usual work. I consider there was sufficient ventilation. I know nothing about the stowage of the cargo. I joined the vessel the day she sailed. It was I who scuttled the vessel. I worked first from a boat, afterwards from a stage slung on the outside of the ship, and as the swell rose and fell I was sometimes almost submerged, in fact it was with great danger to both life and limb that I succeeded in doing what I did. I bored five holes 1¼ inches in diameter in each side of the vessel, having first removed the copper. There was no possibility of cutting out a square block. I consider my best was done to scuttle the vessel; in fact there was no other way. The vessel was well supplied in case of fire.

CHARLES MARSHALL, A.B., was now called, and having been duly sworn, deposed as follows:—

I am 37 years of age. Have served three and a half months on board the "County of Nairn." I was at the wheel when smoke was first reported. From 12 a.m. to 2 p.m. we were on the starboard tack. I saw smoke coming up through the coal from underneath the beams. Everything was done that could be done to put the fire out.

JOHN RUSSELL, cook and steward, was now called, and having been duly sworn, gave the following evidence:—

He confirmed Captain McKenzie's statement in regard to origin of the fire in the galley. The paint which was blistered had not fallen off, and did not come off until I had scraped it off. I never before went as cook and steward.

ARCHIBALD GRAHAM, boatswain, was now called, and having been duly sworn, stated as follows:—

I have been three and a half months on board the "County of Nairn." I have been four years at sea. It was not my watch when the smell of gas was first discovered. I was below when the smell of gas was reported. The captain went down forward and came up aft, but could see nothing.

We were tacking the ship when smoke appeared. The hatches were open, but they were afterwards closed. There was no carelessness on the part of the crew with regard to lights.

Decision.

We, the undersigned, having duly weighed the evidence adduced, give it as our opinion that the loss of the British ship "County of Nairn" was caused by fire, the origin of which is unknown, but supposed to be spontaneous combustion of the coals in the lower middle part of the ship. Considering the limited quantity of coals on board, and the manner in which they were stowed, we are satisfied that the means of ventilation were sufficient, and that everything that could be done was done, after discovery of the fire, to put it out. We are further of opinion that, from the position of the ship and direction of the wind at the time the smoke was discovered, the captain acted rightly in steering the course he did, and finally scuttling the ship at Panka Point. We thus exonerate Captain Richard McKenzie from all blame, and consider that he and his officers acted with energy and judgment throughout.

ALEX. HADAWAY,
H.B.M.'s Acting Vice-Consul, President.
NEILE MACVICAR,
Partner of the firm of Martin Dyce & Co.
JAMES GREIG,
Procuration holder of the firm of Pitcairn, Syme, & Co.
ALEX. MCNAB,
Master of British ship "Daniel Rankin."

"PRINCE ALFRED" Barque, of Shoreham, of 368 tons register, 5 years old, and classed at Lloyds A1 9, years.

Proceedings of the Mauritius Marine Board in the case of the bark "Prince Alfred" of Shoreham, of the burthen of 368 tons, Henry Puttick, master, which vessel was burnt in latitude 23° 30′ S. and longitude 79° 56′ E. on the 31st August last, on her voyage from Hull to Point de Galle.

The Board assembled at the Port Office on the 14th September 1873.

Were present:

JOHN MORGAN, Esq., Acting Harbour Master and President.

J. COWIN, Esqre., W. A. BARCLAY, Esq., J. P. ELLIS, Esqre.	Marine Surveyors.

Assisted by:

Captain T. MILES, Barque "Umvoti."

Presented themselves for examination:

Henry Puttick, master; James Saunders, mate; Peter Thomson, boatswain; Jelmar Wilkman, A.B., of the late barque "Prince Alfred."

The following documents were laid before the Board:—

1. The Official Log book.
2. The Chart by which the vessel was navigated.

All the evidence was on oath.

HENRY PUTTICK sworn:

39 years old; born at Shoreham. Have been 19 years to sea. Have been in command about 8 years. Have a certificate of competency. Have been 2½ years in command of the barque "Prince Alfred." The vessel was built at Shoreham, in 1868. She is, I believe, not insured; classed at Lloyds A1, 9 years; docked and re-metalled in May last at Hull. The vessel was in a proper and seaworthy condition when she left home; continued so throughout, up to the time of the accident. Smoke was first seen on the 30th August, no symptoms before that. At 5 a.m., on the 30th August; the first indication of fire was in the forecastle, reported by the men to the mate and afterwards to me, and I came on deck and saw smoke coming out of the forehatch. I went down in the hold and felt the coals; they appeared quite cold. We then took off the main hatches and trimmed cargo on deck, from 15 to 20 tons; about 4 feet down found the cargo quite hot. We poured water in about one hour and a half, it getting so hot, that the men could not stop down. No ventilation through the cargo. We shortened sail, hauled up the courses, hove the ship too, main topsail aback, and battened down all hatches. The vessel was going free. I did all in my power to save ship and cargo. The vessel was from Hull with a cargo of steam coals for Point de Galle. My vessel was inspected by Lloyds' Surveyor to take in any cargo. We then put out the long boat, provisioned her, passed her astern with two men remaining in her, the remainder stopping on board the ship. At 3 a.m. on the 31st of August, fire broke through the port covering board between the fore and main rigging; put out the small boat and laid by the ship till daylight, then myself and one man went on board and found her burning furiously. I went down in the cabin to get some more provisions and the man with me; smoke being so suffocating that I could hardly get up the ladder, when I got in the air I fell down insensible and the man having to heave water over me to bring me too, sufficiently to get in the boat. About noon on the 31st of August I abandoned the ship in latitude 24° 30′ S. and longitude 79° 56′ E. the ship being on fire and burning furiously. No smell of gas or sign of smoke previous to the 30th of August. I do not know the quality of coals put on board. It was rainy weather at the time they were shipped. I have taken many cargoes of coal before; never used ventilators through the cargo; was neither suggested to me, nor did I consider it necessary this voyage. I beg of the Board to record my grateful thanks for the humane and kind treatment I received myself and crew from Captain Pichon of the French barque "Boussole" from the time he picked us up till he landed us here in the Mauritius, which caused a serious deviation from his voyage.

JAMES SAUNDERS sworn:

31 years old, born at Shoreham. I have been 15 years at sea. I have been mate of the barque "Prince Alfred" for 2½ years, and have been 5 years as boatswain and mate in other ships. I was on board the vessel when she was in dock and re-metalled. Rainy weather when the coals were shipped. The vessel was tight and in a proper and seaworthy condition, and continued so throughout till the accident. At 5 a.m. on the 30th of August, smoke first reported to me by the men in the forecastle and I reported it to the captain. There being a nasty smell of gas in the hold, took off the hatches and trimmed cargo on deck; finding the coals quite hot about 4 feet under the main hatch, put down water, trying to stop it, could not succeed, and the cargo getting so hot, that the men could not stop in the hold, put on the hatches and battened down, ship going free. Took in sail and hove the ship to; got the long boat ready, put her out, provisioned her, and passed her astern with two men in her, the remainder stopping on board. About 4 a.m., on the 31st August, saw the fire coming up through the port covering board amidships; put the small boat out and laid by the ship till daylight, when the master and one man went on board and found the ship burning furiously. The master and man went in the cabin to get more provisions, the smoke being so suffocating that they could not stop. The master on getting on deck fell insensible, the man having to heave water over him to bring him to. (This information I got from the man who accompanied the captain.)

We tried to extinguish the fire by pouring water in the fore and main hatch for an hour and a half, but without avail. I beg leave to bring to the notice of the Board the kind and humane treatment we all received from Captain Pichon and his men of the French barque "Boussole" from the time we were picked up till the time we landed in the Mauritius.

PETER THOMSON sworn:

27 years old. Boatswain of the barque "Prince Alfred." Born in Iceland; joined the vessel in May ultimo, at Hull. The ship was loaded when I joined her. The vessel was in a proper and seaworthy condition to the time of her accident. She was not overladen. Smoke was first seen in the forehatch; opened the forehatch first and smelt a nasty smell. The crew lived in the forecastle. Poured water down in the hold for about 2 hours; trimmed cargo, put coals on deck from the main hatchway; found the coals too hot and the men could not stop down below. Battened the hatches down, took in the small sails, hauled up the courses, and hove the vessel too; main topsail aback, got the things ready for the boat. The captain ordered the long boat to be put out, provisioned her and passed her astern. Two hands were in the boat. We left the ship at 4 a.m., fire was through the covering board, pitch was boiling out the seams of the deck, the deck was so hot so that we could not stand on. The captain, in my opinion, tried all he could to save the ship and cargo. There were no signs whatsoever of smoke or gas until the morning of the 30th August.

JELMER WILKMAN,

Born in Sweden. 22 years old; A.B. I joined the vessel at Hull, the vessel being laden. I have been three years to sea. The vessel was tight and in a proper and seaworthy condition from leaving Hull to the time of her accident. I smelt the smoke in the forecastle at 4 a.m. I came on deck, and at 6 a.m. I took the wheel and saw the smoke coming from the fore hatchway; water was poured down in the fore and main hatchways. The coals were taken out and the water was poured down. The coals were so hot that I could not remain below and the farther I went down the hotter it became. When I came up the captain gave the orders to batten the hatches down and shorten sails; sails were shortened and vessel hove to. We got the long boat out ready, provisioned, and passed astern, and put two men in her. Remained on board till Sunday at 4 a.m. When we left the ship, the fire came through the covering board from the port side, with great quantities of smoke. At about 10 a.m. again boarded the ship with the captain to try to get some more provisions. When we got on board the cabin was full of smoke. I assisted in putting the bread in the boat, and on my return I found the captain lying on the deck senseless, and blood running from his nose. I took a bucket of water and hove it over his head, which brought the captain round. We then left the vessel. We were in sight of the vessel all day, and at night time there was a glare.

Report.

The "Prince Alfred" was built and registered at Shoreham, in 1868; official number 58,023, and classed A1 9 years at Lloyd's; she was flushdecked, barque rigged, and 368 tons register. She was owned by Thomas Gates, Esq. shipowner, of New Road, Shoreham, and commanded by Mr. Henry Puttick, who holds a certificate of competency as master mariner, number 89,520, and was bound from Hull to Point de Galle (Ceylon) with a cargo of 614 tons of steam coals. The "Prince Alfred" left the Humber on the morning of the 5th of June. She was then tight, staunch, and in every way seaworthy, and continued so up to the time of her loss. The voyage was successful up to the 30th August, when smoke was observed in the forecastle, and on examination, it was evident from the heated temperature that combustion was going on; the hatches were taken off, and water was freely poured down the forehold on the coals. About 15 tons of cargo was dug out of the main hold, and hove on deck, in the hope of getting at the real seat of the fire. The men remained at work till driven on deck by the heat and the rapidly increasing volume of gas and smoke; it was then 8 p.m., the hatches were ordered to be put on and battened down, the light sails and courses clued up, and the vessel hove to; the long boat was then got out and rigged, her sails and rigging being below; a small supply of provisions and water was put into her and she was passed astern with two men in her, the rest of the crew remaining on board, still hoping that the fire might smother. At 4 a.m., on the morning of the 31st, the fire broke through the covering board on the port side amidships, the jolly boat was then put out and the master and crew left the vessel and took to the boats, but remained by her until daylight when the master and one of the crew went on board to try and get some more provisions and found the vessel burning with great fury; the master went down in the cabin, the smell and smoke almost suffocating him. He just managed to crawl on deck when he fell down senseless and bleeding at the nose. The man that was with him had the presence of mind in heaving water over him, which brought him sufficiently round to get him back into the boat. At noon, finding the ship to be hopelessly on fire, and having only a small supply of provisions and water, the nearest land being Rodrigues, which was 1,000 miles off; they left the vessel; Mauritius then bore from the west 1,388 miles. The master was the last to leave and the whole proceedings, in the opinion of the Board, are marked with judgment and prudence and highly commendable. As the jolly boat, which was in tow of the long boat prevented their progress she was cast off and turned adrift on the 7th day after leaving the vessel. Having gone a distance of 900 miles they were when in latitude 19° 39′ S., longitude 62° 30′ E., fortunately picked up by the French barque "Boussole," Captain René Marie Pichon, who treated them with great kindness.

The Board cannot close this report without reminding the authorities of the great want of precaution in loading a combustible cargo, such as coals, without the vessel being properly ventilated; this ought to be made compulsory on vessels accepting charters to carry coals on a long passage. Here is a fine vessel lost, valuable lives risked, whereas, if ordinary precautions of ventilating the vessel had she been attended to, she might have been saved.

The Board are therefore of opinion that the evidence on oath of the captain, officers, and of the crew, is a true and accurate account of the circumstances attending the loss of the "Prince Alfred," and that the destruction of the vessel was caused by spontaneous combustion, and consider that all necessary measures were taken by them to extinguish the fire, and that no blame whatever is attached to the Captain, officers, and crew, and have much pleasure in returning Captain Henry Puttick and Mr. James Saunders, chief officer, their certificates.

The pleasing duty now remains of recording the generous and hospitable treatment received by the master, officers, and crew of the unfortunate "Prince Alfred" from Captain René Marie Pichon, of the French barque "Boussole," to which ample testimony was borne in receiving them on board of his vessel, having a long voyage before him, and having come out of his course to land them here, by which his vessel would lose at least four days; we therefore beg leave to recommend him to the favourable consideration of the Board of Trade.

John Morgan,
Acting Harbour Master and
President of the Board.
John P. Ellis,
John Cowin, } Marine Surveyors.
A. W. Barclay,
Thos. Miles,
Captain of the barque "Umvoti."

"ST. MUNGO," of Glasgow, iron ship of 1,375 tons register, eight years old, classed A1 for 21 years.

Proceedings of the Mauritius Marine Board in the case of the ship "St. Mungo," of Glasgow, of the burden of 1,375 tons, Frederick Broom, Master, which vessel was burnt in latitude 34° 57′ South, and longitude 20° 18′ West, on the 13th September last, on her voyage from Dundee to Point de Galle.

The Board assembled at the Port Office on Monday the 20th October 1873.

Were present:

John Morgan, Esq., Acting Harbour Master and President.
John Cowin, Esq.,
W. A. Barclay, Esq., } Marine Surveyors.

Assisted by:

Captain J. Anderson, barque "Lorton Vale."
Captain W. H. Metcalf, ship "Beechardass Ambaidass."

Presented themselves for examination:

Frederick Broom, master; John Henry Buckler, first mate; Alexander Scott, second mate; William Ross, steward; George Wilkie, carpenter; Charles Reilly, A.B.; John Miller, A.B., of the late ship "St. Mungo."

W. H. Hathorne, master of the American barque "Taria Topan."

The following documents were laid before the Board:—

1. The official log-book.
2. The chart by which the vessel was navigated.

All evidence was on oath.

Frederick Broom sworn:

30 years old; born at Rochester; holds a certificate of competency. Has been to sea 16 years. Has been 12 months in command of the ship "St. Mungo." Was my first command. The vessel was insured; she was built in 1865; she was classed A1 21 years. The vessel was tight, and in a proper and seaworthy condition when leaving Dundee. First symptoms of smoke were seen on the morning of the 13th September. The vessel left Dundee on the 9th July; had a full cargo; was not overladen; she had about 500 tons of coals in the tween decks; her draught of water was 20 feet 3 inches when we left. No symptoms of smoke or smell were discovered before the morning of the 13th September. The ship was well ventilated (nine ventilators), one ventilator went down through each hatchway down to the kelson, two on the poop went through the main deck, two through the main deck between the fore and main hatchways, two through the main and top gallant forecastle deck. After the first appearance of smoke my first order was to get all stores of a combustible nature upon deck; whilst that was being done, I went down in the hold accompanied by one of the seamen. The steward, William Ross, was the first person who saw the smoke at 8.15 a.m., on the 13th September, issuing from the after-hatch ventilator and reported it to me. I could not stop long down the hold on account of the smell of the gas, and had great difficulty in getting on deck again. The man Robertson was with me, but he did not appear to be as much exhausted as I. When I came on deck I ordered everything to be battened down. During the time they were battening down hatches I went down the three ventilators followed by the chief officer, and found cool from kelson to about six feet up, the smell of the gas being so strong below that no one could stand to work. No wet coals were taken on board at Dundee. 4 p.m., everything appearing cool and no smoke observable, I had one of the ventilators on the poop opened to allow the foul air, if any had generated, to escape. I allowed it to remain off for a short time and then put it on again. After this precaution I felt no fear of an explosion. At

8 p.m. the crew came aft and asked me to close on a barque which had been in company with us to windward all day, but I had already altered my course so as to keep her in signal distance. At 10 p.m. and also at midnight there appeared to be no presence of fire. At 3 a.m., the fore, main, and after hatches blew off with a terrific roar, and a flame leaping half way up the mainmast and immediately going out, but dense volumes of smoke arising from all hatches. I immediately set all hands drawing water and pumping with both force pumps down the mainhatch, and cut holes in the deck as well, and as soon as the smoke had partly cleared, I made a line fast round me and got into the hold, but still could see no traces of flames, but conclude it was in the body of the ship. At 5.30 a.m. we sighted the barque "Taria Topan," and bore down upon her and asked him to stop by me. I also sent a boat for the captain and got him on board and showed him the exact state we were in and agreed upon signals in case of us being compelled to obtain his assistance, intending in my own mind to try and get the ship into the Cape. The crew said that they would work with greater will if we steered so as to sight Tristan d'Acunha or the nearest land, and if the ship was then safe, they would do their utmost to get her into the Cape. This was agreed upon by myself and the captain of the barque. Every effort was made to get the fire under, but it was of no avail. At 2.30 p.m. the men said they saw no abatement; in fact, the fire was increasing, and they wanted to leave the ship, but I got them to try again with a will for one hour, and then I would see what was to be done (myself and the officers heaving coals overboard, trying, if possible, to get nearer to the fire, but we had to desist on account of the sulphur). At 3 p.m. the flames burst through the deck out of the starboard-chain-locker, quickly spreading all over. Seeing that no more could be done to save the property, and deeming it necessary for the preservation of life, I signalled the barque to close and gave the order to get the boats out and abandon the ship, which was effected by about 6 p.m., by which time the ship was in flames fore and aft. At about 8 p.m., as could be seen, there was only the mizzenmast standing, and it on fire. At 10.30 p.m. the boats being secured, the captain filled on his vessel, the flames could be seen up to about 12.30 a.m., the barque going at the rate of nine knots per hour.

In concluding my evidence, I cannot omit to testify to Captain W. H. Hathorne, his officers and crew, my sincere gratitude for the aid and assistance they kindly lent me on the occasion, and for the hospitable and generous manner in which we were all treated whilst on board his vessel.

John Henry Buckler sworn :

52 years old; born in Sussex. I hold a certificate of competency as master. I have been nine years at sea, since last August. I have been four years as second mate and mate. I have been two years as chief mate of a vessel. Since last July I joined the ship "St. Mungo" at Dundee, a fortnight before she left; when we left Dundee, the vessel was in a tight, proper, and seaworthy condition. Nothing particular on the passage until the morning of the 13th September. Smoke was first seen on Saturday, 13th September, at 8.15 a.m. I was coming out at the cabin door after breakfast, the steward told me there was smoke coming out of the after-hatch ventilation, and I told him to tell the captain. The first thing the captain did, he went down the ventilator himself, ordered all the combustible stores to be removed, then battened tight down. In the afternoon I was sent to feel the hatches whether they were heated, and found them all cool, and in the dogwatch the captain ordered me to take off the cover of one of the ventilators on the poop. I took it off and kept it off until the captain told me to batten down again. During my first watch on deck I frequently felt the ventilators and hatches to see if they were getting heated, but found everything cool. At about 8 p.m., on Saturday the 13th, the men came aft and asked the captain to bear up for the vessel that was in sight. The captain answered that he had already done so. At midnight I was relieved by the second mate, and laid down in the after cabin, and about 3 a.m. (Sunday) I was awoke by a fearful explosion and rushed on deck, where I met the captain on the break of the poop; he was then giving orders to get the pumps and buckets under way. I went down on the main deck and looked after the work, set the men at the fire engine, force pump, and buckets. At about 4 a.m. the captain came on the main deck, and had a rope made fast round him and another small one in his hand, and went down each of the hatches, but had to come up soon on account of the smell of the gas, and when he returned he was almost exhausted. After the captain, I went down myself and could feel the heat of the coals in the main hatch, about 6 feet from the kelson. The captain ordered the carpenter to cut holes in the deck, to get the water down the better. Then at daylight on Sunday, 14th, we sighted a barque down to leeward; bore down to her, and proved to be the American barque "Taria Topan." Captain Broom hailed him and asked where he was bound to; he answered Mauritius; told him the ship was on fire and asked him to see him into the Cape. The captain of the "Taria Topan" said it was out of his course, but he would come on board. I was sent away in the boat for him; brought him on board; whilst on deck, and about to return on board his own ship, the men came aft and asked Captain Broom to steer for the nearest land, and asked the captain of the "Taria Topan" whether he intended to stay by them, as they said they could work with greater will. He told them he had agreed to do so. I swear that Captain Broom did all in his power to encourage the men and save the ship, but to no avail. When we started the boats at 3 p.m., the flames came up the chain pipes; the fire was spreading rapidly, notwithstanding the exertion of all hands at the pumps. The captain signalled the "Taria Topan" to close with him, and that he could do no more. About 6 p.m. the captain and myself left the "St. Mungo;" the flames were coming up all the hatches and spreading very fast; went on board the "Taria Topan." At night, about 10.30 p.m., the captain of the "Taria Topan" squared away; the "St. Mungo" presented one mass of flames. I wish to return our grateful thanks to Captain Hathorne, his officers and men, for the good treatment we all received whilst on board his vessel.

Alexander Scott sworn :

30 years old; born at Dundee. Has been 18 years to sea. Has been 15 months as officer. Has been two months and a few days on board the ship "St. Mungo." I joined the "St. Mungo" after the cargo was on board. I was at the fore part of the after hatch when the smoke was seen. The steward was the first to see the smoke. The steward and I came aft, and the captain at the time coming out of the cabin door, we told him about the smoke. The first thing the captain did, he went down himself with the seaman William Robertson, to examine if there was any fire or heat to be found, but could not feel any. The captain ordered the stores to be removed on deck and hatches battened down. Everything was covered up. At 3 a.m. on Sunday, 14th, I was on the poop and heard an explosion in the main hatch, and I called all hands; captain came out and ordered all the hands to get the fire engine, force pump, and buckets ready, and commenced to throw water, pumping as hard as we were able, but found the fire gaining on us. At 5 a.m. I went aloft and looked for a vessel, and another man went up the mizzen rigging reported a vessel on the starboard quarter. The captain gave orders to square away down on her, which proved to be the American barque "Taria Topan," bound to the Mauritius. The captain spoke the vessel and reported to be on fire. Asked the captain to come on board which he did, and proposed staying by us to try to fetch the vessel to the nearest port, then the captain of the American barque returned to his vessel. The fire was gaining on us, and towards noon could hardly see on account of the smoke. The lower deck and the tank casing were on fire. We tried our best to extinguish the fire, but uselessly. The captain encouraged us, and did all in his power to save the ship. At about 4 p.m. the captain gave orders to haul the main sail and cross-jack up; the fire smoke being severe on our eyes, could not stop to obey the order. The captain gave orders to clear away the boats. The "Taria Topan" shortened sail and laid close to us to receive us on board. The captain ordered me in the first boat, with a note to the captain of the "Taria Topan" to tell him we could not stay longer, the fire was gaining so rapidly on us. The four boys, three men, and myself were ordered to go on board the "Taria Topan." Got alongside the vessel, put the boys on board and ship's chronometer, came back to our vessel to render any more assistance, brought back some provisions on board the "Taria Topan." Got orders to come back for a third trip; came alongside; flames were bursting out of the hatches. The long boat was sent away with part crew and provisions, but on account of the heavy sea and wind freshening, the captain gave me the orders to take the long boat in tow, but did not succeed on account of the heavy swell; got alongside the "Taria Topan," hooked our boat on to the hatches, took her on board, came on the poop of the "Taria Topan," discovered the flames flying up the mast and the rigging of the "St. Mungo;" it was 6 p.m. The "Taria Topan's" boat went back and fetched Captain Broom and the mate on board. After 10 p.m., the ship being in a mass of flames, took the glass; could only discern the mizzen mast standing and flames running out towards

the bowsprit, and afterwards the "Taria Topan" squared away. We were well treated on board the American barque "Taria Topan," and I return my sincere thanks for the same.

William Ross, steward, George Wilkie, carpenter, Charles Reilly, A.B., John Miller, A.B., all on oath.

Swore to the correctness of the captain's entry in the official log-book, which was read to them, and on their being asked whether they were kindly treated on board the American barque "Taria Topan," they all said that they wished to express their gratefulness at the manner in which they were treated on board the said vessel.

Upon their being asked whether the captain and officers of their ship did everything in their power to save the vessel, they all answered in the affirmative.

Captain W. H. Hathorne was called in:

I am the master of the American barque "Taria Topan" of Salem, Mass. At 5.30 a.m., on Sunday, the 14th September ultimo, I sighted the ship "St. Mungo" with signals of distress. I bore down upon her; went on board and found that the vessel was on fire. Agreed with the captain of the ship to remain by the vessel until such time as my assistance will be needed, but found, in spite of the efforts of the captain, officers, and men of the ship "St. Mungo," that the vessel could not be saved. I returned to my vessel. About 10.30 p.m., the ship "St Mungo" presented a mass of flames, the whole of the crew having come on board the "Taria Topan," (31 in all,) I continued my route to Mauritius, the port I was bound to, and arrived on Friday, the 17th October instant.

The Board thanked Captain W. H. Hathorne for his conduct on the occasion, and promised that he will be favourably mentioned in their Report to the Board of Trade.

Report.

The "Saint Mungo," of 1,375 tons register, was built of iron, under special superintendence, at Hull in 1865, and classed at Lloyds A1, 20 years. Her length was 223, breadth 36.5, depth 23.4; owned by Messrs. Gilmour, Rankin, & Co. of Glasgow; registered at the same port, and commanded by Mr. Frederick Broom, who holds a certificate of competency as master, No. 26,933. At the time of her abandonment she was on a voyage from Dundee to Point de Galle, with a cargo of 1,800 tons of coals. Her crew consisted of thirty hands, all told.

On Wednesday, the 9th July, at 5 p.m., she left the Tay, her voyage being prosperous up to Saturday, September the 13th, when, at 8.15 a.m., the steward, William Ross, saw smoke coming out of the after ventilator, and reported the same to the master, who was then at breakfast; he immediately went on deck and saw a slight issue of smoke from the whole of the ventilators, there being nine in number. He then ordered the combustible stores, such as tar, varnish, oil, &c., that were below, to be brought on deck; he, in the meantime, went below, accompanied by one of the crew, to see if he could discover the position of the fire, but the smell of the gas was so intense that they had great difficulty in regaining the deck. The captain and chief officer then went down the main hatch ventilator in the lower hold to see if the fire was on the skin, but found everything cool from the keelson up to about 6 feet; they concluded from this inspection that the fire was in the body of the vessel. The hatches and other openings were then ordered to be securely battened down, in hopes of smothering the fire, and after a time, the deck and hatches appearing cool, one of the ventilators was opened to allow the foul air, if any, to escape. After this precaution, the captain thought there was no fear of an explosion. At midnight there was no appearance of fire, but at 3 a.m., the fore, main, and after hatches blew off with a terrific explosion, the flame at the same time extending half-way up the mainmast, and dense volumes of smoke issuing from the whole of the hatches; immediately all hands were set to work drawing water and pumping with both force pumps down the main hatch; holes were also cut in the deck to allow the water to be poured freely over the burning cargo. At this critical time a vessel was sighted to leeward. The "St. Mungo" was then kept away and bore down on her. She proved to be the American barque "Taria Topan" from Boston, U.S., bound to this port with a general cargo. She was hailed and requested to remain by them, which the American captain readily promised to do. He then went on board the "Saint Mungo" and saw the exact state of the vessel. They agreed upon signals in case of immediate need, the captain still hoping to get his vessel into the Cape of Good Hope. The crew promised to do all in their power if the American barque would remain by them. Every exertion was made during the day to get the fire under, but without success. At 3 p.m. the fire burnt through the deck and rapidly spread over the vessel. Seeing that it was necessary for the preservation of life to abandon the vessel, the "Taria Topan" was then signalled to come near; the boats were then lowered and the vessel finally abandoned in lat. 35° 5′ S., long. 20° 40′ W. Table Bay, Cape of Good Hope, then bearing E. by N. ½ N., distance 2,060 miles. When the boats were in the act of leaving the ill-fated "Saint Mungo" a large ship passed close to but did not take the slightest notice of them, but held on her course to the S.E. Two days after the ship "A. L. Louis" of London, was spoken and told by Captain Hathorne that he had 30 men, the crew of an English ship, on board, and requested that as his vessel was small and having a long passage before him, that he would relieve him of part of the crew. In reply he cooly told him to put into the Cape and land them. It is unfortunately but too true that vessels in distress are frequently passed unnoticed by those in good condition.

Although it would appear, from the number of ventilators, that this ship was well and properly ventilated, but in reality she was not, as only three of the ventilators went to the bottom of the vessel, the others simply ventilating the between decks. The proper way to ventilate a vessel for coal is to have horizontal ventilators, running the length of the vessel under the between deck beams, and on the skin in the lower hold; those ventilators should be attached to the perpendicular ventilators from the deck. There can be no doubt that the explosion was due to the inflammable gases which had been accumulating among the coals and which the ventilators were unable to remove.

The Board, after hearing the evidence of the witnesses, which being the same in every respect, and was corroborated by Captain Hathorne, is of opinion that the abandonment of the "Saint Mungo" was necessary, and that the master, officers, and crew only adopted that measure when all chance of saving the property entrusted to their charge was entirely done away with; that no blame whatsoever can be attached to them, and have therefore much pleasure in returning the captain, chief, and second officers their certificates.

It now becomes the duty of the Board of recording the generous and hospitable treatment received by the master, officers, and crew of the "St. Mungo" from Captain W. H. Hathorne, of the American barque "Taria Topan" to whom honourable and ample testimony was borne. This is the second time in less than five weeks that British seamen have been rescued and brought to this port when in distress by vessels of other nations, the Board ventures to hope that the manly, honourable, and meritorious conduct of Captain William Hollingsworth Hathorne will meet with the favourable consideration of the Board of Trade.

John Morgan,
Acting Harbour Master, and
President of the Board.
John Cowin,
A. W. Barclay, } Marine Surveyors.
J. A. Anderson,
Of the barque "Lorton Vale."
W. H. Metcalf,
Of the ship "Beechardass Ambaidass."

"WAVERLEY," of Liverpool, of 999 tons register.

Inquiry held at Singapore, this 16th day of October 1873, into the loss of the British ship "Waverley," of Liverpool, official No. 45,453, 999$\frac{7}{10}$ tons burthen, bound from Shields to Singapore with a full cargo of coals, before the Hon. J. W. W. Birch, Police Magistrate, and Messrs. E. M. Smith and H. Ellis, Nautical Assessors.

Robert Calvert sworn, states:

I was master of the late British ship "Waverley," official No. 45,453. I left Shields on Friday, 27th June 1873, with a full cargo of coals, bound to Singapore. We proceeded all well until the 16th September, when we sighted the island of St. Paul's. On the following day we got a strong smell of gas. On examining we found neither smoke nor fire down below, but which we thought was caused by want of ventilation. On the 18th September, after removing the coals from the hatchway we found the smoke coming up, an explosion took place. We tried to put out the fire by battening down the hatches. The hatches were blown right off every two hours, and we found it impossible to batten them down. About 10 p.m. on the same day an explosion took place and blew the hatches up, and the flames reached nearly to the masthead; after which we hove to, put the boats out, and put provisions in them. On the following morning after daylight vast volumes of smoke issued from all hatchways, and we considered it time to leave the ship. We remained by her till she sunk the next morning, about 2 o'clock. We remained till sunset, when we were picked up by "President Van Ryckevorsel." I think the leaden pipes of the force pump got melted, and the water must have come in. On the 17th of September our position was, latitude 38° 23′ South and longitude 81° 37′ East; on 18th, latitude 38° 6′ South, and longitude 86° 45′ East; and on 19th, latitude 37° 39′ South, and longitude 88° 7′ East. On Saturday, the day we were picked up, our position was, latitude 37° 44′ South, and longitude 89° 34′ East. Our cargo was West Hartley coals. I had nothing whatever but coals which could have caused an explosion. When we left the ship it was blowing a strong breeze from north-west. Everything was quite correct three weeks before, when I had occasion to trim the coals. A fire occurred in the centre compartment, and it was impossible to get down here, as the coals were choked full to the top.

R. Calvert,
Master, late ship "Waverley."

Before us,
J. W. W. Birch, President,
Police Magistrate.
E. M. Smith,
H. Ellis, } Nautical Assessors.

William Cole sworn, states:

I was mate of the late ship "Waverley." I hold a certificate of competency as master, No. 5,032, from the Board of Trade. I lost my certificate in the ship. The vessel was lost. On 17th September we perceived a slight smell of gas. On the 18th we discovered smoke coming up the ventilators. On the night of the 17th all was shut up; the water in the tanks was quite cool. About 10 a.m., 18th September, an explosion took place. I never saw explosions of that sort before. We got provisions on deck and secured the hatches. On the evening of the 18th, about 10 o'clock, a heavy explosion took place and blew the hatches off. We got the boats off, and at daylight on the 19th, at 5.30, we left the ship. The vessel went down about 2 o'clock on the morning of the 20th. I think the melting of the pipes and brass around must have caused the vessel to go down so soon. I lost my log-book. I believe the ship's papers were saved. The fire broke out in the centre compartment. It was impossible to get down there, as the coals were up to the beams. We did pour water down on the main hatch, but gave it up as useless. I never saw explosions like this before. The explosions were accompanied by a noise like a steam whistle. All hands were saved. We were to the eastward of St. Paul's about 500 miles.

Wm. Cole,
First Mate, late ship "Waverley."

Before us,
J. W. W. Birch, President,
Police Magistrate.
E. M. Smith,
H. Ellis, } Nautical Assessors.

Patrick Miagha sworn, states:

I was second mate of the "Waverley." I have a certificate from the Board of Trade, No. 93,771, which I now produce. I recollect the day the gas commenced to escape, viz., on Wednesday, 17th September. We thought it might be foul air. At 6 a.m. the next morning we discovered the fire. Smoke was seen issuing from the ventilators, which I at once reported to the captain. I received orders to go down amongst the coals. I went to all parts of the ship I could get into, but found no signs of fire. In the afternoon we set to work to get provisions on deck, in order to batten down the hatches, thereby putting out the fire. We were prevented by the explosion, which blew off the hatches. We got the boats out during the night of the 18th, and at 5.30 a.m. on the 19th we took to the boats, but hung on to the ship. The vessel was hove to before we got the boats out. She was all in a flame; the masts fell, and she went down about two hours before daylight. A few hours before she went down the boats got parted. There was a heavy sea on. We were picked up the next evening. We poured water down the main hatch, but it did no good. The ventilators were taken down and the opening for them stopped up. At the latter part of the 18th the explosions became very frequent, and it was impossible to keep the hatches down. The explosion was something like the noise of steam passing through the safety valves. We sounded the pumps regularly all the way. I never saw the pumps used during the time I was in her. All was done to save the ship that could be done.

Patrick Miagha,
Second Officer, late ship "Waverley."

Before us,
J. W. W. Birch, President,
Police Magistrate.
E. M. Smith,
H. Ellis, } Nautical Assessors.

Robert Philip sworn, states:

I was carpenter of the "Waverley." I recollect the day she was lost. We first noticed the smoke issuing out of the ventilators. I examined it, but there was no sign of fire. I saw smoke coming out of the quarter hatch. The flame first showed itself that night at 10 p.m. I did sound the pumps, She did not make any water. The water in the tank was quite cool. The smoke came out of the after part of the main hatch. The hatches were battened down, but they were blown off by explosions. The explosions were not very strong. The one that went up the mainmast was like a rocket. I have been four years at sea. Everything was done to save the ship that could be done.

Robert Philip,
Carpenter, late ship "Waverley."

Before us,
J. W. W. Birch, President,
Police Magistrate.
E. M. Smith,
H. Ellis, } Nautical Assessors.

Hugh Hughes sworn, states:

I was boatswain of the late ship "Waverley." I recollect when the ship caught fire. We discovered the fire when we went down the after hatch. We got a slight smell of gas. The smoke was coming out from the centre compartment. After the smoke cleared away I went down, but did not go far, as the coal was chocked up to the beam. We put the hatches on, trying to smother the fire, but the explosions blew the hatches off. We had done all we could. We put some water down the after hatch, but it did not do much good. We left the ship at daybreak. It was not safe to stand by her any longer. The vessel burnt up, and in the morning we could not find her.

Questioned by the Master.

When I and some men went down to clear away the coals the smoke issued out. I did not stop there long.

Hugh Hughes,
Boatswain, late ship "Waverley."

Before us,
J. W. W. Birch, President,
Police Magistrate.
E. M. Smith,
H. Ellis, } Nautical Assessors.

We the undersigned seamen of the late ship "Waverley" consider that the burning of the ship was purely accidental, and everything was done that could be done.

Henry Watt, apprentice.
John Howell, A. B.
Charles × Brown, A. B.
Louis Olsen, his × mark, A. B.
Frank Murray, his × mark, A. B.
Raymond H. Coope, apprentice.
Edward Bell, apprentice.
Richard Dowman, apprentice.
James Low, A. B.
Wm. Johnson, his × mark, A. B.
F. Earp, his × mark, cook.
J. Grace, his × mark, A. B.
H. Carlson, his × mark, A. B.
Edw. Harrison, O. S.
Olof, Anderson, A. B.
Louis Pierre, his × mark, O. S.
Aug. Bloom, his × mark, sailmaker.
Walter S. Freeman, steward.

Decision.

The Court having inquired into the loss of the British ship "Waverley," of Liverpool, official number 45,453, finds that she was destroyed by fire at sea in or near lat. 37° 39′ S., long. 88° 7′ E.; that she foundered, and that all hands were saved. The Court considers that it would probably have been wiser to have used water a little more freely than appears to have been done; but looking to the weather and the situation of the ship so far from land, and to the constant explosions, they consider that no blame can properly attach to the master, and they are of opinion that his certificate should be returned to him, as well as to the first and second mates.

J. W. W. Birch, President,
Colonial Secretary and Police Magistrate.
E. M. Smith, } Nautical Assessors.
H. Ellis, }
Singapore, 16th October 1873.

"HERBERT GRAHAM," of Newport, Mon., wooden ship, of 337 tons register, six years old, classed A1, 13 years.

Town Hall, Newport, Tuesday, January 6th, 1874.
Coram:
Nelson Hewitson, Esq., Mayor,
Thomas Beynon, Esq.
Assisted by—
Captain H. Harris, }
W. Parker, Engineer, } Assessors.
Mr. Darley, Shipwright, }

Mr. Hamel appeared for the Board of Trade to conduct the case.

Inquiry as to the loss of the barque "Herbert Graham." Mr. Hamel opened the case.

Evan Roberts sworn, states:

I was late master of the "Herbert Graham," and held a certificate of competency as master, which certificate I lost during my travels in Patagonia. I joined the "Herbert Graham" on the 1st day of July last. She was laden, and ready to go out. We had a crew 10, all told. We had 536 tons of coal on board, and were bound for Valparaiso. I went into the hold several times on the voyage. In fore hold there was a large space between the coals and the deck, about 7 or 8 feet; the after hold was full. I never saw the vessel empty. We had a half-poop on deck rising 3 or 4 feet; the cabin occupied the whole of the poop. We ventilated the ship by means of the after and fore hatchways. These were continually kept open except in bad weather. The hatchways were, I think, about 8 by 6. We had fine weather all the voyage except for about 12 days off the River Plate, in the beginning of September. It was windy weather, which obliged us to close our hatches; so much water was going over that we were bound to close them. No complaints were made till the 19th of September by the crew of the gas. On that morning at eight bells the mate told me the watch had not come on deck. I went forward and saw three men lying on the forecastle floor unconscious. I could not get a word out of them. They had a stove fire in the forecastle at this time; it was cold weather. I got a rope and made it fast to the three men, and pulled up one at a time on to the deck and in about 20 minutes they came right and then they told me the forecastle was full of sulphur. I ordered the men to leave the forecastle, extinguish the fire, and clean it out. I sent them to a house in the break of the poop. The hatches fore and aft were open at this time. The men would not live in the house; it was too cold, they said, and they returned to the forecastle. This was the next day. I let them go. I heard no complaints. After that the forecastle was nicely cleared up, and they lighted the fire again. Nothing occurred till the 22nd of September. At 6 p.m. they reported (that is, one of the seamen did) that smoke was coming out of the fore hatch. Myself and the mate went to see what was the smoke, and we found very little, but ordered the hatches fore and aft to be battened down close; that was done, and in about an hour, more or less, they were opened again. When they were opened a second time the vessel was full of smoke, which cleared away in about 10 minutes. I went below to examine the hold, and found that the cargo of coal was on fire right amidships. I went on deck and ordered the vessel to be battened down close again, and changed the ship's course for the land. Smoke was escaping through the night from the tarpaulins on the hatchways, and the fore companion. Next morning my men were very anxious, and wanted me to put out the boats. At daylight in the morning (23 September) we erected two big davits, one on each quarter, to support the boats. It was not safe to leave them on the hatchways. They were put into the davits ready for lowering. In the morning continually the smoke was coming out from under the tarpaulins over the hatchways, and the deck was getting hot. At 9 a.m. we sighted the land, Port Santa Cruz, bearing W.N.W., distance about 35 miles. All sail, except the two royals, were set, and we were making for the land. At about 10 a.m. it became calm; then she had no way. It was a very fine day. I consulted with the mate, and we thought the best thing to do was to open the hatchways and discharge a part of the cargo. We opened them between 11 and 12 to see if we could extinguish the fire. When we did so, we found the vessel was full of smoke; we could hardly breathe in it on deck. There was a smell of timber burning in the hold. The smoke all cleared away in about 10 or 15 minutes, when the fire burst out from the fore and main hatch. The boats were at once lowered, and the men went into them, except the mate, boatswain, and myself. We remained on her poop for about an hour and a half. We were looking at the flames coming up. I went on the main deck once or twice, and the pitch was boiling out of the seams, so that I could not remain there. The men remained in the boats by the ship. We three got in at about half-past 1 or 2 o'clock of the 23rd. When we left the ship the flames were ascending 4 or 5 feet above the main hatch. I did not go on board again. We saved most part of our clothes and effects, and the log books, but we lost them, the men left them in the long boat. We did not remain. The wind came off the land in the afternoon. We pulled for the land, and I saw her masts, fore and main, come down at about 5 o'clock that afternoon. She burnt up till 3 o'clock of the morning of the 24th September, and then disappeared. The ship on the voyage was very tight, made very little water, and was pumped out morning and evening in two or three minutes. We landed on the 26t about 25 miles N.W. of Cape Virgin on the coast of Patagonia. In landing one man fell overboard into the surf from the long boat. We picked him up again, but he afterwards died from the cold and exposure. His name was John Rowley, able seaman, a native of Quebec. We buried him on the beach under a cliff: he was a healthy man. We proceeded in the boats about 54 miles by the shore, and on the morning of the 6th of October a large steamer passed us. We signalled to her; she took no notice. I could see people on the deck. I don't know whether they saw us. It was about 9 o'clock in the morning. Seeing our provisions were getting short we started overland to Sandy Point. We started nine in number till we came to a river; five of us crossed, and the other four said they would go back to the boats; they went; I waited two hours; they would not come with us. In six days we reached Sandy Point, walking most of that time. We met some natives about a mile from that place. We were very kindly treated by the Governor of that place. We had nothing but water the last three days of our journey; previously we had six biscuits each. From Wednesday till Sunday, the 12th, we had no food. We left there on the following Tuesday in one of the Royal Pacific Mail Steamboats for Valparaiso, and from there we were sent to Liverpool in another steamer. I can give no opinion as to the cause of the fire.

(*By Mr. Graham.*) We had two chronometers on board.

[The Court decided that Mr. Graham had no *locus standi.*]

(*By Captain Harris.*) There never was much water in her.

(*By Mr. Darley.*) We had first-class iron pumps; 9 inches of water in the well; the delivery pump about 6 inches, fitted with breaks. We had double breaks but never fitted them, as we never had occasion to do so. We first discovered the fire under the main hatchway. I could not find the particular spot of the fire. I could not bear my hand in the coals 6 inches down on the 22nd September. We had no ventilation but the after and fore hatches. The ship was in fine trim, draught 16 aft and 15 forward.

Evan Roberts.

George Farrer sworn:

I was mate of the "Herbert Graham." I hold a certificate of competency as mate. I joined her on the 1st of July, same day as the captain, the day she left. Her cargo was in. On the Friday before we saw the smoke the men complained of gas. We had been 81 days at sea; the weather was very hot. On the 19th September saw the men lying on the forecastle floor, and I helped to pull them up; they were all right in about 20 minutes. I saw the smoke coming from the hatches we battened down. I went with the captain to inspect; we found the cargo very hot. We dug in as far as we could with our arms; we felt it the hottest about midships, where was the bulk of the cargo. The hatches were battened down till the day after we saw the smoke. At about midday the smoke overpowered us; shortly after the flames broke out. We lowered the boat and prepared to get to land, finding we could do no good. I and three others left the captain at the river; we returned to our boats, and seeing a steamer, the "Cuzco," we pulled to her. She took us on board, waited for us about half an hour, and landed us at Liverpool. I never smelled any gas from the coal. I could not smell sulphur. In the hold the smell caught our breath. It was the smell like the burning of oak timber. During the voyage we put no water down. We did not use shovels.

(*By Mr. Darley.*) When we first saw the smoke we did not think the coal was on fire. Our vessel had about 5 feet freeboard. Our pumps were fixed in.

G. Farrer.

William Roberts sworn:

I was an able seaman on board the "Herbert Graham." I joined her two or three days before she sailed. She was in the river at Newport. Cargo in. I lived in the forecastle. I was on deck and saw the men lying in the forecastle. Me and the captain went down. Smelt of gas; it made me sick. I could not stay long there. We got the men upon deck and cleared out clean of the forecastle. We returned to the forecastle, but did not smell the gas again till she caught fire. I thought it was the smell from the store. It was like a smell of sulphur; we could not remain in the forecastle. On Sunday night when the flames came up we left the forecastle, about 8.30, and battened everything down. We had to take to the boats.

(*By Captain Harris.*) The stove we had in the forecastle had a funnel through the deck.

his
William × Roberts
mark.

William James sworn:

I was formerly master of the "Herbert Graham," and was formerly part owner of her. I joined her as master when she was on the stocks in the builder's yard at Scilly. She was on the stocks built when we bought her. On the 1st of July, the day she sailed, I gave up the command to Captain Roberts. I was by her when she was being loaded. She had discharged a cargo of iron ore from Spain. On the 20th of June, I took in stiffening about 70 tons, finishing on the 23rd June. This was in the lower Newport dock. We then went to the upper and took in the remainder of the cargo of coal. We were loading two days, Wednesday and Thursday. I told the trimmers to leave a space aft and forward, but fill her up midships at the main hatch. She left her loading berth on Friday, and we hauled her down to the lower dock again, near the lock. She remained there. I took-in stores. I hauled her out into the lock of the dock on the 1st July, and then gave up the command. I had a cargo of coal on board this vessel once before. I weighed out then 139 tons in Japan. I had the same ventilation then as afterwards—the hatches; that is the only ventilation we had.

(*By Mr. Beynon.*) I think it was raining when we took in the stiffening. I never remember any cargo of house coals shipped at Newport taking fire before.

(*By the Mayor.*) With steam coal it is usual to have shutes as a means of ventilation, but this coal on board the "Herbert Graham" was house coal.

(*By Mr. Darley.*) I think the shutes are quite useless as ventilators. The "Herbert Graham" was insured for 4,000*l*. I value her at 4,800*l*. She was classed 13 years at Lloyds. No force pump was on board.

(*By Mr. Parker.*) We had no provision on board for extinguishing fire.

Wm. James.

Thomas Edwards sworn, states:

I am a shipbuilder of St. Mary, Scilly. The "Herbert Graham" was built in my yard under a special survey of Lloyds. She was classed A1, 13 years. She was built of English oak frame and upper deck beams. Main keel English elm. I built her as a merchant vessel to carry cargo. She had three hatches on the main deck, air courses fore and aft, and under the hold beams was the ventilation. No ventilation shaft to the ship.

(*By Captain Harris.*) We always put chocks between the frames, but they are taken out before the vessel is planked.

(*By Mr. Darley.*) The ventilation is to prevent dry rot, and not for ventilating a cargo of coals. It was understood by Lloyds surveyor that the "Herbert Graham" was being built for a general cargo. All vessels that I have built generally take coal as a first cargo. I never heard of one taking fire before. Lloyds surveyor never called my attention to the importance of ventilation in the ship. I never heard of a valve or a cock to plug the vessel if required in the event of fire. I would recommend such a pipe to be fitted under water.

Thomas Edwards.

James Barber sworn, states:

I am foreman coal-shipper under Messrs. Webb, Spittle, and Bladon. They supplied the coal to the "Herbert Graham." I superintended the shipping of the coal. I saw the stiffening put in. The loading of that began on the Saturday. It was put in in the old dock, laden in the open, not under cover; it was brought in trucks. We put about 10 trucks (50 or 60 tons) on the Saturday, seven trucks being left; they stood outside. It rained on the Saturday while the stiffening was being put on board, and it rained up till midnight on the Saturday night. The men knocked off work because of the rain. On Monday morning it was dry, and the men got to work again, and put in the seven trucks that had been left out. Then the ship went to the other dock to take in the remainder of her cargo. I superintended the shipping of that. The cargo was dry. It came direct from the colliery in trucks. The first lot came on Monday morning, and more coal arrived till she was filled up; it was perfectly dry weather while she was loading her cargo. I was in the hold twice or three times while she was loading; I was not in the hold after the cargo was completed. The coal shipped was of three different qualities. The stiffening was all brush coal; it came principally from the Mamhole Colliery; brush coal is next to large in quality. Small coal came from Coalbrooke Vale and some from Cwmserchan. The large coal was from Cwm Colliery at Aberbeeg. I can tell the coal one from each other on seeing them. The two pieces produced are Cwm Coal. This piece is the Crumlin coal from Pant Glas Colliery. Most of the coal put on board was brush coal. I do not know the quantity of Cwm coal put on board. The piece of coal now produced with a stain upon it is Crumlin coal from a lower vein from that we work at present. The customers did not like the stain in it, and therefore, we work the other. The stiffening was a mixture of small and round coal.

James Barber.

Patrick Donovan sworn, states:

I am a coal trimmer. I trimmed the coals on board the "Herbert Graham," and helped to stow them; the coal was dry and I could see no fault in it. It was fine as we loaded, could not be finer. I trimmed her all right. She was full in the main hatch and after hatch, but not forward. I was not present when the stiffening was put in.

his
Patrick × Donovan.
mark.

William Kirton Quest sworn, states:

I am deputy dockmaster of the Newport Dock Company. We keep a record of the wind, &c. The weather was fine during the last week of June and up to the 3rd of July. I know it was raining when they took in the stiffening of the "Herbert Graham." I was going past, and the tarpaulins not being properly placed to prevent coal falling into the dock, I complained to them about it, and I distinctly remember it was raining hard at the time. When she left the locks she was reported 16 feet draught of water.

W. K. Quest.

Adolph Pierre Vassard sworn, states:

I am a consulting chemist in London. I have had submitted to me some samples of coal similar to that carried by the "Herbert Graham," and I have analysed them. The result of my analysis is contained in the written report produced.

Report read as follows:—

Re "Herbert Graham."

A sample of the coal carried by this ship at the time she was burnt sent to me under the seal of "Searchers Customs, Newport," has been analysed and otherwise examined by me according to instructions. I found the coal highly pyriteous and very brittle.

It contains per 100 parts—

Moisture	0·35
Hydrogen and nitrogen	9·10
Carbon	81·00
Mineral ash	5·40
Iron pyrites	3·04
Divers and loss	1·11
	100·00

Though the sample I received was in three lumps, I am warranted, considering the general practice of the coal trade with the Pacific Coast and the brittleness of the coal under examination, to say that it was shipped small, or became such by the loading. Small coal absorbs easily any amount of moisture.

The quantity of iron pyrites is very large. Iron pyrites are a bisulphuret of iron, that is to say, a compound of iron and sulphur. Over three per cent. of iron pyrites, as shown by analysis, would give, if allowed to naturally decompose, nearly one per cent. of free sulphur. To the presence of so large a quantity of pyrites I attribute the burning of the 'Herbert Graham."

When bisulphuret of iron or iron pyrites is left when wet to the action of the air, or to the action of a moist atmosphere, it gradually undergoes a chemical change; one part of the sulphur is oxidised partly by the *oxygen* of the air and partly by *that* of the decomposing *water*, and becomes *sulphuric acid.* Thereby the other part of the sulphur is disengaged from the pyrites, and also hydrogen from the water. This sulphur and the then free hydrogen are thus thrown in presence of each other, combine, and form *hydrosulphuric acid* or *sulphuretted hydrogen.*

These chemical reactions produce a great heat, whereby the sulphuretted hydrogen is ignited and therefore oxidised, producing *sulphurous acid* and *free sulphur,* which is also ultimately burned.

It may then be easily understood that if a combustible material like coal, disengaging already inflammable gases through its continually going on decay, is found mixed with the chemical bodies producing the above reactions and effects, this combustible will be heated and ultimately ignited.

Such is the chemical explanation of what is called "spontaneous combustion," and of what undoubtedly took place in the hold of the "Herbert Graham."

If the coal had been shipped completely dry, and so kept all the while, no spontaneous combustion could happen.

A. VASSARD.

I was present in court during the examination of the witnesses, and I heard Mr. Barber, the foreman coal shipper, state the different kinds of coal shipped on board the "Herbert Graham." I have no doubt that the stiffening about 70 tons of coal put in during wet weather would moisten the whole cargo in any circumstances; in three or four days it would moisten the cargo. It further appears that the whole of the cargo was shipped direct from the colliery, and would contain a large quantity of moisture, more than three per cent. It is the same with all coal except large coal. Three per cent. of water would be sufficient in time to ignite the cargo if the coal remained in bulk undisturbed, and would ultimately take fire. There is no danger with large coal, but with small coal there is. I think the chemical composition of the coal explains the fire on board the "Herbert Graham." About half a ton of sulphur would produce a sufficient quantity of sulphurous acid to extinguish a fire in 1,000 tons of coal on board ship. To apply this a pipe would have to be fitted to the ship and carried under the cargo. I would have a small stove on deck, and when a fire has been lighted in it throw sulphur on the fire. Sulphurous acid would then arise and be carried down the pipe attached to the stove into the vessel's hold. In five or ten minutes that would extinguish the fire. Even if the pipe was containing a little water the acid would still escape. It would not be necessary to have the hatches down when the process is going on. The sulphurous acid is twice as heavy as air. A small air pump might be necessary to force the sulphurous acid through the water covering the pipe. The acid would not be dangerous to life, through being oxidised before reaching the deck. I doubt whether ventilating the cargo with shafts when loading would have any effect unless the coal was shipped perfectly dry. I don't think the shafts are of any use except to prevent the explosion of gas. If water was used to extinguish the fire, the cargo would have to be covered all at once; a small quantity would be of no use; this would facilitate combustion. Captains of ships ought to be provided with thermometers, and when they find the coal is heating the application of sulphurous acid would check further combustion. The sulphuretted hydrogen produced by the decomposition of the pyrites would ignite between 170° and 173° Fahr. The ordinary temperature even down to freezing point is sufficient to make the pyrites decompose, providing water and air be present. I mixed the whole five samples of coal from the "Herbert Graham" together, and on analysis found the mixture contained over three per cent. of pyrites. I should not be surprised if one of the samples contained as much as 10 per cent. of pyrites. I do not think washing the coal would have prevented ignition. The washing of coal is based on the difference of specific gravity between iron pyrites and coal, and can only be applied to small coal. I never saw it done. I never heard of it till lately. The pyrites will take fire sooner if the temperature is warm.

By Mr. Darley.—Can you explain why the introduction of wood trunks and louvre boards amidships placed against the stanchions and other means of ventilation produce a good effect,—produce a good effect in one case and are injurious in another?

Answer.—All depends on the nature of the coal; with coal containing no pyrites, therefore not liable to spontaneously ignite, ventilation has a good effect in carrying away the gas, but the contrary is the case with coal containing pyrites, and therefore liable to spontaneous combustion. In the last case a good reason can only be possible if the coal is shipped dry and so kept all the voyage. I think testing tubes should be fitted to ascertain the temperature of the coal, which should be ascertained every day. I think if testing tubes had been fitted on board the "Herbert Graham," they might have ascertained the existence of fire earlier and discharged the cargo. Testing tubes could not be used to send the gas down. It would be better that coals should not be shipped direct from the pit; but this would be quite impossible.

A. VASSARD.

Report.

Report of a Court of Inquiry held at the Town Hall in Newport, Monmouthshire, by order of the Board of Trade, before Nelson Hewertson, Esq., Mayor, and Thomas Benyon, Esq., J.P., with Captain Harris as Nautical, and Messrs. Parker and Darley as Engineer and Shipwright Assessors, into the circumstances attending the burning of the barque "Herbert Graham," which occurred on the morning of the 24th day of September last, near the entrance of the Straits of Magellan.

The "Herbert Graham" was a barque, built of wood at St. Mary's Scilly, in the year 1869, and registered at this port in the same year, of the burden of $337\frac{60}{100}$ tons. She was owned by Robert Graham and others of this port, and on this present voyage was commanded by Mr. Evan Roberts, who held a certificate of competency as master, which was lost after the destruction of the ship. The "Herbert Graham" left Newport on the first of July 1873, manned by a crew of 10 hands all told. She was laden with a cargo of 536 tons of coal, bound for Valparaiso. Nothing worthy of remark occurred till the 19th of September, when three of the crew were found in the forecastle in a state of stupor. They were removed to the upper deck, when in about 20 minutes they recovered their senses, and stated that the forecastle was full of sulphur, which they attributed to the presence of a close stove which was allowed in the forecastle, the weather then being very cold. They were removed to a house aft, and the fire in the stove put out. The following day they returned to the forecastle, and no further complaints were heard. On the morning of the 22d smoke was discovered issuing from the fore hatch, which was battened down. In about an hour the hatch was taken off, and it was discovered that the ship was full of smoke, which, clearing off in about 10 minutes, enabled the master to go into the hold. Upon descending he found that the cargo was on fire amidships. All the hatches were closely battened down. and the ship's course directed towards the land. The following morning they sighted the land in the vicinity of Port Santa Cruz, but falling calm, after consultation it was decided to open the hatches and throw overboard part of the cargo, in the hopes of reaching the fire. In about 10 minutes after the hatches had been taken off flames burst up from both fore and main holds. In this state of things, and being powerless to control the flames, the crew were sent into the boats, the master, mate, and boatswain remaining on board till 2 p.m. of the 23rd, whan, for their own safety, they were obliged to abandon the vessel and pull towards the shore. At 3 a.m. on the following day they saw the vessel disappear. On the 26th they landed near Cape Virgin, on the coast of Patagonia. In landing one of the crew fell overboard from the effects of which and exposure on shore he died, and was there interred. From this they proceeded in their boats about 50 miles up the Straits of Magellan, but finding the wind against them, and their provisions getting low, they determined to walk to Sandy Point, a penal settlement of the Chilian Government. Five of the crew accomplished this after great suffering from exposure and hunger, and were kindly treated by the authorities at Sandy Point, who forwarded them home. The other four who separated from their companions preferred proceeding along the coast in their boat, and falling in with a passing steamer, were taken on board and conveyed to England. The captain and his crew seemed to have done all in their power, under the circumstances in which they were placed, and only abandoned the ship for the preservation of their lives. The main object of this inquiry was to ascertain the cause of this casualty, and, if possible, to devise some means of preventing similar accidents in future. From the evidence it would appear that five different descriptions of coal, taken from the vicinity of Newport, were shipped on board the "Herbert Graham." About 70 tons of this coal was put on board as ballast in rainy weather, and the vessel was removed to another dock to complete her cargo, during which time the weather was fine. These coals were brought direct from the colliery, and would therefore be more or less damp, according to the evidence of M. Vassard, who was examined as an expert on this inquiry. This gentleman analysed three samples of coal, similar to that forming part of the cargo of "Herbert Graham," and found that they contained three per cent. of iron pyrites, which would decompose through moisture and exposure to the atmosphere, and thus become sulphate of iron and sulphuretted hydrogen. This sulphuretted hydrogen, under the influence of the heat produced by the above reaction and the atmosphere, would ignite and cause spontaneous combustion. And these coals having been shipped in a moist state, and containing a large amount of pyrites, would, in his opinion, be alone sufficient to account for this casualty. With these facts before us, it becomes necessary, if possible, to seek some means of prevention. Several plans were discussed by means of ventilation on the one hand and of water on the other. It was found, however, upon investigation, that in accordance with the above hypothesis the introduction of either water or air would have a precisely opposite effect. It further appeared, from the evidence of M. Vassard, that if sulphurous acid could be circulated through the cargo it would prevent spontaneous ignition. This acid could be obtained by burning sulphur, 10 cwt. of which would be sufficient for a cargo of 1,000 tons of coal. It is a matter of after consideration how this may be applied, either on the principle of the fire annihilator or otherwise. This method seemed the most practical that was offered, and, in the opinion of the Court, might be usefully applied to vessels carrying coals and bound on distant voyages. In offering this opinion, however, the Court desire it to be understood that they do not consider the general question exhausted with reference to the preventing the recurrence of casualties of this description.

Given under our hands this 8th day of January 1874.

NELSON HEWERTSON,
THOS. BEYNON,
Justices of the Peace.

We concur in this report—

HY. HARRIS,
WM. DARLEY,
WM. PARKER,
Assessors.

"MINNIE GRAHAM," of Newport (Mon.), barque of 273 tons register, five years old.

Town Hall, Newport (Mon.),
Friday, March 13th, 1874.

Coram :

NELSON HEWERTSON, Esq., Mayor.
THOMAS BEYNON, Esq.

Assisted by :

Captain HARRIS, and } Nautical assessors.
FINTON WAKE, Esq. }

Inquiry directed by the Board of Trade as to the circumstances attending the abandonment of the "Minnie Graham."

Mr. Hamel appeared for the Board of Trade to conduct the case.

Case opened by him, and calls—

ROBERT BALE sworn, states :

I am a seaman, and shipped as boatswain acting second mate on board the "Minnie Graham." I shipped on the 16th July, and sailed the 17th from Newport. We had a crew of ten all told and a full cargo of coals. I noticed the coal was shipped wet. I saw a few trucks that was wet. It was mixed coal, large and small. We were bound for Valparaiso. We had two main pumps on board abaft the main mast. We had four small ventilators coming through the mooring post. They were cast iron and came through the covering board. I could get my arm into them. They went straight up and down, and could be plugged at the top. The plugs were out when it was fine weather and put in when it was rough. We had three good sized hatches, ordinary size; they were always off on the voyage when the weather permitted. Not the main hatch but the fore and aft ones. Always a wash of water across the main hatch; that was not so in fine weather. We never raised the main hatch in fine weather. We had a long boat shipped across the main hatch, and it was a good deal of trouble to get off. We had fine weather till we got off the Plate. She rolled easily. The wash I refer to came over the covering board. I believe the mate saved the log-book. Three or four days before we first saw smoke there was a strong smell of gas in the forecastle and also in the cabin. Nothing happened before this. We had an average voyage. The escape of gas was reported to the captain. He came to see what was the matter. He went into the forecabin and also in the hold. After that the hatches which had been opened were battened down. It was cold weather at the time. We were the other side of Cape Horn at the time. We were not in sight of land. Soon after the hatches were battened down we could not stay in the forecastle for smoke. The hatches were kept on till we saw the ship that took us off. It might be four days. The ship was the "Naval Reserve." We signalled her. She put out her boats, came alongside. Our boat was taken off the hatches after she boarded us. We had two boats. The small one was got out before the "Naval Reserve" boarded us. We had not provisioned the boats. The captain and carpenter of that ship came on board. They passed a survey on the ship and the main hatches were taken off. I dug away the coal under the main hatchway by direction of the two masters. We found the coal so hot just below the surface that I could hardly hold it in my hand. I dug down about two feet. Smoke was coming up as much as I could bear. The iron stanchions from the deck beam to the hold were hot, very hot; it was as much as I could do to hold them. The captain was down in the main hold and in the fore hold. I went into the fore hold and found the coals there just the same. I saw no fire. I poured in four or five buckets of water into the hole that I dug under the main hatch, but it would not penetrate, it stopped on the top. I did this to see if any steam would come up. Our captain, the captain of the "Naval Reserve," and the carpenter held a consultation (our mate was sick in bed), and they determined it was not safe to remain on board. We could not tell but what in any minute she might blow up. The ship was abandoned by us all. The carpenter of the "Naval Reserve" bored a hole just underneath the cabin deck to sink her. We were taken off in the "Naval Reserve" boats. I left in our own little boats; it was fine weather. I saved some of my clothes, just as much as I could scramble up. It was not safe to take a light below. It was in the evening when we left. Our long boat got stove. Our small boat was got on to the "Naval Reserve's" deck. It was fine weather, with whole sail breeze. I don't know how the wind was. She had not gone down about 10 o'clock that night. It was about 8 when we got the boats in. The "Naval Reserve" landed us at Callao.

We could not see land when we abandoned our ship. We altered the ship's course a point or two in towards the land before we saw the "Naval Reserve."

(*By Captain Harris.*) I think we had observed the smell of gas three or four days before we saw smoke.

(*By the Mayor.*) About four days after we saw the smoke we abandoned the ship. It was, I think, about eight days after we first smelt gas that we left her.

(*By Captain Harris.*) Our forecastle was close bulkheaded and the smoke came through the nicks. When under the line we felt no heat from the cargo. We went right round the Horn. The mate stopped at Callao; he had been ill a few days. During the voyage I am sure no water was thrown down into the hold.

(*By the Master of the Minnie Graham.*) You went off in the small boat to fetch the master of the "Naval Reserve," and the captain and carpenter of that vessel returned with you?—The captain of the "Naval Reserve" considered it unsafe to remain on board; he returned in our boat and sent his own life-boat to our assistance.

his
ROBERT × BALE.
mark.

EDWIN ELLIOT sworn, states :

I was cook and steward of the "Minnie Graham" on her last voyage. I joined her at Newport on the 16th, and she sailed on the 17th July last. I lived in the house on deck. I did not see the cargo shipped. On the voyage the fore and aft hatches were generally kept off in fine weather, that is, left off by day, we put them on at night; the four ventilators which were right aft and led on through the lazarette in the hold were metal pipes (I think lead), were kept open day and night in fine weather. In the month of October, four or five days before we left the ship, we smelt a strong smell of gas in the forecastle; the hatches had been off the day before; they were immediately taken off again; we then saw smoke, it came up in small curls at first; it gradually increased. The captain was called to see it; the men said they could not live in the forecastle; he went into the fore and after hold. I think the hatches were battened down after this till the Saturday before we left her. I suffered from the gas; I was ill from it; it laid me up. It came into the deck-house through the lazarette house just inside the door; I was bad all day, and could not observe what was going on. We caulked the lazarette-hatch down, and slept that night in the house as usual. We were rescued on Sunday. I was a little better that morning, but cannot say what happened. We saw the "Naval Reserve" just after dinner. The eboats were being got ready when she hove in sight with a view to abandoning the ship. We signalled her, and as soon as she got alongside our captain went on board in his jolly boat, and brought back the captain of the "Naval Reserve" and the carpenter; they examined our ship, took off the hatches, and smoke arose, a good deal, and gas. I saw Bale put several buckets of water down the hold. The captain of the "Naval Reserve" said it was not safe to remain on board; he was on board about a quarter of an hour before he said this. We were all called aft and asked what we thought we had better do. We said we had better leave her. He said it was not safe to remain on board as we did not know the moment she might blow up. The captain and carpenter returned to their ship and sent their life-boat to take us off. I saw the carpenter of the "Naval Reserve" go below into the lazarette with an augur in his hand, and I was told he bored a hole in her. One of the crew said so. The gas that made me ill appeared to smell like paraffin; it made me giddy and sick. I could not eat nor lift my head up. It made me rather short of breath and cough a good deal. I lost best part of my clothes. I was too ill to pick them up. We used an oil lamp in the deck-house.

EDWIN ELLIOTT.

GEORGE HARDY sworn, states:

I was an able seaman of the "Minnie Graham." I joined her the day before she sailed at Newport. I had been five years in a pilot boat, and this was my second foreign voyage. I lived in the forecastle. About three days before we left her (we left her on Sunday, the 26th of October), I noticed gas and smoke in the forecastle. It smelled like paraffin upset. It got worse and worse; it drove us on deck the night before we left her. There was no heat, but the forecastle got full of smoke. We could see it. It was reported to the captain, and he searched in the hold. He was driven back. Then we battened down the aft hatch, but the fore hatch was kept open, and the smoke escaped through it. The smoke got worse. We knew the ship was on fire; the stanchions were hot. So on Sunday we got the boats ready in the morning before the "Naval Reserve" hove in sight. We signalled to her, the ensign union down. She came to our assistance, and the captain and carpenter came on board, examined our ship, and we were called aft, and the captain of the "Naval Reserve" asked our opinion. We said we thought the ship was on fire. He said the same and we had best abandon her, as she might blow up or break out on us, and so we all left her. I saved most of my clothes; I went down into the forecastle for them. It was not quite so thick but what I could see my way to find them. We had thoughts of abandoning her before we saw the "Naval Reserve." We worked the pumps during the voyage. It took about five minutes to pump her out with one pump. The pumps were not choked. We were never more than 10 minutes at the pumps, and that was after a breeze.

GEORGE HARDY.

WILLIAM THOMPSON sworn, states:

I am an able seaman of the "Minnie Graham." I was the first to discover smoke on board. It was in the forecastle. When I got up in the morning I could hardly breathe. It was about eight bells. My head was like to split. I found gas and smoke in the forecastle. It smelt like paraffin. I reported this to the captain. I was at the wheel at the time. I went to the hold the second and third day after, and dug about two feet into the coals, and found plenty of gas and smoke to choke me. I came away, fearing I would be blown up altogether. We prepared to leave the vessel before we saw the "Naval Reserve." This was the third day after, (Sunday morning). That day was the first day the main hatch was taken off. The captain and carpenter of the "Naval Reserve" came on board, but I don't know what they did, as I was busy. The end of it all was we left her. We were called aft and our captain spoke to us. He asked what we thought of it. We said the ship was not in such a condition as we could remain on board of her. I was in the small boat which brought the captain and carpenter of the "Naval Reserve" to our ship, and remained in the boat. I have been 12 months at sea, foreign, having previously served five years' apprenticeship in a pilot boat.

By Captain Evans.

I made a mistake. Both captains were there when we were called aft.

WILLIAM THOMPSON.

WILLIAM COLLINS sworn, states:

I am a stevedore, and was in the hold helping to load the "Minnie Graham" with her cargo of coal. It was in the beginning of July last. I was with her the whole time she was loading. She had no stiffening on board; she did not want it. We put in small coal at first, slack; it was quite wet when we put in. It adhered to the trucks, it was so wet. We began with small wet coal. We put in perhaps about 40 or 50 tons of this, and then commenced with large coal. That was wet also. It was heavy rain the previous night, and the trucks were standing in it. I was paid for the cargo, 400 and some odd tons. She was full amidships. There was a good deal of room left fore and aft. We could have got 70 or 80 more tons in her. We were told not to let the coal go too far aft upon some wood, very inflammable. If you put a candle to it or against the side of the ship, it caught fire.

his
WILLIAM × COLLINS.
mark.

TOM JONES sworn, states:

I am traffic manager for Messrs. Powell. Coals from their two pits were put on board the "Minnie Graham." The cargo consisted of 60 tons 3 cwt. of Hope large coal, 20 tons 6 cwt. of Hope brush, 323 tons 1 cwt. of Tyrfilkin brush coal. No slack was mixed with the brush coal. The coal was fresh worked from the colliery when it was shipped. Some of the coal was in the waggons before being shipped 48 hours. That lot was 49 tons 9 cwt. The next lot was 91 tons 1 cwt. in the trucks, 30 hours; next lot 85 tons 8 cwt., in the trucks 24 hours; and 60 tons, in the trucks 24 hours; next lot 54 tons 11 cwt., in the trucks 54 hours; and next 59 tons 11 cwt., in the trucks 30 hours. When we commenced to load the vessel it was dry weather. Part of the cargo was out on Sunday and shipped Monday morning. The dates of the shipping were 11th, 12th, 14th, and 15th of July. The piece of coal produced I believe is Hope coal.

TOM JONES.

Adjourned till to-morrow morning, at 9 o'clock.

JOHN COOK sworn:

I am a shipbuilder. I have built many ships. With regard to ventilation, I believe there should be trunks laid fore and aft and ventilators up to the deck, trunks like a shoot. I produce a sketch I have made. (*The Court examined the sketch, and witness pointed out how the trunks should be placed.*)

JOHN COOKE,
Shipbuilder.

The Captain WILLIAM EVANS here put in his written statement.

ALFRED EDWARD HAYNES sworn, states:

I am a master mariner. I have commanded ships engaged in the coal trade. I have shipped coal at Swansea three times. I have had ships on fire from spontaneous combustion, one of the ships, the "Golondrina," took fire about 500 miles on the other side of Cape Horn. Her cargo was 630 tons of coal; it was mixed coal shipped dry; it was dry weather when shipped. I tried to extinguish the fire by boring a hole down below in the runnels and letting in water. I let in six feet. I then pumped her out after plugging the hole. I then put her on the other tack and let the same quantity of water into her and pumped her out again, opened all the hatches and found no fire only steam. At 4 o'clock next morning I saw fire coming out of her side in the upper works. We then prepared to leave her, and had to leave her. We had ventilators on board fore and aft on each side of the kelson, also in the bilges, and two up and down to the main hatch, two to the fore hatch, and one to the after hatch.

The other two ships were about the same tonnage. The "Corridora" carried a similar cargo shipped in dry weather; she was ventilated in the same way, and after 60 days out from Swansea, Cape Horn distant about 60 miles E.N.E., I discovered smoke coming up through the ventilators. We opened all the hatches and dug down as far as we could and found the seat of fire; heat increased till we got down and we found a calcined mass of white cinder-like lime. Smoke came from the hatches before we opened them. We found this seat of the fire near the bottom of the cargo. In 14 or 15 days afterwards the same ship took fire again from the same cargo. My men were brought out of the forecastle on to the decks in a state of insensibility. I went forward with my chief officer and found fire in the cargo near the foremast. I saw it through the platform boards. We dug down the fore hatch and got to the fire; it was right at the bottom of the hold on the platform about four feet from the ship's bottom; the fire extended eight or nine feet forward. We found a large mass of red hot coke, applied water to it, and put it out from our fire engine. We had 17 hands, all told, on board. Since this, we have altered the mode of ventilation. We now have trunk boards placed right fore and aft on each side of the midships stanchion, right up from the kelson to the decks. We also take the platform boards and put them on the 'tween deck beams. This leaves a width of 4 inches of ventilation right through the centre of the ship making as it were four separate cargoes. We are particular in taking in the coal dry, we would not ship it wet.

A. E. HAYNES.

DAVID PRICE sworn, states:

I am a master mariner and shipowner. I have shipped and carried cargoes of coal a great many times, principally from Newport. I never shipped smelting coal for the west coast. Always carried steam coal.

D. PRICE.

The following is the evidence given by M. Vassard to which the Court referred:—

ADOLPH VASSARD sworn, states—

I am an analytical chemist, and have examined three specimens of coal carried by the "Minnie Graham," No. 1 from the Hope Colliery, No. 2 Tyr Filkin, No. 3 small coal and coal dust. No. 1 is very brittle, and it shows two veins of iron pyrites, each vein measuring about a quarter of an inch, that is, half an inch in 4 inches of coal. It contains by weight three per cent. of iron pyrites, equal to 1·60 of sulphur. It absorbs water easily, 2 per cent. in five minutes. There is no binding power between the coal and the pyrites, so that the water passes through like a sieve. It contains 40 per cent. of volatile matter (gas and sulphur). No. 2 sample. This coal (Tyr Filkin) is more sulphurous than No. 1, the pyrites completely permeate the entire mass, which is a mass of sulphur. This is more porous than the Hope Colliery coal; this absorbs 2·8 per cent. It contains 5·25 of pyrites, being equal to 2·80 of sulphur, volatile matter being 49 per cent., including sulphur. No. 3 sample, small coal and dust, contains 6 per cent. of pyrites, which gives 3·2 of sulphur per cent. It emits only 37 per cent. of volatile matter, including sulphur. It contains also a large quantity of aluminous shale amounting to 6½ per cent. Shale would absorb a large quantity of water easily, which makes the coal dust adhere like mortar. Besides that the presence of shale facilitates the accomposition of pyrites to a great extent, on account of its great absorbent power. I have already given my theory in the case of the "Herbert Graham," on a previous inquiry, and am still of the same opinion as to the means to be adopted by ships carrying cargoes of coal.

From the evidence I have heard yesterday, I am of opinion that, considering the nature of the cargo of the "Minnie Graham," and the wet state in which it was shipped, no system of ventilation would prevent the cargo taking fire, and I am surprised that she did not take fire before. If coal was shipped in casks tight the coal would not ignite.

I produce slips of a letter sent by me to the Mining Journal on the subject of coal cargoes, expressing my view on the subject pretty fully.

With a cargo like that of the "Minnie Graham," spontaneous combustion is a mere question of time. Ventilation of heaps of coal exposed to the atmosphere is very efficacious. Ventilation of coal on board ship would be useless for want of proper circulation of air.

Report of a court of inquiry, held at the Townhall, in the borough of Newport, Monmouthshire, by order of the Board of Trade, before Nelson Hewertson, Esquire, Mayor of Newport, and Thomas Beynon, Esquire, J.P., assisted by Captain Harris, and Fenton Wake, Esquire, into the circumstances attending the burning and subsequent abandonment of the barque "Minnie Graham," which occurred on the 26th day of October 1873, in the South Pacific Ocean.

The "Minnie Graham" was built at Cremyl, in the county of Cornwall, in the year 1868, and registered at Newport, Monmouthshire, in the same year, 273 tons. She was owned by Mr. Robert Graham and others, of this port, and on this present voyage commanded by Mr. William Evans, who holds a certificate of competency dated 1867. The "Minnie Graham" left Newport on July 17th, 1873, manned by a crew of 10 hands, all told, and laden with a cargo of 414 tons, consisting of three qualities of Newport coal, all shipped in a very wet condition. Early on the 22nd of October a strong smell of gas was perceived coming up in the forecastle and the cabin aft. The hatches were opened, and the following day smoke was seen issuing from the fore and after hatchways. The long boat was removed from over the main hatchways, and upon digging down the coal was found to be greatly heated, and the iron stanchions extremely hot. Finding nothing could be done to extinguish the fire, the hatches were put on, and preparations were made on the 26th to abandon the ship, being then 700 miles or thereabouts from the port of Valdivia. On the afternoon of that day a vessel, the "Naval Reserve," of Liverpool, hove in sight, the master and carpenter of which came on board, and after taking the hatches off and examining the cargo, advised the abandonment of the ship. Accordingly a hole was bored in the run, by the advice of the master of the "Naval Reserve," in consultation with the master of the "Minnie Graham," and his chief mate; and the whole crew, with provisions and part of their effects, were taken on board of the former ship, and all landed at Callao. The loss of the vessel by spontaneous combustion was very similar to that of her sister ship, the "Herbert Graham," the coals in both instances having been shipped in a very wet condition. It must, however, be observed that there was more slack, or small smelting coal, on board of the "Minnie Graham" than in the former vessel, which, according to the evidence of Mr. Vassard, the analytical chemist, was more liable to produce spontaneous combustion. The time in which this took place was very nearly the same in both vessels. An analysis of the three qualities of coal shipped on board of this vessel showed that they were all of a very inflammable nature, containing large amounts of iron pyrites, the details of which will be found in the evidence. The Court then addressed itself to the question of ventilation, but with no better result than in the case of the "Herbert Graham." Some evidence was given by the master of a vessel who had tried the mode of ventilating by air shafts, which had signally failed, and these, on subsequent voyages, were replaced by trunk boards placed on each side of the midship stanchions fore and aft in the hold, which seemed to be more effective. The separation of the cargo, as far as practicable, appears to be the most desirable mode of carrying a coal cargo liable to combustion, but even this cannot be relied on for long voyages unless the coals be shipped in a dry condition. With these remarks we beg to refer to the valuable evidence of Mr. Vassard, which, together with that of the former inquiry, appears to have exhausted the subject. Having regard to the position of the "Minnie Graham" in the South Pacific when the fire occurred, and the great distance from any port of refuge, the Court did not consider the abandonment of the ship premature, and returned Mr. Evans his certificate.

Given under our hands at the Town Hall in Newport, Monmouthshire, this 14th day of March 1874.

NELSON HEWERTSON,
THOMAS BEYNON,
Justices of the Peace.

We concur in this report.

HY. HARRIS,
FENTON WAKE,
Nautical Assessors.

CORRESPONDENCE, relating to SPONTANEOUS COMBUSTION, between the BOARD OF TRADE and the LOCAL MARINE BOARDS in the UNITED KINGDOM, arising out of the case of the "PERSIA."

The Board of Trade considered the subject referred to by the Court in their report of the loss of the "Persia" of so great importance, especially as it appeared therefrom that three vessels had been burnt in 1868, and their crews taken into Mauritius, that they forwarded an extract from the report to each of the Local Marine Boards in the United Kingdom and to Lloyd's, inclosed in a circular requesting their observations thereon. The following are copies of the circular and its inclosure:—

Board of Trade, Whitehall,
February 1869.

SIR,

I AM directed by the Board of Trade to transmit to you for consideration the accompanying extract from the report of the Mauritius Marine Board on the loss of the 'Persia,' relating to the frequent cases of fire on board coal-laden ships.

The Board of Trade agree with the Marine Board in thinking the question one demanding serious consideration and would be glad to be favoured with any observations on the subject.

I am, &c.
T. Gray.

The Secretary to the Local Marine Board.
At .

Extract from Report of Mauritius Marine Board.

"In reference to this loss the Board would remark, that in their opinion the burning of coal-laden vessels demands the serious investigation of scientific men. These fearful accidents are increasing in frequency, and spontaneous combustion is hardly a satisfactory conclusion to arrive at. Why is there spontaneous combustion? Why does one ship burn from that cause, and dozens of others laden from the same pit, at nearly the same time, arrive safely at their destination? Is ventilation of the coal advisable or dangerous? Ought hatches to be kept on or off? Could not inflammable gases be pumped out of a ship's hold? Is there not a method of creating gases in an inexpensive manner on board ship that would smother and extinguish any amount of fire in a vessel's hold? Such measures are spoken of commonly enough, but are apparently never carried into practice. Three large coal-laden vessels, the 'Pernix,' 'Volante,' and the 'Persia' have been burnt at sea, and their crews arrived in this colony during the last year. What destruction of property? What risk to life? What privation and suffering do these burnings entail? At this moment there is a valuable vessel in this port, coal laden which narrowly escaped the same fate as the 'Persia,' the coals having been smouldering many days before they could be extinguished."

To this circular the following replies were received by the Board of Trade from the Local Marine Boards:—

Mercantile Marine Office, 2, Dean Street, Cork,
Sir, March 12th, 1869.

At a meeting of the Local Marine Board held today, Philip Scott, Esq., in the chair, my Lords' letter, M. 1658 of the 5th inst., was read, when I was directed to say that this Board fully recognise the necessity of some precaution or step being taken to prevent spontaneous ignition or combustion in coal-carrying ships, and would suggest that the opinion of scientific chemists should be taken at coal-loading ports. Mr. Scott directs me to say that he had ships for some years in the trade, and the precautions he used were as follows:—

The ship was platformed on a level with the kelson, and a wooden ventilator, 3 feet square, was placed in the centre of each hatchway; the top of the ventilator, 6 feet above the deck, and the bottom resting on the kelson in connexion with the platform. In stowing the coal care was taken to place good square blocks of coal at each side of the ventilator to prevent injury in loading up. He never had accident.

I have, &c.
John H. Hall,
Secretary.

To the Assistant Secretary,
Marine Department, Board of Trade.

Local Marine Board, 5, East India Avenue,
Sir, London, E.C., March 18th, 1869.

I am directed to acknowledge the receipt of your letter of the 5th instant referring to the accompanying copy extract from the Report of the Mauritius Marine Board on the loss of the 'Persia,' relating to frequent cases of fire on board coal-laden ships.

I am to inform you that this Board have directed their attention to this most important question, and they are of opinion that the subject of spontaneous combustion in the coal of this kingdom when stowed in large quantities in ships and the consequent danger and destruction to life and property, should receive the most serious attention from the community at large, with the view of discovering, if possible, some simple and available preventive.

The Board are also of opinion that stowing coals in a ship's hold in a wet condition is the principal cause of spontaneous ignition. Coals are never transported either in barges or railway waggons under cover, so that whenever rain or snow is falling the coals absorb it all. In all, excepting the coal ports, coals are taken to ships in open barges which may have been exposed to two or three days' rain, they are frequently taken on board in rainy weather, and this, it is believed, causes them to become heated when stowed in large quantities, and in hot weather and under certain circumstances, ignition is the result.

This Board could instance cases where vessels ventilated in precisely the same manner, and when the coal has been shipped comparatively dry, have made their voyages successfully, while others which have taken in their coal during wet weather have had partial ignition.

The modes of preventing the shipment of wet coals are obvious; when in the coal ports and under the shoots it would be necessary to cease loading when rain or snow is falling, and when coals are taken alongside in barges, these barges should be furnished with tarpaulings, and coals when shipped for voyages which might extend to a month should be treated as dry cargo, those in charge of ships declining, of course, to load in wet weather.

The Board are also of opinion that the more ventilation resorted to the better; this should be done in the most simple and inexpensive manner, or it will be evaded as the freights for coals, notwithstanding the danger of ignition, are always low.

The Board in this communication will not attempt to suggest how such ventilation can be best managed as various modes may be adopted by different persons which might be equally efficacious.

The subject, however, is of such vital importance that the Board would suggest a prize being offered for the best paper on the most effectual and simple mode of preventing the ignition of coal on board ship.

I have, &c.
John Domett.

The Secretary, Marine Department,
Board of Trade.

Local Marine Board, Sailors' Home,
Sir, Liverpool, March 19th, 1869.

I am directed by the Local Marine Board of this port to acknowledge the receipt of your letter of the 5th instant, accompanied by an extract from the report of the Maritius Marine Board, and in reply to state that they can arrive at no other conclusion than that the destruction of ships referred to, was the result of spontaneous combustion of the coals.

Without entering into the investigation of the causes of spontaneous combustion, it is sufficient to know for all practical purposes, that all coals are more or less liable to spontaneous combustion; that the longer they remain undisturbed the greater is the danger, consequently vessels conveying coals to India, China, or other distant countries are more liable to be destroyed by the spontaneous combustion of their cargoes than those on shorter voyages; that some coals are more liable to this accident than others, especially those coated with iron-pyrites; some coals abound with this mineral, whilst others *from the same pit* are frequently *quite free from it*. This may in some cases account for coals in some ships undergoing spontaneous combustion, whilst 'dozens of others laden from the same pit at nearly 'the same time arrive safely at their destinations.'

A well-arranged system of ventilation will undoubtedly prevent the decomposition of coals that leads to spontaneous combustion; but ventilation, when applied after decomposition is carried to a certain extent, hastens the conflagration; thus, in the case of the 'Hesperus,' no indication of fire was observed until she had arrived at her destination in China, and her hatches had been opened for a few days.

Ventilation is, however, the best means of preventing the chemical action that leads to spontaneous combustion, but to be efficacious must be so conducted as not to expose the coals to moisture, since dampness is one of the conditions that leads to the spontaneous combustion of coals; for the same reason a cargo of coals should be always put on board as dry as is practicable, and every means adopted to protect them from moisture.

Dr. Inman, the brother of Mr. William Inman, of the Inman line of steam ships, has directed a considerable amount of attention to the subject of spontaneous combustion generally, and if consulted by the Board of Trade would probably afford valuable suggestions on this subject.

I am, &c.
John T. Towson,
Secretary.

The Assistant Secretary,
Marine Department,
Board of Trade.

In accordance with this recommendation from the Local Marine Board at Liverpool, the Board of Trade forwarded an extract from the Report of the Mauritius Marine Board on the case of the "Persia," to Dr. Inman, stating that they would gladly receive and consider any communication relative to the matter which he might think fit to favour them with.

The following reply was sent to the Board of Trade by Dr. Inman:—

12, Rodney Street, Liverpool,
Sir, March 25th, 1869.

In answer to your request for information respecting spontaneous combustion, let me, in the first place, express my gratification that the subject has been taken up by the Board of Trade. My investigations into it, some years ago, taught me that it was a fertile source of fire on shipboard, and in warehouses, yet that great ignorance prevailed respecting this form of catching fire amongst those whose business was to store various goods on land or water.

As it is necessary to touch lightly upon the whole question ere I answer specifically respecting the spontaneous heating of certain coals, allow me to state that there are many things which ignite without the intervention of extraneous fire. Amongst these the most prominent are freshly-burned charcoal in powder or in bulk, lamp-black, cotton-flax, or other fibrous materials saturated more or less with vegetable oils; the worst oils being such 'drying' oils as linseed, which are rendered much more liable to ignite spontaneously when they have been boiled. These fluids when in bulk have no such propensity, for an extensive surface exposed to the air is necessary for the chemical change to take place that produced the heat. Practically, all materials that are said to 'dry' by exposure are liable to heat, the dryness referred to being due to the absorption of oxygen from the air, and not to the evaporation of a volatile fluid. From the cause thus indicated engineers cotton waste, used for cleaning oily hands or oily machinery, recently made oil cloth or tarpaulins, bales of fibrous materials upon which oil has accidentally dropped, or in which it naturally exists, are very prone to ignite spontaneously. In Liverpool a large fire was attributed to the ignition of a heap of matting and other material permeated by castor oil.

Certain vegetable products, amongst which grass and hay stand foremost, are liable to self-combustion; and it is doubtful whether certain cotton is not somewhat dangerous from the same cause.

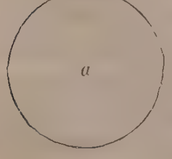
When spontaneous combustion arises it is brought about by chemical changes and the absorption of air.

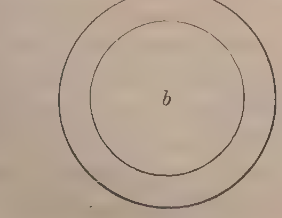
As an illustration of the former, I may mention that very pure iron and lead, in a state of impalpable powder, will become incandescent in a few seconds after being exposed to the air. Potassium sodium will do the like. In these cases oxygen has been absorbed, and a new substance formed. Amongst the substances liable to heat spontaneously by chemical agency is sulphur. This phenomenon is seen on a large scale in volcanic districts, wherein vast tracts are heated to whiteness by combination of sulphur with other elements. It is important to notice that coals frequently contain a large portion of this material, generally in the form of "pyrites." The ignition of haystacks is also due to a chemical change in the juices of the grass.

As an example of ignition by rapid absorption of air, I may state that barrels of freshly-burned charcoal finely powdered have been found red hot at a foot from the surface.

I must next call attention to a very important fact which, though it seems to be a truism, has not yet been duly appreciated, viz., "the hotter a thing is the sooner it ignites, and the faster it burns." As an apt illustration, take a bottle of rum from a cold cellar, and plunge a candle or other light into it, you will find that the spirit will not ignite, and will extinguish the flame. Allow that same bottle to remain in a temperature of 80 degrees, and the spirit will then ignite on the approach of a lighted candle. Woodwork, persistently in contact with steam pipes, will ultimately ignite spontaneously; and hot or warm painters' oils and cotton wool together will begin to ignite almost instantaneously, although some minutes elapse ere the heat rises to redness. To this subject we must allude again when we speak of the ignition of coals.

There are many materials which ignite spontaneously under certain circumstances which do not necessarily under all, and although it seems paradoxical the very same agent which prevents combustion in one may encourage it in another case. That which promotes ignition is access of air, but if the dangerous body be small in bulk, this access of air will carry away the heat produced as fast as it is formed. A certain bulk in the mass liable to be heated is necessary for complete ignition. I may express what I mean by two diagrams, *a* and *b*, both representing spheres of newly-burned charcoal, both equally absorb air and become heated, but the smaller sphere radiates away or otherwise loses its heat from the surface as fast as it is formed, but when the sphere is large the heat generated in the interior does not readily pass away, and it continues increasing, providing there is access of air, until a red heat is produced.





But if, knowing this, we should endeavour to cool the heated mass, or increase the current of air around it, with the same view we shall do no good, such a wind would operate as a blast upon a fire. If, however, the mass is cold to start with and of small bulk, good ventilation will prevent ignition. But in this case there is much danger, for a cask may be perfectly cold outside and incandescent in the middle, and in such a case ventilation would promote fire.

The fact that combustion may be proceeding in the centre of a mass whilst the outside is cold is readily recognised by the farmer who has to probe his hay-stack with a long iron rod before he can tell the extent of its heating, or if it is hot at all.

In one of my experiments I had a thermometer in the centre of a piece of cotton wool, on which I had sprinkled some painters' oil, yet these spots were so hot as to burn my finger, whilst the thermometer was unchanged. In a short time (half an hour) thereafter, half the mass was of a dull red heat, and on my simply touching it, the whole burst into a blaze. Whilst it was smouldering I could have saved half my cotton; after the flame arose this would have been impossible.

I must now call attention to the bearing of the law, "the hotter a thing is, the faster it burns;" thus, spontaneous combustion is a comparatively slow process; and requires a certain amount of bulk for its production; the heat in the mass steadily increases from without, inward ignition taking place at a distance from the surface; when, therefore, the inside is red hot, the outside is simply warm, but the whole mass heated by the chemical action becomes rapidly hotter as soon as it comes into contact with fire. Hence almost all fires arising spontaneously spread with amazing rapidity, for they take their origin from fuel peculiarly prone to ignite. Even the cook knows that hot wood will kindle the fire sooner and burn faster than cold wood, however dry.

When we consider that many of our ships travel for weeks together in a tropical climate, that their cargoes are kept perhaps at a temperature of 90° or more, the great importance of this law is at once recognised. All ships in hot climates are far more liable to ignition, and spontaneous combustion is more to be feared in them than in those trading in temperate zones. The more prolonged the heat the greater the danger.

Spontaneous combustion may be prevented or suspended by surrounding the material with any gas which does not support combustion, *e.g.*, nitrogen and carbonic acid. The first is innocuous to human life, the second is a deadly poison; a similar result may be obtained by preventing the access of air, as by surrounding the doubtful substances by tin or iron.

Having thus called attention to spontaneous combustion in general, I will address myself to the subject of coal. This material may be divided into two classes, the first of which is called anthracite, and appears to have undergone a sort of distillation by heat underground, and to be free from certain gases and sulphur; the second, or ordinary coal, contains a large quantity of gas in its interstices, and a notable amount of sulphur in the form of pyrites; the gas from it continues to escape from the time it leaves the mine until the whole is dissipated, hence old coals do not blaze, and coal in store always loses weight without there being peculation. In warm weather and when coal from fiery pits has been placed on board ship the gas extricated from the coal (carburetted hydrogen) mingles with the atmospheric air in the hold, and makes an explosive mixture that is liable to be fired by a sailor lighting his pipe, or by the galley fire. This danger continues for some time after the vessel has sailed, but may be obviated by careful ventilation.

The sulphur in the coals it is, however, which renders them most liable to spontaneous combustion, but there is some doubt as to the precise way in which it operates. In the absence of accurate chemical information the following broad facts may be stated:

1. A heap of slack in the centre of a coal yard will ignite spontaneously and in the central part first. This may be due to the same cause which operates in igniting freshly-burnt charcoal or lamp black, or it may be due to some chemical change with the sulphur. By some it is attributed to the decomposition of the pyrites by the influence of water or rain, or in the atmosphere. But none of these explanations can be regarded as otherwise than provisional.

2. A heap of coals stacked up, under cover, against a warm wall, whose temperature does not exceed 100°, will ignite spontaneously, combustion beginning in contact with the wall.
3. A heap of coals in a steamer's bunker, and in contact with the steam pipes or the warm iron-work around the furnaces and engine will ignite spontaneously, the change occurring first in contact with the heated surfaces. Sulphur being liable to ignition at a low heat, it is probable that this substance has a great deal to do with the combustion. It is, I believe, certain that the ignition does not occur in non-sulphurous coals.
4. In every mine there are, I believe, varieties in the relative amount of sulphur present in the seams and the various workings, and hence I conclude that in vessels laden with coals from the same pit, there may be considerable variety in the quality of the material as regards sulphur.
5. I think that I may safely say a large block of coal has never been known to ignite spontaneously; even when burned on a fire it consumes itself slowly.

We are now in a position to draw the deductions:
1. That slack is more liable to ignite than lumps of coal.
2. That heat increases the propensity to spontaneous combustion.
3. That sulphurous coal is dangerous according to the amount of pyrites that it contains.

Hence follow the conclusions:
1. That coal ought not to be shipped in bulk when a large amount of slack must necessarily exist.
2. That shippers or merchants should be recommended to eschew sulphurous coal, which are readily recognised by their containing much shining whitish or yellowish material that almost looks like metal.

As the existence of heat as a source of danger, is constant in tropical regions, and as some coal-laden vessels, even under such circumstances, escape combustion, it is necessary to investigate the probable causes of exemption. These may be stated to be,—

1. The quality of the coal on board.
2. The method of packing.
3. The actual temperature met with during the voyage.

Of the first I need say no more.

Of the second I would tell an anecdote. Shortly after reading an essay upon the subject in hand, a gentleman, to me unknown, communicated to me a patent that he had taken out for ventilating a coal ship's hold, so as to prevent the accumulation of heat or dangerous gases. The plan was simple, and I sought for evidence of its utility. Within a twelvemonth, however, I read a description of the loss by fire of three coal-laden ships, all furnished with this man's patent. Consequently I could not regard ventilation of the coal, however efficient, as a preventive against spontaneous ignition.

In trying to pick up what information I could, along our quays, I found the opinion generally entertained, that it was safest to send coal to tropical countries, wherever the material was not anthracite, in barrels, large pieces without slack being selected. In any published or private reports that have come to my knowledge no mention has ever been made of coal thus packed igniting spontaneously.

It will be understood that the coal ignites not from the presence of inflammable gases, but from the chemical change taking place between the sulphur and carbon in the material itself, favoured by warmth, or by the absorption of air or both combined. But as to the exact nature of the chemical action there is doubt, and I am not now in a position to clear up the subject, not being a professional chemist.

I am now able to answer categorically the questions and remarks in the abstract referred to me.

1. 'Spontaneous combustion" is a very adequate cause for explaining the destruction by fire of coal-laden ships, indeed, without the existence of heated coals, it would take a long time to consume a cargo of coals in a vessel's hold.
2. There is spontaneous combustion, because of the presence, in bulk, of sulphur, carbon, or slack, with a tropical temperature.
3. The ships ignited by such a cargo may have taken in their coals wet, moisture in slack promoting its heating, or their coals may have been very dusty, close packed, and generally small; or they may have been unusually sulphurous, or the weather may have been unusually hot. I once read of five ships from one port, carrying coals from one pit, sailing to the same destination at the same time, all igniting.
4. Ventilation of the coal is not desirable, except to get rid of the danger of explosion. It increases the danger of spontaneous combustion.
5. The hatches ought to be opened from time to time to explore the coals, &c.
6. Inflammable gases can be pumped out of a ship's hold; but these do not influence the combustion of the mineral.
7. Inexpensive gases, which will not support combustion, could readily be made to deluge the hold. One form is that of Philips' fire annihilator, which consisted of nitrogen and steam, in which men could work for a short time. Another is carbonic acid, that may be generated in large quantities from common chalk, limestone chippings, with diluted acid of some kind. This gas would prevent seamen going down the hold to inspect the seat of fire.

Yet there is reason to doubt whether either of these would do good. Without absolute knowledge, I suspect that the fire always originates close to the vessel's side, which is heated by the sun. Every timber contains a large amount of air in the cells of the wood; the fire would be fed thereby, even if the hold was deluged by the irrespirable gas.

It must be noticed, too, that if the gas did put out the fire, unless the coal were cooled, the mass would soon ignite again. Even when a coal-pit on fire has been 'put out,' air is not permitted to enter until the ashes are cold. This cooling is effected by steam or water finely divided, as by syringes.

After giving the subject of the transport of coals to India, China, or generally hot countries, much consideration, the conclusions that I have come to are the following:—

1. Iron ships are superior to wood, for wood supplies air, and may ignite.
2. That each ship should have iron partitions, iron decks, and light iron covers to fit over the coals, so as to make, as cheaply as possible, so many iron chests, which would be available for any cargo, and would permit but little air to enter.
3. Each receptacle for coal should be probed every day with an iron rod in many places, so as to ascertain the existence of heating, and iron tubes should be at hand to pour water on the required spot.
4. In wooden ships coal ought to barrelled, having been previously "screened," or if coal is carried in bulk, there should be some empty space left, or other contrivance by which the mineral could be turned over, as is malt in a mill, or wheat in a warehouse.
5. In vessels not specially adapted for the trade, when the coals have ignited, air must of course be excluded; and carbonic acid or other gas forced into the hold, half a ton of limestone chippings, and a carboy of spirit of salt; (muriatic acid, which is, I think, cheaper, and certainly better than oil of vitriol or other acid,) that might be diluted with sea-water, would be ample supply for a voyage. To complete the apparatus, there should be a stoneware retort, or large kettle, into which the lime and acid might be placed, and a hose passing from the spout into any appropriate hole cut or left for the purpose.

I trust that I have explained sufficiently the subject of spontaneous combustion in coals. To illustrate my views still farther, I am taking the liberty of forwarding a copy of my essay that was written in 1855. To me the matter has long been one of philosophical interest, and I have regretted that it is so little known. Of the pamphlet, only about half a dozen copies remain, but if the Board of Trade consider the matter of sufficient importance to be more generally known, and should desire my services as editor of a new edition, I shall be glad to give my assistance.

I am, &c.
THOMAS INMAN, M.D.

To the Assistant Secretary,
Marine Department,
Board of Trade, Whitehall.

"Local Marine Board, Sunderland,
SIR, 31st March, 1869.

REFERRING to your letter of the 5th of March instant, M. 1658, forwarding copy extract from the Report of the Mauritius Marine Board on the loss of the 'Persia' relating to the frequent cases of fire on board coal-laden ships, I am directed to inform you that at the meeting of the Local Board held on the 17th instant, the communications were handed to a sub-committee to go into and report; and at a meeting of the Board held to-day, the sub-committee brought up a Report which was confirmed by this Board, who resolved that the same with the papers annexed be forwarded to the Board of Trade.

I now therefore enclose the Report and papers herewith, setting out the views of this Board, and, I may add, that they consider the matter a very important one, and they have made a very full report on the subject.

I am, &c.
Robert Brown,
pro Secretary.

The Secretary, Marine
Department, Board of Trade.

Your sub-committee, having considered the questions submitted to this Board by the letter of the Board of Trade, dated March 5th, 1869, respecting the frequent cases of "fire on board coal-laden ships," have to report that they have not known any cases of spontaneous combustion on board ships load with north country steam coals. The cases which have come to their knowledge are mainly of ships laden with Welsh coals.

The cause of spontaneous combustion is in our opinion from the coals getting wet either before shipment or during the course of the voyage. We know that coals are frequently put on board in a wet state, as they are entirely exposed to the weather when they are conveyed from the pit to the ship. Cargoes shipped in this condition are very liable to spontaneous combustion.

The overloading of ships is a fruitful source of this evil, as it causes them to take much water on board at sea, a portion of which in all such cases makes its way among the coals, and, in many instances such overladen ships strain and leak, which increases the danger.

We consider that care should be taken that the water is not permitted to accumulate in the pump-well, so as to wet the coals in the bottom of the ships, particularly at the commencement of the voyage. Weight or pressure being one of the forces promotive of combustion the larger the ship, and the deeper the hold the greater will be the danger. But we think if coals are shipped dry and kept in that state with sufficient surface ventilation, fore and aft, at all times, it would effectuately prevent spontaneous combustion.

The reason why coals shipped from the same pit at nearly the same time, are conveyed safely to their destination by several ships, while one ship is burnt, is in our opinion to be accounted for by the cargo in the ship destroyed having become wet.

Respecting ventilation, we are satisfied that surface ventilation is right and good, and cannot be injurious unless combustion has taken place, but we think that shaft or funnel ventilation may be dangerous where the cargo has become wet.

We consider that hatchways should be kept uncovered in fine and dry weather, care being taken to prevent water going down upon the cargo.

Respecting the question of extinguishing and smothering fire in a vessel's hold by the application of gases, we are not prepared to offer any opinion on this scientific problem, our attention being particularly directed to the prevention of spontaneous combustion.

Thomas Anderson,
Chairman, Sub-Committee.

The following paper was also inclosed in the letter from the Local Marine Board at Sunderland to the Board of Trade:—

The Board of Trade send a Report of the Mauritius Marine Board relative to the loss of the "Persia," and other two ships, yet they don't mention the description of coal, if North Country or Welsh.

The writer having been loser by two ships taking fire, has caused inquiries to be made as to the loss of property. The two ships will be described as No. 1 and 2.

No. 1 had a cargo of Welsh coals, 700 tons, on board, and was ventilated by three wood ventilators going down each hatch, yet after completing nearly half her voyage out, she took fire and was totally lost; crew saved.

No. 2 ship also took fire; and the captain ordered a hole to be bored in the bow to let five to six feet in the hold, and by perseverance the ship arrived at her port, the quality of the coals destroyed, in fact, a portion of them were cinders. Each of these ships had wood ventilators and from a report from this ship by the mate, he states the after ventilation was stopped up with coals, and yet most remarkable, the cargo in this part was quite cool.

It is remarkable that, few (if any) of North Country coals ignite, and yet one colliery near Sunderland, shipping about 2,000 tons daily, seldom have a case of firing. This is remarkable, for the coals are sent immediately to the ships, but every precaution is taken in keeping the hatches off whilst in harbour.

The cause of spontaneous combustion is difficult to discover, yet from experience it is found that large coals are not so liable, and several of the cargoes that have lately fired, had a large proportion of dead small in. This by lying so close, like the heaps at the pit mouth, is very liable to take fire.

It is evident there is something in Welsh coals which is not in North Country coal, and yet it cannot be gas, for explosions at sea are never heard of.

As to ventilating, it is the general practice in Wales to put wood square box ventilators in each hatchway with holes in the side; this is objectionable, and if the cargoes have to be ventilated, a division fore and aft the ship is preferable; the hatches kept off whenever the temperature is not too high.

There can be no doubt that salt water with some coals will cause fire, see extract from Captain Edwardes' letter herewith.

And a tin manufacturer states they have their charcoal heaps take fire, and they dare not put water on. It only helps to spread it. They carefully avoid water and simply rake it out.

The subject of spontaneous combustion is a very important one to coal owners, shipowners, underwriters, and the lives of the crew. If a scientific inquiry was made we have no doubt a saving of life and property would follow.

Extract from Letter from Capt. W. Edwards, of Barque "Mangosteen," dated December 18, 1868, Coquimbo, to Messrs. W. Nicholson & Sons.

"I write to inform you of my safe arrival, after a long passage of 93 days, all well. The "Patagonia" and us, we arrived both together last Saturday evening and moored on Sunday morning. The "Patagonia" has discharged 60 tons of coal overboard. He told me they were on fire, but there is no appearance of fire now. I always think it is the salt water getting in the coals that makes them take fire, which on my arrival, hearing of so many ships taking fire, I tried the experiment and wet the coals in the main hatch on Sunday evening, and commenced discharging on Wednesday morning; the coals were that hot where they were wet that it was almost impossible to stand in them to fill them. I am certain they would have taken fire in three or four days, but as soon as the wet coals were discharged the others were as cool as possible. I think if the hatches are taken off every opportunity, if only for an hour, and the salt water kept from them, I don't think there is any fear of them taking fire.

Local Marine Board, Leith,
3rd April 1869.

Sir,

I am directed by the Leith Local Marine Board to acknowledge the receipt of your letter, M. 1658, of the 5th March 1869, transmitting for consideration copy extract from the Report of the Mauritius Board on the loss of the "Persia," relating to the frequent cases of fire on board coal-laden ships.

I am directed by the Board to report that they have been informed by Captain Thomas Lindsay, a member of the Local Marine Board, and also by several other captains of merchant ships who have had great experience in the command of coal-laden ships on long voyages. All these gentlemen give it as their opinion that *in all coal-laden vessels ventilators should be placed throughout the whole of the cargo up to the hatches, and that the hatches ought to be kept off as long as possible, when the state of the weather will admit of this being done*, a practice they state they have adopted for many years in carrying coals from various pits in the United Kingdom; by taking such precaution, they consider there is no danger of fire from spontaneous combustion on board coal-laden ships.

The Leith Local Marine Board, taking into consideration the opinion given by these very experienced ship-masters while in command of coal-laden vessels, beg to submit the same as worthy of the attention of the Board of Trade.

I am, &c.
Thomas Thompson,
Secretary.

The Assistant Secretary, Marine Department,
Board of Trade, London.

Local Marine Board, Dundee,
6th April 1869.

Sir,

I am directed by this Local Marine Board to acknowledge the receipt of your letter of the 5th ultimo, transmitting an abstract from a report by the Mauritius Marine Board, relating to the frequent cases of fire on board coal-laden ships.

The Local Board having had this subject under consideration, and being convinced of its very great importance, desire me to state in reply, that the branch of the subject relating to means that might be employed for extinguishing fires in such cases is so much a question of science that they decline to offer any opinion on it.

At the same time the Local Board is convinced that much might be done in the way of prevention by proper ventilation, and the Local Board would feel obliged by being furnished with the reports (for perusal) the Board of Trade has received following investigations in cases of the kind referred to, if any such investigations have been made.

I have, &c.
THE SECRETARY.

The Assistant Secretary,
Marine Department, &c.

Local Marine Board, Newcastle,
14th April 1869.

SIR,

I AM directed by the Local Marine Board to acknowledge the receipt of your letter of the 5th ultimo, together with a copy extract from the Report of the Mauritius Marine Board on the loss of the "Persia," relating to the frequent cases of fire on board coal-laden ships, and stating that the Board of Trade would be glad to be favoured with any observations on the subject.

In reply, I am to state that the Local Marine Board are of opinion that the subject in question is one that demands the serious attention of merchants, shipowners, underwriters, and all parties interested in the commerce of the country.

The Local Marine Board have felt it to be their duty to obtain the opinion of some of the most eminent colliery engineers in this neighbourhood, acquainted with the nature of the substance of coal, on the important subject in question. Gentlemen believed to be the best able to form a correct judgment on the points raised in the correspondence referred to, and in whose opinion the Local Marine Board have great confidence.

The following is the opinion of each of the gentlemen referred on each of the questions raised:—

1st Question.—*Why is there spontaneous combustion on board ship?*

Answers.

(*a.*) From the heat produced by the decomposition of the pyrites existing in the coal.

THOS. E. FORSTER.

(*b.*) Coal is a substance composed of many ingredients, all of which are, under certain circumstances, liable to the action called fermentation; but the most active agent is the presence of moisture in contact with iron pyrites, commonly called coal brass. It has been found in colliery practice that large heaps of coal placed on the ground in the unscreened condition are much more liable to take fire from this cause than similar heaps of the same coal put down in the screened condition. This difference is obviously due to the smaller amount of moisture held by the coal in the latter. There is no reason to suppose that this cause will be in any degree less active in ships laden with coal than in heaps of that mineral placed upon the gronnd of the same cubical measure.

JAMES EASTON.

(*c.*) It is caused by the decomposition of iron pyrites which ought to be separated and taken out before the coals are shipped.

JOHN R. LIDDELL.

2nd Question.—*Why does one ship burn from that cause and dozens of others laden from the same pit at nearly the same time arrive safely at their destination?*

Answers.

(*a.*) Some cargoes may contain more sulphur than others and salt water may get amongst the coals during the voyage.

THOS. E. FORSTER.

(*b.*) So far as the coal itself is concerned, there cannot be any great difference in the quality of that raised from any one pit or coal seam from day to day, except in the care taken to pick out all the pyrites, or other foreign matter, which also has a tendency to produce spontaneous combustion. The greatest difference will be found in the relative condition of the ships themselves, in regard to their water-tight conditions, especially in their decks and topsides, which in heavy weather may admit more or less water upon the surface of the cargo, as well as the care with which the bilge water is pumped out.

JAMES EASTON.

(*c.*) One cargo may contain the pyrites and others not, all depends on the cleaning of the coals.

JOHN R. LIDDELL.

3rd Question.—*Is ventilation of the coal advisable or dangerous?*

Answers.

(*a.*) I consider that ventilation is of little consequence.

THOS. E. FORSTER.

(*b.*) Ventilation of the cargo of coal can only be dangerous when the process of fermentation has passed through the incipient into that of active combustion, the former known by the smell of carbolic acid which accompanies it, the latter by an evolution of visible vapour, which is often inflammable. It is advisable in all cases to ventilate the coal placed on board a ship bound to a distant port, and more especially so when that coal is shipped in the unscreened condition, as it usually is when exported for gas making, or manufacturing purposes. The mode of ventilating a cargo of coal of the kind above alluded to in the most efficient and least expensive manner, is to use a considerable extent of wooden trunks or boxes of variable area to suit the size of the ship, say from 8 to 12 inches square inside. These should be placed horizontally, fore and aft of the ship, in one or more lines, and a few feet above the ceiling of the ship's hold, each line connected with the different hatchways of the ship by vertical trunks or boxes of the same dimensions, and reaching up to the under side of the hatches themselves. These air trunks, both horizontal and vertical, should be perforated with small holes wherever they are placed below the surface of the coal. These air trunks must of course be put into their places while the ship is being loaded, and should in no case be allowed to become filled with coal. It is also of very great importance that in all cases of wind and weather permitting the commanders of ships and their officers when carrying coal should be required to remove one or more hatches from every hatchway each day during daylight.

JAMES EASTON.

(*c.*) Desirable to be done with gas coals, not necessary with others.

JOHN R. LIDDELL.

4th Question.—*Ought hatches to be kept on or off?*

Answers.

(*a.*) Not of much consequence.

THOMAS E. FORSTER.

(*b.*) Answered in No. 3.

JAMES EASTON.

(*c.*) Hatches to be kept on.

JOHN R. LIDDELL.

5th Question.—*Could not inflammable gases be pumped out of a ship's hold?*

Answers.

(*a.*) Spontaneous combustion does not arise from the gases.

THOMAS E. FORSTER.

(*b.*) Inflammable gas can only arise in a ship's hold from two causes; the first is from the gas contained in the pores of the coal on its severance from the coal seam, and is not dissipated in the course of its transit and shipment, and many explosions have arisen from this cause by reason of the hatches having been placed on the hatchways immediately on the vessel having been loaded. The other cause is derived from the coal when the incipient combustion is about to burst into the active condition.

JAMES EASTON.

(*c.*) Don't think pumping practicable.

JOHN R. LIDDELL.

6th Question.—*Is there not a method of creating gases in an inexpensive manner on board ship that would smother and extinguish any amount of fire in a ship's hold?*

Answers.

(*a.*) I do not consider this practicable.

THOMAS E. FORSTER.

(*b.*) There is a method of making a gas that would be inexpensive on board ship or elsewhere, and if it could be applied direct to a fire would certainly assist very materially in extinguishing it. The difficulty of its application to a fire, and that attending the exclusion of atmospheric air at the same time, would render its effect extremely uncertain.

This gas produced by the application of *dilute* muriatic acid to dry chalk or marble, and called carbonic acid gas, would be readily made on board ship by the use of a few old tubs or casks, but considerable risk would attend going below for some time after its application.

James Easton.

(*c*.) I believe not.

John R. Liddell.

The Local Marine Board from their own experience can say that in all well-managed ships loading from this district for foreign ports and distant voyages, the practice observed is to give as full and as free ventilation as possible during the loading, and whilst in harbour prior to sailing; and when the weather permits at sea to open all hatches, using wind sails, &c., care being taken to keep the coal free from moisture.

I have, &c.
Wm. Storey Whitfield,
Secretary.

The Assistant Secretary,
Marine Department,
Board of Trade, London.

Local Marine Board,
Belfast, 14th April 1869.

Sir,

With reference to your circular of the 5th March last, M. 1658, I am directed to state that this Board considers that iron lower masts, perforated below and above, by acting as funnels to carry off inflammable gases, would greatly tend to the safety of coal-laden vessels on long voyages.

I have, &c.
A. Atkinson,
Secretary.

To the Assistant Secretary,
Marine Department,
Board of Trade.

Local Marine Board, South Shields,
16th April 1869.

Sir,

I am directed by the Local Marine Board to acknowledge the receipt of your letter of the 5th March last, accompanying copy extract from the report of the Mauritius Marine Board on the loss of the 'Persia,' relating to the frequent cases of fire on board coal-laden ships, and requesting to be favoured with any observations on the subject.

And I am to state that, in the opinion of this Board, danger of fire on board coal-laden ships appears to arise from two causes:—(1) from the coals being laden in a damp state, or from their becoming wet afterwards from the casualties of the voyage; and (2) from the cargo containing in some cases a considerable quantity of carburetted hydrogen gas.

When coals are to be shipped for a long voyage they should not be allowed to be taken in after any considerable exposure to wet; and whether for a short or long voyage, coals from workings known to contain explosive gas should not be shipped without having stood some days (protected from wet) to allow the escape of gas as much as possible.

From the experience of several members of the Local Marine Board, they are convinced that proper ventilation is advantageous, and ought in all cases to be provided for. This can be cheaply and efficiently done by wooden vertical ventilators properly perforated, their number and position being regulated by the size of the vessel. If they are connected by horizontal tubes the ventilation will be more perfect. They need not be brought above the hatches, which, moreover, should be opened from time to time, whenever the state of the weather will allow without exposing the cargo to wet.

The plan recommended in the Shipping Gazette (19th February 1869) appears likely to abate materially if not entirely to remove danger from fire, but the Local Marine Board are of opinion that the object can be obtained by less expensive means.

But by whatever means it is to be accomplished, the Local Marine Board would urge the absolute necessity of providing sufficient ventilation.

I am also directed to send for the consideration of the Board of Trade the annexed remarks on the subject by James Mather, Esq., a member of the Board, whose knowledge of the chemistry of gases, and whose experience in relation to pit accidents and explosions, render these remarks worthy of attention.

I am, &c.
Your obedient servant,
J. W. Lamb.

The Assistant Secretary,
Marine Department,
Board of Trade.

Mr. J. Mather's Paper.

Oxygen 1·000	Carburetted Hydrogen. 0·555	Nitrogen 0·9722
Life and Flame giving.	Explosive Gas. Mines.	Destructive gas.

To clear ships from the effects of cargoes of coals, the light carburetted hydrogen gas, the inflammable gas of mines, which becomes hot, and then bursts into flame by spontaneous combustion, and renders it dangerous for the ship to be exposed to it. This gas is about half the weight of common air, 0.555. It is found sleeping in the upper part of mines, or seeking to free itself from deposited or broken coal, or detaches itself from amongst water, and comes to the surface, and rises to the atmosphere where it is received.

Nitrogen is absorbable by about 100 volumes of water, and extinguishes flame. Its specific gravity is about 0·9722. The nitrogen forms about four fifths of the atmosphere, is unlike its oxygen, does not add light to combustibles, but on the contrary extinguishes it; the carburetted hydrogen dies in it. It being pumped through wooden or metal pumps perforated with holes from 1-inch or to 2-inch holes, the latter, if thought proper, to be covered with wire gauze or with perforated zinc, where it will be diffused when carried to the atmosphere, the breeze absorbing it; and thus will the threatened catastrophe of this great maritime calamity be mastered and dispersed.

James Mather,
Member of the Local Marine Board, South Shields.
April 14th, 1869.

The Gases produced in Coal Cargoes.

To clear ships from the dangerous concomitants of coal cargoes, it is simply necessary to pump out the light carburetted hydrogen from the hold, and thus free them from this gas, which becomes heated and spontaneously bursts into flame. This gas is about one half the weight of common air (0·555). It is always found floating about the upper part of mines, or seeking to creep away from deposited or broken coal, or detaches itself from amongst water and rises to the surface, thence passing away into the atmosphere.

Nitrogen also found amongst coal is absorbable by about 100 volumes of water. Its specific gravity is about 0·9722. It extinguishes the flame of the light carburetted hydrogen.

Wooden pumps perforated with holes, reaching to the decks from a platform laid upon the keelson, would certainly cause the carburetted hydrogen to find its way to the decks, whence it would be harmlessly diffused in the atmosphere.

The usual and safe way is to fill in the coals around the perforated tubes. These perforations should be one inch, or even two or three inches in diameter. In the latter case the holes might be covered with strong gauze or perforated zinc, whereby the inflammability of the carburetted hydrogen would be materially abated, if not entirely destroyed, whilst the pumping would discharge this gas freely upon deck, whence it would be swept away by the breeze.

By this simple process I think all danger of such fearful catastrophes as have occurred will be obviated.

James Mather,
Member of the Local Marine Board, South Shields.

Local Marine Board, Greenock,
7th May 1869.

Sir,

Referring to your letters of the 2nd and 5th March, I now enclose a report by a committee of this Board adopted at a meeting of the Board, held on the 27th ult., on the subject of spontaneous combustion in coal-laden vessels.

I am, &c.
Thomas King, Secretary.

The Assistant Secretary,
Marine Department, Board of Trade,
Whitehall, London.

The Committee of Marine Board on spontaneous combustion in coal-laden ships, after considering information obtained from various parties likely to be well informed on the subject, report as follows, but with reference solely to the Clyde district.

Conflicting opinions are entertained as to the cause of recent casualties to coal ships loaded in the Clyde, (vessels loaded there having for many years, till within about a year ago, enjoyed an immunity from such casualties,) but there seems to be a general agreement of opinion that spontaneous combustion at sea arises more frequently from loading what is locally named main seam coal, which contains much sulphurous matter, than from the other principal seams in this neighbourhood called ell and splint, also that mixed coal, viz., cargoes loaded from different pits, are from whatever cause more likely to ignite, than those loaded altogether from one pit, and that especial danger arises from coal being loaded in a wet state.

Suggestions in the way of remedy which the committee venture to make are,—

1st. That it is safest to ship only splint coal for long voyages.
2nd. That cargo should be as far as practicable loaded from one pit.
3rd. That the greatest care should be taken to secure the shipment of coal in as dry a state as possible.
4th. That ships' holds should be efficiently ventilated, and to aid ventilation that the hatches should be taken off at sea as frequently as possible.
5th. That some simple plan should be adopted for ascertaining and recording the temperature of the hold daily or oftener, so that the earliest possible notice may be given of danger.

The whole subject is surrounded with difficulties, and perhaps ought on account of the conflict of opinion to be reported on by eminent chemists. The Committee therefore hesitate to give any very decided opinions on the question.

JOHN H. CARMICHAEL, Convener.
Greenock, 23rd April 1869.

Local Marine Board, Glasgow,
2nd June 1869.

SIR,

AT a meeting of this Board on the 31st ultimo, I was directed to acknowledge receipt of your communication of 5th March last transmitting for the consideration of this Board a copy of an extract from the report of the Mauritius Marine Board on the loss of the 'Persia,' relating to the frequent cases of fire on board coal-laden ships, and requesting the observations of this Board on the subject, and I am to state that the experience of the members of this Board was against spontaneous combustion, that in their opinion ignition was usually caused, either by leakage, or by the coals having been shipped in a wet state, and that they would recommend that care should be taken that coals are shipped dry, kept so during the voyage, and thoroughly ventilated.

I am further to state that the members resolved, if possible, to obtain the opinion of practical men on the subject, and that any information so obtained will be duly reported to the Board of Trade.

I have, &c.
J. MUIRHEAD,
Secretary.

The Assistant Secretary,
Marine Department, Board of Trade,
Whitehall, London.

The following letters were also received at this time by the Board of Trade on the subject of spontaneous combustion in coal ships:—

1st. On 24th March 1869 Mr. Towson of Liverpool forwarded a copy of the printed slips issued since Dec. 1867 by the Liverpool Salvage Association, containing the following report on the system of ventilation in coal ships, with suggestions for its improvement:—

FIRES IN COAL SHIPS.

REPORT ON THE PRESENT SYSTEM OF VENTILATION, AND SUGGESTED IMPROVEMENTS.

Salvage Association: Underwriters' Rooms, Liverpool,
Monday, December 16, 1867.

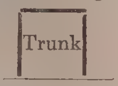
The mode of ventilation at present adopted here on board of vessels taking coal cargoes for long voyages is to place a vertical shaft down each hatchway, composed of two strong boards about 24 inches apart, connected by short pieces of wood, which overlap each other like a Venetian blind; these shafts rest on the keelson, and are usually connected together by horizontal trunks which run fore and aft on each side of the keelson, and are formed by boards which rest on the ceiling and lie obliquely against the keelson.

These shafts and trunk have hitherto been considered sufficient to allow the escape of any gas which may emanate from the coal, and to allow the circulation of sufficient air to carry away any heat which may be generated in the hold of the ship.

The burning of three or four coal-laden ships from this port, and the reports from other vessels which sailed hence and which have arrived at their destination with their cargoes in a very heated state, indicate that this method of ventilation is not sufficient, or that it is not adequate to the ventilation of large ships.

It has been supposed that in ships which are laden at the coal tips the great weight of the coal thrown in at once may break the trunk or shafts, and allow small coal to fill them in places, and that occasionally the coal may even be projected into the top of the shaft, and that thus the proper circulation of air through the shafts is prevented.

After duly considering the subject, and without denying that accidents to the shafts and trunk may sometimes occur while the vessel is loading, the surveyors of this Association have come to the conclusion that in large ships a much more extended system of ventilation should b adopted. *They attribute the heat to the spontaneous and slow combustion of the small and dust coal in the bilges, and think it can be traced to this small coal becoming wet during the voyage, or to its being shipped in a more or less damp state.* On the supposition that the fires reported are due to this cause, it seems not unlikely that a limited supply of air would tend rather to hasten than retard the combustion of the coal. With a view, therefore, to give such a quantity of air as will keep the cargo cool, they suggest that, in vessels of 900 or more tons, a trunk, similar in section to the annexed figure, and formed of stout planks, shall be carried fore and aft, about 3 feet above each bilge, and that these shall be connected by transverse trunks to the vertical shafts which lead up to the hatchways. These are in addition to the usual trunks alongside the keelson.



In ships of large burthen which have no 'tween decks laid, two additional trunkways should be placed fore and aft on the top of the 'tween deck beams, and these should also be connected by transverse trunks with the vertical air shafts.

There is another system of ventilation of coal cargoes which the surveyors to this Association consider may advantageously be adapted in those ships which are permanently fitted with the large flange ventilators which run down to the lower deck at their fore and after ends, as in the Australian or American packet-ships, viz., that of trunks running horizontally through the whole body of coals to the fore and after ends of the vessel beyond the cargo, and connected with the flange ventilators, by means of which currents of air could be made to pass through them from aft to forward, or forward to aft, according to the direction of the wind at the time.

Either of these plans, it is thought, would tend to reduce the risk of fire to which coal-laden vessels are now exposed.

It is also suggested that the temperature of the air in the shafts or trunkways should be taken every night and morning and recorded in the ship's log-book, as such observations would indicate the presence of heat in the hold long before it could reach any dangerous degree.

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2. The following letter from Mr. George Fawcus, of North Shields.

North Shields,
April 27th, 1869.

DEAR SIR,

I BEG leave respectfully to submit for your consideration the following suggestions to prevent the so frequently recurring explosions on board of gas coal laden vessels.

Many plans have been tried to ventilate *the hold up to the deck,* by means of perforated boxes alongside of the keelsons, boarding alongside of the hold beam stanchions, or shafts up to the hatchways, &c.

A simple plan now seems wanted to allow the gas to escape *from underneath the deck without allowing water to fill the hold.*

There are two ways of doing this, either by ventilating tubes in the way of the masts amidships, or in the way of the rigging at the sides.

A metal tube closed up to the hold beams and perforated between the hold beams and the deck, and carried up, say, four feet above the deck, would serve as a ventilator to the hold, and also as a well for sounding the pump well without drawing the pump boxes. The end of this tube could be left open always, with safety, in a coal-laden vessel, and in vessels with damageable cargoes could be closed with a plug during bad weather.

A tube could be placed on the rails (protected by the rigging), communicating with ventilators or air ports in the between-deck ceiling, the covering boards, and a water-tight space between two stanchions under the rail.

I am, &c.
GEORGE FAWCUS.

The Assistant Secretary,
Marine Department,
Board of Trade,
Whitehall, S.W.

P.S.—Grain cargoes also require ventilating.

And 3rdly, two letters from Mr. W. C. Perry, of which the following are copies:

West Hartlepool,
20 Scarboro' Street,
May 25th, 1869.

SIR,

I BEG to acknowledge the receipt of your letter of the 19th instant, stating that the Board of Trade is prepared to receive any observations upon the causes of spontaneous combustion in coal-laden vessels.

The remarks I have to submit are few and simple, and apply only to vessels making sea voyages, and I will ask permission to repeat a former question, whether the risk of these disasters would not be greatly lessened if this kind of freight were confined to iron ships having nothing of wood below the upper deck.

1. An admitted cause of spontaneous combustion of coal is that of its being shipped wet, and when the form of the trucks used for conveying coals to ships is considered it will appear how it happens at times that it is shipped very wet.
2. The form of these trucks is that of an inverted cone, and in consequence there is a very large exposed surface, sufficient in a single day's rain to allow the whole mass to be penetrated with this wet.
3. From want of skill in working, or from the nature of the material, there is a large proportion of small coal shipped in these parts, and this greatly increases the danger in wet weather, and from some experiments I have made since the receipt of your letter it is apparent that small coal has a capacity for retaining moisture, beyond what is likely to be considered. In this particular, as well as in mineral difference, the Welsh steam coal appears better and safer for long transport.
4. The remedy suggested is that all sea-going ships be loaded from covered trucks (or "waggons"). A light inexpensive cover open at both ends and removable when not required could be fitted in a way not to interfere at all with loading and discharging.
5. Another known cause of spontaneous combustion is that of the gases in some kinds of coal. In a deep mine in the north I was led to notice the incessant sound of gas escaping from a "new working" and it is not easy to doubt that if coal in this state were at once to be shot down a ship's hold it would be unsafe. The remedy appears to be exposure to the action of the air for a sufficient time before being put on board.
6. Passing to the possible causes of spontaneous combustion, may there not be cases where the condition of the hold from a previous cargo has operated in this way? As an instance, may not coal after guano (especially in wooden vessels) be an originating or at least an accelerating cause of generating inflammable gases.
7. Whether in some instances of apparent spontaneous combustion in cotton and in wool-laden ships the cause may not be due to excessive friction of the cargo at certain points in the hold when the ship is labouring heavily in a high sea?
8. Concerning the question as to whether ventilation is advisable in coal-laden vessels, I would submit whether partial ventilation is not more calculated to aid combustion than to check it, and that unless ventilation can be carried out more effectually than by any means yet discovered, it would not be safer to keep the hold shut up, and in an emergency to resort to flooding rather than to any other means.

I am, &c.
W. C. PERRY.

Assistant Secretary,
Marine Department,
Board of Trade.

Cantreff House, Abergavenny,
July 8th, 1869.

SIR,

SUPPLEMENTARY to my letter of 25th May I beg to offer one or two other observations upon the subject of spontaneous combustion in coal-laden vessels.

Disregarding doubtful causes of these disasters, there are two held certain: first, moisture either at the time of loading or afterwards; second, the gas contained in the coal.

MOISTURE.

From experiments I made in the north, I found that the amount of moisture coal will retain varies not only with the size of it, as was to be expected, but with the quality; so that hardly any two samples from different mines in the same district appeared alike in this particular. The small stuff called 'duff' retained 6 per cent. of moisture, which in a cargo of 1,000 tons would give 60 tons of water, and although this is an exceptional case, it is to be noted that general moisture or dampness in a coal cargo is not at all required for combustion. In local and probably very limited centres of moisture may be found the germs of danger, and to a single waggon of wet coal at loading might be traced the destruction of a ship at sea.

Hence, as I before submitted, the importance of not allowing sea-going ships to be loaded from open waggons in all weathers.

GAS.

To illustrate the necessity of exposing some kinds of coa to the action of the atmosphere before loading, it is only necessary to mention that I have notes of nine cases of explosions which occurred shortly after the loading had been completed.

VENTILATION.

It does not seem as if the question of ventilation can be met by fixed rules, as on account of variety of circumstances cases require to be dealt with differently, and I would submit to your department as a subject for inquiry, the difference between the gas or gases native to the coal, and which occasioned the explosions alluded to above, and the gas or gases generated from wet coal.

The former requiring the contact of sparks or flames before explosion, the latter resulting in spontaneous combustion.

I am, &c.
W. C. PERRY.

Assistant Secretary,
Marine Department,
Board of Trade.

The following CASES of SPONTANEOUS COMBUSTION in COAL-LADEN SHIPS, which have occurred since the appointment of the Royal Commission for inquiring into the Cause of the Spontaneous Combustion of Coal in Ships, have been reported to the Royal Commission by the Board of Trade.

"AURELIE" barque of Quebec, 548 tons register, six years old, and classed L. 11 Veritas, for nine years.

REPORT of a NAVAL COURT held at HER BRITANNIC MAJESTY'S CONSULATE GENERAL, NEW YORK, on Friday the 3rd day of September 1875.

PRESENT:

E. M. ARCHIBALD, Esq., C.B., Her Britannic Majesty's Consul General, New York, President.

THEODORE COOK, master of the steamship "Russia," of Glasgow, O.N. 58312.

WILLIAM WALKER, master of the ship "Greenock," of Greenock, O.N. 66083.

The court was assembled for the purpose of inquiring into the circumstances connected with the loss of the barque "Aurelie," of Quebec, official number 59894, which vessel foundered and was lost at sea on the 7th August last.

The late master of the "Aurelie," Octave Côté, the chief mate, James Bailey, and the scond mate, Alphonse Côté, were in attendance, and, being respectively examined, stated as follows:—

OCTAVE CÔTÉ sworn:—I was master of the barque "Aurelie," of Quebec. I took command of her on the 17th of October 1873. I hold a certificate of competency as master of the "Dominion of Canada," No. 621.

The "Aurelie" was a wooden ship of 548 tons burthen, built at Quebec in 1869. She was coppered. We took in a cargo of coal at Liverpool in April last in the Bramley Moore dock. We were occupied two days in shipping it. It was English steam coal, brought from some place near Liverpool. I cannot say from what place it came. It was bituminous coal. I cannot say whether it was Welsh coal or not. We took in 701 tons. My opinion is that it was English, and not Welsh. As far as I can recollect, the weather was fine and dry. I know that the second day that the coals were shipped was a very fine day. I do not recollect that it rained on either of those days. The coals were apparently of good quality and good order. The vessel was not completely loaded; she could have carried 150 tons more. I have never carried a cargo of coals in this vessel before. She had a freeboard of fully six feet. She was in good order and well found in every way. She had just been newly coppered in April. Her class was L 11 Veritas. She had been classed for nine years, and there were three years then to run. Our crew and myself were 14, all told. We sailed from Liverpool on the 16th of April for Barbadoes and a market. We arrived at Barbadoes on the 31st of May without any occurrence worth mentioning.

We remained at anchor in Carlisle Bay for 20 days. There was no sale for the coal there, nor in the neighbouring islands to which I telegraphed; and having received orders from my owners to proceed to Montevideo, we sailed from Barbadoes for Montevideo on the 20th June. We had very fine weather during the time we lay at Barbadoes, not very hot. We had the fore hatch, or rather one half of it off, during the time we lay there, in order to ventilate the ship. There were two or three showers during the time, but when it rained the hatch was put on, and the cargo covered. We left Barbadoes with strong trade winds and ran to the eastward until we were in about 32° north latitude. After that we had light and variable winds, but beautiful weather, until the 1st of August, when we were in 16° 39 north latitude, and 30° 16′ west longitude. Between 6 and 8 o'clock on the evening of that day the second mate reported to me that there was fire in the hold. He said he judged so because there was unusual heat and gas. I went at once into the hold and digged down into the coals about a foot, near the quarter hatch; I put my hand on the coal where I had digged and found the coal so hot that I could not bear my hand on it for a minute. We were then on the port tack stretching to the southward. The crew wished me to turn back to Barbadoes. I said I would not, as Pernambuco was quite as near, and that when it would be daylight on the morrow we could see better what was best to be done. Next morning I took a jack stay, an iron rod about 15 feet long, which I shoved down through the coal at the quarter hatch to a depth of about 12 feet, This was at 5 a.m. on the 2nd of August, I left the rod in the coal for two hours, and then drew it up. The middle part of it was red hot, the bottom part cool, and the upper part so hot we could not handle it, but had to put straps round it. At 7 o'clock I decided to bear up for Barbadoes, in order, if possible, to save the ship. As soon as we drew up the rod the smoke followed and burst up. Some of the crew also digged down with shovels some 3 feet, and about 5 or 6 feet from the ship's side, and the smoke came up so strong that the crew could not stand it. I ordered them to fill up the hole again and come on deck, and then I had the hatches closed down tight, and we abstained from pumping, so that the water might rise in the ship, and if possible extinguish the fire. We made all the sail we could for Barbadoes. We perceived the smell of wood smoke, and knew that some of the beams must be burning. On the 3rd of August the fire increased, and the deck became hot. I left part of the forehatch open, to permit the smoke to escape, and also least the gas might blow up the ship. We got the longboat over the side and fitted her, in case we might have to leave the ship. On the 4th the heat and smoke still increased, and we got the chains on deck lest the heat should prevent us. On the 5th August smoke and heat still increased greatly. The pitch was boiling in the deck seams, and we could no longer live in the cabin on account of the dense smoke and gas. All the ropes and gear from below were got on deck.

On the morning of the 6th of August, it became evident that the ship must be abandoned, as she was then burning. We got out the second boat and provisioned it, taking as few clothes and effects and as much food as possible, as we we were then 1,500 miles from land. At noon on the following day, the 7th of August, we pumped the vessel dry. This was the first time we pumped her after the discovery of the fire. At 4 p.m., on sounding again, we found 3½ feet of water, showing that her side must have been burnt through. At 5 p.m. she had about 6 feet of water in her; at all events she was settling fast. We then all took to the boats and lay by the ship until 9 p.m., when she sunk. Besides our crew, there was my wife and an infant child of about two years. We steered for Barbadoes, the two boats keeping together from the 7th of August until the 10th of August, on which day, at about 7 a.m., we were picked up by the American ship "Moonlight" from Batavia bound to New York, and were brought in that ship to this port, and landed on the 31st ultimo. My wife was confined on board the "Moonlight," and gave birth to a male child on the 28th ultimo.

I suppose the cause of the fire was spontaneous combustion. The ship made regularly nearly one inch an hour of water on the voyage out to Barbadoes. After leaving Barbadoes she did not make more than three-quarters-of an-inch per hour. She was regularly pumped dry twice a day. I suppose that the damp or water rising up through the coal, in connexion with the sulphur in the coal, generated combustion. The coal was loaded on the skin of the ship, that is the usual mode. The bilge-water never could have risen to the skin, except when we left it after the discovery of the fire. I do not know whether the ship or cargo were insured.

(Signed) Octave Côté.

(Signed) E. M. Archibald,
H.B.M. Consul General,
President.

James Bailey, examined: I was chief officer of the late barque "Aurelie" of Quebec. I joined her at Liverpool on the 16th of April last. I hold a mate's certificate of servitude, No. 79,049. I superintended the loading of the ship at Liverpool. We were two days taking in the cargo of coals. They were common house coals. I do not know where they came from. The hold was dry when we took them in. The weather was dry while we loaded. No rain or wet got on them at that time. I remember the discovery of fire in the coals on the 1st of August. It was my watch on deck. The second mate first reported it. On the following morning we pierced the coals with an iron rod, and left it for two hours, and when it was drawn up the rod was red hot. The smoke and heat increased from day to day until the 7th of August, when we were compelled for the safety of our lives to leave the ship. There was nothing in our power which we could have done which would have saved the ship. The cause of the fire must have been damp among the coals, and they had been nearly four months in the ship. I have been two voyages to Bombay in ships with coals from England, we had no fire on either occasion.

The length of each voyage was about three-and-a-half months. We were perhaps about a month in passing through the tropics.

I think that being detained for a long continuous period in the tropics would increase the hazard of fire in a coal cargo. (The master's statement was here read to this witness.) The statement of the master is quite correct so far as I recollect. We had very moderate weather the whole voyage from Liverpool, and did not lose even a ratline.

(Signed) James Bailey.

(Signed) E. M. Archibald,
H.B.M. Consul General,
President.

Alphonse Côté.—I was second mate of the late barque "Aurelie." I am brother of the master. I remember the occurrence of the fire on the 1st of August last. One of the sailors and cabin boy said to me they smelt fire below. I then went into the hold; this was about half-past 6 in the evening. I did not see anything, but I smelt the gas very strong. I then went and reported it to the master. He came immediately and went below, and made an examination, and we all smelt the smoke.

The same night I digged a hole about two feet and a half deep in the coal, and I could not bear my hand on the coals they were so hot. Next morning we pierced the coals with an iron rod, which when drawn up was red hot. The smoke and heat continued from day to day until they were at last unbearable, and we were obliged to leave the ship. The crew wanted to leave earlier lest the flames should burst out, but the captain stayed as long as it was possible. We made all the sail we could to return to Barbadoes; but we were very far from land. Nothing could have been done, in my judgment, to save the ship. She sunk in about five or six hours after we cast off from her. We saw her go down. We were nearly four days in the boats when we were picked up and brought to New York. I cannot give any opinion as to the cause of the fire.

(Signed) Alphonse Côté.

(Signed) E. M. Archibald,
H.B.M. Consul General,
President.

Octave Côté, being further examined, saith: The cargo was shipped at Liverpool on account of the owners of the ship, through their agent. I am not aware of any regulations in regard to the loading cargoes of coal for long voyages, or for any voyages. About five years ago I commanded a barque in which we carried a cargo of coals from Liverpool to Havana. It was shipped in the Bramley Moore dock. Our voyage lasted about 50 days. The coals were delivered at Havana in good order. I have carried coals from Cape Breton to the West Indies, but the voyages were of short duration. In my late voyage the hold was ventilated by having off half of the fore hatch, as I have stated; sometimes, when the weather was fine, I had the quarter hatch off also. Before the discovery of fire, we did not notice any settlement of the cargo; but afterwards, we observed that the coals settled down. We considered that this was owing to the fire in the body of the coals below making more space. We had a very rough night after we took the boats, and shipped a good deal of water. We could get no rest or sleep. As far as I could judge, there must have been nearly 6 feet of water in the hold of the barque when we quitted her. She had the appearance of being waterlogged.

(Signed) Octave Côté.

(Signed) E. M. Archibald,
H.B.M. Consul General,
President.

Finding of the Court.

From the evidence in this case it appears that the "Aurelie," a wooden ship of the registered burthen of 548 tons, six years old, in good order and condition, having been recently repaired and newly coppered, took on board a cargo of bituminous coal at Liverpool about the 14th of April last. The quantity shipped was 701 tons. It was taken on board during two days, when the weather was fine and dry. The ship was not fully loaded, it being testified that she could have carried 150 tons more. The coal was loaded in bulk on the skin of the vessel, without any artificial provision for ventilating the cargo. The ship sailed from Liverpool on the 16th of April "for Barbadoes and a market," and arrived at Barbadoes on the 31st of May, having had fine and moderate weather throughout. She lay at Barbadoes for 20 days, during which time the master, after communicating with various West Indian ports, and finding no market for the coal, received orders from his owners to proceed to Montevideo, for which port the ship consequently sailed from Barbadoes on the 20th of June.

She pursued her voyage with moderate weather until the 1st August, when, owing to the effect of the trade winds, she had reached 30° 16′ W. longitude, and 16° 39′ N. latitude.

On the evening of this day, the second mate having reported that he suspected the coals to be on fire, owing to an unusual odour of gas, a hole was digged in the coals, which, at the depth of 2 feet, were found to be greatly heated.

On the following morning a rod of iron being driven down through the coals to the depth of 11 feet, was allowed to remain two hours, when, on being drawn up, the middle part of the rod was found to be red-hot. The ship was then steered for Barbadoes, and all sail made to reach, if possible, that port. Day by day the smoke and heat from the coals increased; the cabin became uninhabitable; the pitch in the deck-seams boiled, and the beams and lining of the ship below were, from the nature of the smoke, evidently burning.

Meantime the ship was kept under a press of sail, steering for Barbadoes, while the boats were provisioned and fitted for quitting her in case of necessity. When the fire was discovered on the 1st of August the master ceased pumping the ship, in order to allow the water to rise and thus possibly extinguish the fire. The hatches were for a day or two closed, but afterwards partly opened to obviate an explosion of gas. On the 7th August, at noon, the ship was pumped dry, and on subsequently sounding the well at 4 p.m., 3½ feet of water were found in it, showing that the ship's side must have been burnt through. At 5 p.m., she began to settle, there being nearly 6 feet of water in her hold, and the heat and smoke being also intolerable. The master and crew embarked in the boats and lay by the ship until 9 p.m., when she sank and went to the bottom.

Together with the master and crew in the boats was the wife of the master, in a very delicate state of health, and their little daughter two years old. They were then at a distance of more than 1,500 miles from the nearest land, and the boats keeping together were steered towards Barbadoes. After nearly four days of hardship and loss of rest they were fallen in with and rescued by the American ship "Moonlight," bound from Batavia to New York, and conveyed in that ship to this port.

In reviewing the evidence we find that, during the voyage out to Barbadoes, the vessel made not more than three-quarters-of-an-inch of water per hour, and less than that quantity per hour after leaving Barbadoes.

That the water could hardly have reached the coals, as the ship was regularly pumped. The moisture from the bilge-water would, however, be absorbed by the coals, and the only ventilation, if it can be called such, of the cargo, consisting in leaving a portion of the fore hatch open, spontaneous ignition of the coal naturally ensued, resulting in the loss of the ship. Such a result was obviously natural, when it is considered that the coal had been on board of this ship for very nearly four months, and during nearly three months of that time continuously within the tropics. If, as is usually the case in ships carrying coals on a voyage likely to extend over three months, a provision had been made for ventilating the cargo by means of wooden boxes or tubes penetrating the coals, the ignition in this case would probably have been prevented; and as this ship's cargo was not to be certainly landed at Barbadoes, but was shipped for that port "and a market," which, as it turned out, required her to proceed to the distant port of Montevideo, we are of opinion that some sufficient means of ventilating the cargo, and allowing the gases to escape, should have been supplied at the time the cargo was taken on board. It does not appear upon whom the duty of making such a provision devolved; and it was probably not taken into consideration by anyone connected with the ship or cargo. Having regard, however, to the circumstances of the loss of this ship, we deem it important that ships carrying cargoes of bituminous coal to distant ports, or on voyages likely to extend over 30 days, and especially when bound for tropical regions, should be loaded under official inspection, and subject to the compulsory adoption of such appliances for ventilating the cargo as may be best calculated to obviate the danger of spontaneous combustion.

Under the circumstances in which the master found himself placed when the coals were discovered to be on fire, we think he conducted himself with skill and courage. His determination, indeed, not to quit the ship so long as there was a possibility of bringing her to port was creditable to him, and was carried almost to the verge of rashness; for on the last two days, when the ship was excessively heated, the fire might have burst forth at any moment, wrapping the ship in flames and imperilling the lives of those on board.

(Signed) E. M. Archibald,
H.B.M. Consul General,
President.

L.S.

(Signed) Theodore Cook,
Commander R.M. "Russia."

(Signed) W. Walker,
Master, ship "Greenock."

New York,
September 3rd, 1875.

"SENATOR," of Windsor, N.S., of 1,101 tons register, sailed from Philadelphia on the 8th June 1875, for Panama, with a cargo of coals.

Examination on Oath, instituted by Receiver of Wreck at the Port of Liverpool in pursuance of the 448th Section of the Merchant Shipping Act, 1854, 17th and 18th Victoria, cap. 104.

William Adams, being duly sworn, deposes as follows:

That he was ordinary seaman of the ship "Senator," of the port of Windsor, N.S.

That the crew consisted of 17 hands, including deponent.

That the ship had on board a cargo of coals.

That said ship had on board, in addition to the cargo aforesaid, three passengers, master's wife and two children.

That the said ship proceeded from Philadelphia.

That at the time of sailing as above the said ship was in good condition and well found. There were ventilators in each hatchway, which came up about level with the deck, but when the hatches were closed there was no ventilation.

That the said ship was bound for Panama.

That the said ship proceeded on her said intended voyage as above stated, and we had fine weather until we approached Cape Horn, and all the hatches were kept open. We had bad weather all the while we were rounding Cape Horn, and the hatches were all battened down when in the Pacific ocean. About 8 o'clock one evening I heard the ship was on fire, and went aft. A hole was made in the fore cabin on port side, and water was played down it with the force pump. All hands were on deck all night, and on the following morning the smoke disappeared. We continued, however, to send water down. In the evening of that day the smoke again came up from the ventilator in after hatch. We then poured water down through the hole in the cabin, and also through the after hatch, but the smoke continued, and two or three days afterwards ship was kept away before the wind and ultimately anchored in the Straits of Magellan in smooth water. There was then fire on the port side, and cabin was full of smoke, and we all left her in two boats as soon as possible after coming to anchor. The captain's wife died on the way up to Sandy Point, and after remaining there some time we all left by the s.s. "Liguria." At Monte Video we landed four seamen, and at Rio the first and second mates, and remainder came on to Liverpool.

That the above contents are in all respects correct and true to the best of deponent's knowledge and belief.

(Signed) William Adams.

Sworn at Liverpool this 17th day of November 1875, before me,

(Signed) W. H. Coulston,
Receiver of Wreck.

(Signed) Edmund H. Verney,
Commander R.N.

Charles De Fleury, being duly sworn, deposes as follows :---

That he was able seaman of the ship "Senator," of the port of Windsor, N.S., of the register tonnage of 1,101 tons.

That the said ship was rigged as a barque; that she was built of wood.

That the crew consisted of 17 hands, including deponent.

That said ship had on board a cargo of between 1,600 and 1,700 tons of coal shipped at Philadelphia for Panama.

That said ship had on board, in addition to the cargo aforesaid, three passengers, viz., master's wife and two children.

That said ship proceeded from Philadelphia on her intended voyage, as named below, on the 8th day of June last past.

That at the time of sailing as above the ship was in good condition and well found in every respect. Her draught of water was 21 feet. There were ventilators below the hatches, but when off Cape Horn we experienced bad weather, and the hatches were not taken off for a month.

That the said ship was bound for Panama.

That the said ship proceeded on her intended voyage as above stated and when off Cape Horn, during my watch below, a seaman was washed overboard and drowned.

That on Monday the 13th day of September, at 8 p.m., the weather variable and the wind in the westward, blowing a very strong breeze, with a heavy sea from the westward, the said ship was standing to the northward when one of the crew came forward and said the ship was on fire. Just after, the captain called all hands aft to pump water into the ship. When I went into the cabin smoke was issuing from the port quarter between the timber into the cabin, and the captain ordered the carpenter to cut a hole through the lining of the ship on the port side, through which we pumped water all night. On the morning of the 14th the smoke disappeared from the cabin, but was coming up through the ventilator in the after hatch, and we continued to throw water inside the lining of the ship where the fire was supposed to be. The smoke was confined to the after part of the ship. We drew the pump boxes to allow the water to run down through them. On the 15th we commenced to throw cargo overboard, but although we could not reach any fire some of the coals near the beams were consumed and large quantities of smoke and gas continued to ascend, so that it was difficult to work there. On the evening of the 16th the captain stood to the eastward for the Straits of Magellan. We had quite a gale of wind and we run her under topsails (the upper ones being reefed) reefed spanker and jib. About 7 a.m. on the 17th September we anchored about seven miles in the Straits of Magellan, abreast of Cape Pillar, at which time the smoke in the cabin was so dense that we could not enter. We immediately left the ship and landed near Cape Pillar, and about half an hour after she burst into a flame aft. The next day we left Cape Pillar and proceeded up the Straits, having scarcely any provisions. On the morning following the master's wife died, and on the following Saturday night we reached Sandy Point. The master, his two children, and eight of the crew came to Liverpool by the steamship "Liguria," arriving here on Saturday the 13th November about 4 p.m.

That in consequence of the casualty to the said ship two lives were lost, one washed overboard, one died from exposure, the remainder, 18, being saved by our own boats.

That the above contents are in all respects correct and true to the best of deponent's knowledge and belief.

(Signed) Charles De Fleury,
Deponent.

Sworn at Liverpool this 17th day of November 1875, before me

(Signed) Edmund H. Verney,
Com. R.N.,
Receiver of Wreck.

"CLEVEDON," of Liverpool, an iron ship of 698 tons register, seven years old, and classed A1 at Lloyd's.

Naval Court of Inquiry on the burning and abandonment of the British barque "Clevedon," official No. 60086, of Liverpool, held at the British Consulate, Valparaiso, on the 4th day of October 1875.

(Signed) J. de V. Drummond Hay,
H.B.M. Consul and
President.

British Consulate, Valparaiso,
6th October 1875.

Schedule of Enclosures, transmitted with letter dated 6th October 1875, from H.B.M.'s Consul at Valparaiso. President of a Naval Court held at Valparaiso on the 4th day of Ocrober 1875.

Letter.	Nature of Document.
—	Minutes of Proceedings.
A.	Orders summoning the court.
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Minutes of Proceedings taken at a Naval Court of Inquiry held at the British Consulate, Valparaiso, on the 4th day of October 1875, to investigate the causes of the burning and abandonment of the British barque "Clevedon," official No. 60086, of Liverpool, on the 21st September 1875.

Present :

President :

James de Vismes Drummond Hay, C.B., Her Brittanic Majesty's Consul.

Members :

Lieutenant John Deane Deane, of Her Majesty's ship "Tenedos."

Mr. William Harris.
Master of barque "Maida," of London.

Clerk of the Court :

Mr. Thomas Owen Joynson, Paymaster of H.M.S. "Tenedos."

The master of the barque "Clevedon," David Hughes, with the first mate, second mate, carpenter, and 11 men lately forming the crew of the vessel, and named severally :

William Royce, first mate.
James Barnes, A.B.
Frederick Broom, ordinary.
Henry Boyes, second mate.
Frederick Henrickson, A.B.
A. W. Hignett, apprentice.
Benjamin Ouset, carpenter.
James Hands, A.B.
Charles Maughan, steward.
Angus Sutherland, A.B.
John Hartgreaves, sailmaker.
Joseph Dromedius, A.B.
William Morgan, A.B.
Edward Jacobson, A.B.

Absent :

Nicholas Demetrius, A.B.
Herbert Morley, A.B.
Walter Gilbert, ordinary.

Order summoning the court read in the presence of all the witnesses and the members of the court and clerk of the court sworn in.

Witnesses were then ordered to retire from the Court, except the first, David Hughes, master, whose certificate of competency, No. 88,378, was laid on the table, as also those of William Royce, first mate, No. 96,187, and Henry Boyes, second mate, No. 26,557.

David Hughes, being called on by the court to make a statement of how the burning and abandonment of the vessel occurred, laid before the court a written statement, contained in the official log book, which was read and signed by deponent, the first and second mates, sailmaker, and a number of the crew, and ordered to be annexed to the proceedings, and is marked J. ex J.

David Hughes was then examined, and in answer to questions put by the court replied as put down in the annexed deposition marked B. ex B.

An extract from the official log of the British barque "Foxhound," of Swansea, official No. 58090, duly signed by James Halliday, the master, and John Evans, mate, was read and ordered to be annexed to the proceedings, and is marked J. ex J.

William Royce, first mate of the barque "Clevedon," was next called before the court, and in presence of the master was examined, and stated as set down in deposition marked C. ex C.

Henry Boyes, second mate of the barque "Clevedon," was next called and in presence of the master was examined, and stated as set down in deposition marked D. ex D.

Charles Maughan, steward of the barque "Clevedon," was next called before the court, and in the presence of the master was examined, and stated as set down in deposition marked E. ex E.

Benjamin Ouset, carpenter of the barque "Clevedon," was next called before the court, and in the presence of the master was examined in French by the president, and stated as set down in deposition marked F. ex F.

The members of the crew, and named severally—

John Hartgreaves, sailmaker,
William Morgan, A.B.,
James Barnes, A.B.,
Frederick Henrickson, A.B.,
James Hands, A.B.,
Angus Sutherland, A.B.,
Joseph Dromedius, A.B.,
Edward Jacobson, A.B.,
Frederick Broom, ordinary,
A. W. Hignett, apprentice,
Herbert Morley, A.B.,
Walter Gilbert, ordinary,

were then called together before the court and examined in the presence of the master, and deposed as set down in annexed deposition marked G. ex G.

The court was then cleared, and the members proceeded to deliberate and frame their opinion and finding, which was written out, and, being duly signed, is annexed to the proceedings and marked H. ex H.

The court was then re-opened, and the finding of the court was read over in the presence of the master, mates, and crew of the late barque "Clevedon."

The certificates of the master, as also of the first and second mates, were returned to them.

The court was then closed, and the proceedings terminated at 5.30 p.m.

(Signed) J. de V. Drummond Hay,
H.M. Consul and President.

Annex A.

By Edwin John Pollard, Esq., captain of Her Majesty's ship "Tenedos," senior naval officer present at Valparaiso, being the officer empowered to order naval court by the Merchant Shipping Act, 1854.

To

James de Vismes Drummond Hay, C.B., Her Majesty's Consul at Valparaiso, as pres
John Deane Deane, Esq., lieutenant of Her Majesty's ship "Tenedos;"
William Harris, Esq., master of the British ship "Maida;" as members.

Whereas the master and 17 men belonging to the British barque "Clevedon," of Liverpool, official No. 60086, have arrived at Valparaiso, and have reported that the said vessel was burnt and abandoned at sea on the 21st September last:

And whereas I consider it requisite that a naval court should investigate the cause of said loss and abandonment: I do hereby order and direct you to assemble together to form a naval court at the British Consulate on the 4th day of October 1875, to inquire into the causes of the loss and abandonment of the aforesaid vessel, such court being hereby ordered to investigate the same.

Given on board Her Majesty's ship "Tenedos" at Valparaiso this 2nd day of October 1875.

(Signed) E. J. Pollard,
Captain and Senior Officer.

Annex B.

Examination of David Hughes, Master of the late Barque "Clevedon," of Liverpool, official No. 60086.

A written statement of the occurence being annexed to the proceedings, deponent, in answer to questions put by the court, replied as follows:—

The ship was seven years old, and classed as A1 at Lloyd's, and is iron, 698 tons. The crew consisted of 18 men, all told. The cargo consisted of steam coal. The fire was first discovered on the morning of the 20th September 1875, in lat. 41° 52′ S., long. 78° 33′ W. Had two pumps on board, both in good order. The ship was well found in everything. The ship was ventilated by a ventilator on the quarter deck, and one on the house forward, and one on main deck into chain lockers. The coal was shipped at Liverpool by F. A. Widdows & Co. The chief mate received the cargo. I saw about 300 tons passed on board, and what I saw was dry and in good condition. I am unable to account for the fire. Had one tin of paraffin oil on board; it was stowed in the oil locker on deck; at commencement of the voyage it was there, but subsequently it was condemned as unfit for use, and stowed away in the store-room. This can was examined after the smell of fire was discovered, and found to be perfect without leakage.

(Signed) David Hughes,
Master.

Before me,
(Signed) J. de V. Drummond Hay,
Consul and President.

Annex C.

Deposition of William Royce, first mate of the barque "Clevedon," holding a certificate of competency, No. 96,187, as first mate.

This is my first voyage as first mate. The coal was received in dock; the latter portion seemed to be larger coal than that first received. I did not notice any portion of the coal being wet. It appeared to me all dry. The weather was fine and it did not rain. The vessel was trimmed in general from 2 to 4 feet from the main deck. The fore and aft hatches were taken off whenever we had fine weather. The vessel was also ventilated by three or more ventilators. When we first perceived the smell of fire it was suspected that the steward might have upset some paraffin oil, but on examination of the lazarette the can was found untouched, and showed no sign of leakage. There was one 10-gallon can on board. Over the fore hatch there was no smell, nor was the coal hot. The after hatch the same. In the cabin there was a slight smell. We had had heavy weather and the ship had taken in some water. Two days after the smell, the smoke came out of the foremost ventilator. The foremost hatch was taken up, the smoke was so great that it was battened down again; the same with the after hatch. Had no knowledge how long the coal had been in the lighters before shipment. I am of opinion that the vessel was not left too soon. The decks were blown up by the main and after hatches. From the time we first saw the smoke the boats were got ready on deck, and afterwards all hands were employed endeavouring to smother the fire with water. After going on board the "Foxhound," the vessel which saved us, I returned to the "Clevedon" at the master's wish, in order to see what I could save. I entered the vessel by the stern, but could not go further than the companion, the fire was so great. The last I saw of the "Clevedon" the main and mizen masts had gone overboard. The seat of the fire appeared to be on the port side of the main hatch, that is where I believe it originated, amongst the small coal.

In answer to question by master, witness deposed—

I believe the master did ask the steward whether he had been using paraffin oil; I think his answer was that he had not, as some time before he found he could not use the paraffin oil and used colza oil instead.

(Signed) William Royce,
First Mate.

Before me,
(Signed) J. de V. Drummond Hay,
Consul and President.

Annex D.

Deposition of Henry Boyes, second mate of the barque "Clevedon," holding a certificate of competency, No. 26,557, as second mate.

This is my first voyage in this ship, and my first with the master, Mr. Hughes.

When I joined, a portion of the cargo was on board. I assisted in trimming a great portion of it below. It appeared to me to be dry. The large and small coal mixed. I know not the difference of steam coal and other coal at sight, but the cargo seemed to me to be all of one class. The first suspicion of fire was caused by a strong smell which seemed to be of paraffin oil, and search was made. The smell was perceived in the cabin and not in the fore part of the ship. This occurred on a Saturday, and on the Monday following we saw smoke and first became convinced of a fire on board. The hatches were battened down, and about two or three hours afterwards blew up, the main hatch going first, and through that hatch water was bucketed down. I do not think the vessel was abandoned too soon. I can give no opinion as to the cause of the fire, and have no suspicion of any foul play. The coal extended underneath the cabin deck. I went down on Saturday, the day of the smell, and examined that part of the cargo, but perceived nothing.

The smell was first perceived by the first mate, myself, and steward on the companion; the lazarette was further aft, below the after cabin where we had our breakfast, and where I had not smelt a smell.

(Signed) Henry Boyes,
Second Mate.

Before me,
(Signed) J. de V. Drummond Hay,
Consul and President.

Annex E.

Deposition of Charles Maughan, steward of the barque "Clevedon;" being duly sworn, stated as follows:—

I belong to Weymouth and have been 32 years at sea; am 48 years of age. It is my first voyage with Mr. Hughes. I did not know the master before I joined the vessel. The stores of the ship were all in my charge. We had paraffin oil and colza oil on board for the lamps. I used the paraffin oil at first, but not burning properly, with the consent of the captain I used only colza. The can with the paraffin was stowed away in the lazarette and I had no occasion to touch it again, except once, and that was before we rounded the Horn, in order to get a little for the carpenter's use. When we perceived the smell on the 18th September, being afraid that the paraffin can might have been capsized, I went down to the lazarette, but found that notwithstanding the knocking about of the vessel, the can had not been upset and was tightly corked. I can form no opinion as to the cause of the fire except that it originated in the coals. I think the master quite right in having abandoned the vessel. I have no suspicion of foul play. The decks were burnt and blown up in three places before we left.

(Signed) Charles Maughan,
Steward.

Before me
(Signed) J. de V. Drummond Hay,
Consul and President.

Annex F.

Deposition of Benjamin Ouset, carpenter of the barque "Clevedon;" being duly sworn, stated as follows:

I joined the "Clevedon" at Liverpool. I am a native of France. On Saturday the 18th September the hatches were closed. On Monday morning the mate called me hurriedly to bring a hammer to open the hatches. On opening the hatches a great volume of smoke came out and the hatches were again closed. It was my duty every morning to oil and clean the worms or screw of the wheel. I used olive oil—I call it so, for so it was called to me, but you will understand I did not taste it. It was given to me by the first mate out of a tin can in the oil locker on deck near my berth. The oil for paint was kept in other cans, and larger, in the same locker. At one time when near or off the Horn I tried paraffin oil to clean the stays of the worm, I used it for several days. I got it from the steward who went down with me to the lazarette for it, and he gave me about half a pint; I used it for 8 or 10 days, but finding it was not good I told the captain of it, and at his desire I used lamp oil. When on the Friday previous to the fire I had, as usual on a fine day, opened the hatches, I did perceive a slight smell such as of petroleum, but very slight. I can form no opinion as to the cause of the fire; but with reference to the suspicion of the paraffin, I must say that it could not have caused the fire, for the following reasons. The seat of the fire was on the port side of the main hatch, between that portion of the ship and the main hatch there is a bulkhead partly of iron.

When I had battened the hatches and secured the ventilation tightly I kept a watch on the beams of the deck, and it is my opinion that the fire commenced in the upper portion of the cargo, for not only did I notice the deck bulging on the port side between the main hatch and the kitchen, and the pitch in the seams, melting, but when told by the captain to bore holes in the deck to let water through, I found the planks, which were originally 4 inches thick, to be burnt to the thinness of 1½ to 2 inches proving that the fire was on the surface of the cargo. The pumps of the ship sucked, but not freely, at 4½ inches. I consider the vessel had always 4½ inches in her. There were 18 inches between the keel and the ceiling in depth.

The ship was not abandoned until the fire had completely got possession of her. I had secured my tools, and, by the good fortune of the vessel which saved us, was enabled to save them. I lost some of my clothes, but saved my tools.

(Signed) Benjamin Ouset,
Carpenter.

Before me,
(Signed) J. de V. Drummond Hay,
Consul and President.

Annex G.

Depositions given by the Members of the Crew named severally, as follows:—

John Hartgreaves, sailmaker,
William Morgan, A.B.,
James Barnes, A.B.,
Fredk. Henrickson, A.B.,
James Hands, A.B.,
Angus Sutherland, A.B.,
Joseph Dromedius, A.B.,
Edward Jacobson, A.B.,
Fredk. Broom, A.B.,
A. W. Hignett, Apprentice,
Herbert Morley, A.B.,
Walter Gilbert, Ordinary;

who did severally and together declare that they had no complaint to make; that they had no suspicion as to the cause of the fire, beyond its being caused by the coal; that the master did not leave the vessel until the fire had rendered it impossible to remain any longer in her; that they saved little or nothing of their clothes; that the master exerted himself in preparing and provisioning the boats; and that they were fortunately saved by the "Foxhound," on board of which vessel they were well treated until their arrival at Valparaiso.

Before me,
(Signed) J. de V. Drummond Hay,
Consul and President.

Annex J.

Extracts from the Official Log of the British barque "Foxhound," of Swansea, official No. 58090.

At Sea, September 18, 1875,
Lat. 47° 00′ South, long. 80° 30′ W.

The "Clevedon" barque, of Liverpool, passed close by us steering northward.

(Signed) James Halliday,
Master.
" John Evans,
Mate.

Septr. 21, 1875, 4.30 a.m.,
Lat. 40° 10′ South, Long. 77° 00′ W.

Made out a vessel to windward burning blue lights. I then hauled my ship to the wind and came to within hailing distance, when the said vessel proved to be the "Clevedon," of Liverpool, with cargo on fire, and the master asked me to lay by him, which I did until daylight, when I saw volumes of smoke and flame ascending from the hatchways, and the fire seeming to increase very rapidly. At 7 a.m. the master and crew came on board my ship in their boats. I then bore away and passed close under her stern and saw that her main deck was in a blaze. After the chief mate and four hands revisiting the ship, and seeing that nothing further could be done, and shortly afterwards seeing the main and mizen masts fall over the side, I then made sail and proceeded on my course as nothing more could be done.

(Signed) James Halliday,
Master.
" John Evans,
Mate.

True copy.
(Signed) J. de V. Drummond Hay.
Consul and President.

Annex H.

Finding of the Court.

At a naval court of inquiry assembled at the British Consulate, Valparaiso, on the 4th day of October 1875.

Present:

President.

James de Vismes Drummond Hay, C.B., Her Britannic Majesty's Consul.

Members.

Lieutenant John Dean Deane, of Her Majesty's Ship "Tenedos."

Mr. William Harris, Master of Barque "Maida"

Clerk to the Court.

Mr. Thomas Owen Joynson, Paymaster H.M.S. "Tenedos."

The court, pursuant to an order from Edwin John Pollard, Esquire, captain of Her Majesty's ship "Tenedos," and senior naval officer present at Valparaiso, proceeded to investigate the circumstances attending the burning and abandonment of the British barque "Clevedon," of Liverpool, Official No. 60086. And having deliberately weighed and considered the evidence and observations preferred by the master and the men, late members of that crew, who have appeared before the court: The court is of opinion that the statement made by the master of the barque "Clevedon," that the vessel was abandoned on fire at sea, is correct. That such fire had its origin in the spontaneous combustion of the coal. That the master, Mr. Hughes, his officers and crew, appear to have performed their duty to the utmost of their power to save the vessel; and that no blame can be to them attached for the casualty.

The court, therefore, has much pleasure in returning to the said master his certificate, as also to the first and second mates their certificates.

Further the court desire to express their opinion that had the "Clevedon" been supplied with force pumps or other appliances for extinguishing a fire, there might have been a greater chance of preserving the vessel, and that the provision of such appliances should be obligatory on all vessels.

(Signed) Wm. Harris.
" John D. Deane.
" J. de V. Drummond Hay.
(Signed) Thos. Joynson,
Clerk to the Court.

"RIVER BOYNE," barque of Liverpool, iron ship of 498 tons register, eight years old, classed A1 Lloyd's.

From the Wreck Report it appears that she sailed from Liverpool on the 15th July 1875, for Valparaiso, with 776 tons of coal.

No means of ventilation beyond keeping the hatches off.

Part of the cargo burnt owing to spontaneous combustion on October 12th, 1875, part of cargo (about 60 tons) thrown overboard, put back to False Cape Horn, when vessel was anchored; but finding fire could not be extinguished, master put the vessel close in shore and scuttled her by letting in water through decks, extinguished fire, raised the vessel and proceeded on voyage; arriving at Valparaiso on 27th November, where cargo has been sold and vessel undergone slight repairs.

A protest was received from H.B.M. Consul at Valparaiso by the Board of Trade, who forwarded to the Commission the following extract:—

Extract from the Protest of the British Barque "River Boyne," of Liverpool, Official No. 55,054.

That on the 15th of July they, the said appearers, left Liverpool in and with the said barque "River Boyne," laden with a cargo of coals and bound to this port of Valparaiso. That nothing worthy of note occurred until the 8th of October, from which date they experienced heavy weather, and the vessel laboured and rolled, shipping much water. That from the 8th October the hatches had been kept closed, on account of the heavy weather, until the 12th October, when they removed the fore hatch and found smoke issuing from the hold.

That all hands were set to work to extinguish the fire, and for that purpose they were obliged to throw overboard a portion of the cargo for the benefit of all concerned. That in spite of their endeavours the fire increased rapidly, and on the 13th October they deemed it prudent to run for an anchorage. That they accordingly battened down all the hatches and made for the land. That the smoke continued to issue in volumes from under the tarpaulins. That at 1.30 p.m. on the 14th October they arrived in a small bay near False Cape Horn. That they anchored the vessel fore and aft, and immediately took measures to extinguish the fire by making holes in the decks, flooding them with water, and pumping down the hold. That at 5 p.m. there were 7 inches of water in the hold. That at 8 p.m. the crew refused to continue working, complaining of want of rest. That the officers and carpenter continued flooding the hold until 4 a.m. on the following morning, and at 5 a.m. all hands again turned to. That the fire increased, and their efforts were unavailing. That at 6 p.m. there were 5 ft. 6 ins. of water in the hold. That during the night all hands were divided into two watches, and the head pump and main pumps were kept going, and the decks were kept flooded with water. That the cabin and forecastle were so full of smoke that all hands had to remain on deck. That at 6 a.m. on the 16th October there were 6 feet of water in the hold. That they took off the after hatch, and finding very little smoke they proceeded to dig the coals out to trim the ship more by the head, and also in order to get at the fire, which appeared to be on the fore side of the main hatch. That they threw overboard about 30 tons of coal for the benefit of all concerned. That at 4 p.m. the smoke drove the men from below and constrained them to close the hatch, and although the coal was quite cool as far as they had dug, the fire increased rapidly. That they did not cease flooding the coal, and at 8 a.m. on the 17th October there were 8 ft. 10 ins. of water in the hold, and the ship was down to the hawse pipes and scuppers in the water. That they removed the main hatch, but found the smoke denser than ever, and they were constrained to batten it down again. That after due consultation with his officers the master determined, for the safety of the ship and cargo, and the interests of all concerned therein, to scuttle the vessel. That after examining the shore and putting the vessel in position they beached the ship on the W.N.W. side of the bay, abreast of a stream of water, on a smooth sandy bottom, void of rocks. That they then proceeded to scuttle the ship by letting the water into the forecastle through the side lights, and the carpenter drove rivets out of the bulkhead of the fore compartment to assist the sluices in flooding the hold. That at noon the tide fell and left the vessel laying quiet and quite upright on the sand. That at 4 p.m. the tide rose and filled the vessel to 13 feet. That the smoke continued issuing from below, and all were kept at work flooding the decks. That they experienced hard gales of wind, but that the bay was perfectly sheltered from the heavy sea. That at 6 a.m on the 18th October the tide rose and filled the ship fore and aft, covering the main deck as far as the fore part of the main hatch. That there were 3 feet of water in the cabin, the coals being completely covered and the fire extinguished. That at 8 a.m. the tide fell clear of the hawse pipes and scuppers and all hands were employed at the pumps and the carpenter proceeded to plug up the hawse pipes and every aperture in the bulwarks and decks. That the wind continued to blow a strong gale from the S.W. That at 6 p.m. it was high tide and the water rose just above the hawse pipes but none entered the vessel. That the pumps were kept going all night. That at 6.30 a.m. on the 19th October the tide rose high and completely filled the vessel again. That at 9 a.m. the tide fell and the men returned to the pumps. That the weather continued boisterous. That at 6.30 a.m. on the 20th October the tide rose and flooded the vessel's decks again, but little or no water entered the hold. That they lightened the vessel by throwing cargo overboard into the sea out of the main hatch, for the benefit of all concerned, and that the pumps were kept constantly at work. That they got the tow line out and endeavoured to bend it, but were unable to do so on account of the strong gale, which also prevented them from pumping that night. That at 5 a.m. on the 21st October the pumps were again set at work. That at 7 a.m. the tide rose to the scuppers. That they ran out the tow line and 15 fathoms of chain, and shackled it on to the outlying cable which had been slipped. That at midnight the weather moderated. That there were 10 feet of water in the well, and at 6 a.m. on the 22nd October there were 8 feet of water in the hold. That at 7.30 a.m. the ship floated without any perceptible motion. That the weather was fine and moderate. That they hove on the tow line and got the ship out to her former position in 6 fathoms of water, with 60 fathoms on the port cable and 25 fathoms on the starboard cable, the ship being by the head, and the hawse pipes just clear of the water. That the pumps were kept constantly at work. That during the time the vessel was aground the lowest water round the ship was 10 ft. 8 ins. That at noon there was 1 ft. 9 ins. of water in the well. That the vessel did not leak. That at 6 a.m. on the 24th October they trimmed the coals from forward to the after part of the ship, and bailed out the fore compartment, the sluice being choked. That the water in the tanks was rendered useless, and had to be renewed from shore. That the pumps were kept constantly at work, and at 8 p.m. there were 7 inches in the well, which they pumped out. That at 6 a.m. on the 25th of October they continued trimming the ship. That they bailed the fore peak dry, and otherwise proceeded to get the vessel ready for sea. That at 7.30 p.m. they found 10 inches of water in the well, and pumped it out. That they continued making the ship ready to sail. That at 10 a.m. on the 26th October the American schooner "Golden West," of New London, came into the bay and anchored for an hour in order to offer assistance to the "River Boyne." That they weighed the stock of bread, and found there were 450 pounds fit for use left. That all the ship's provisions were more or less damaged, and a great portion of them was entirely destroyed by the salt water. That they prepared everything in order to set sail the first opportunity. That the weather continued boisterous until the following day, the 29th October, when they proceeded on the voyage. That the total amount of cargo thrown overboard was about 60 tons. That nothing further worthy of note occurred, and they arrived and anchored in this port of Valparaiso on the 27th day of November, on which day a survey was called on the condition of the vessel and cargo, and the instructions of the surveyors are now being carried out.

"LE BARON" of Liverpool, wooden vessel, 889 tons register, 6 years old.

From the Wreck Report it appears that she sailed from Hull on the 16th August 1875 for Valparaiso, with 1,378 tons of coal.

Was burnt at sea about 30 miles north of Cape Horn, owing to spontaneous combustion of coal cargo.

"BEUCLEUCH" of Alloa, iron ship of 1,350 tons register, 1 year old, classed A1 Lloyds'.

From the Wreck Report it appears that the "Beucleuch" sailed from Greenock the 18th August 1875, for Bombay, with 1,781 tons of coal.

Ventilated by means of four patent ventilators and usual hatches.

Partial lost of cargo and damage to vessel caused by spontaneous combustion of coals. On the 22nd November, in lat. 8° N., long 68° E., smoke and gas were observed to be issuing from the foremost part of the main hatch. The steam fire engines were set on and for four days played down the ventilator.

The hatches were then battened down, the hose continuing to play into the hold. After four hours the hatches were removed, the gas appearing to have greatly accumulated; coal was discharged from main hatch to the fore and after hatches until they were full. A large amount of water having been pumped below, about 200 tons of cargo was jettisoned from between decks. On the fourth day, the fire increasing, all hands were compelled to withdraw from the main hatch. Four rivets were now driven out from under water line to save the ship, there being at the time 7 feet water in the hold. At about 4 a.m. next day the water had increased in the hold to 10 feet, and the fire was extinguished. The holes were then stopped, sluices shut, pumps set on, and at 5 p.m. on the 27th November the pumps sucked. Trimmed ship upright, she had taken a slight list on transferring the coal, and resumed voyage, arriving safely at Bombay on the 5th December last.

"SAN RAFAEL" of Liverpool, wooden ship of 955 tons register, about 13 years old, re-classed at American Lloyds' A1 in September 1875.

From the Wreck Report it appears that she sailed from Liverpool the 18th September 1875, for Valparaiso, with 1,300 tons of coal.

Was ventilated by means of a ventilator in each hatch.

The vessel had arrived in the above-mentioned latitude and longitude all well. The second mate was in charge with the starboard watch on the morning of the 2nd January last. At 7 a.m. the sailmaker called the chief officer and stated that smoke was issuing from the forehatch, the second mate having been below previously for some lashings. The master was called, and after consultation the hatch was replaced and the vessel continued her course. At noon smoke was observed issuing from the after-hatch ventilator. The hatch was removed and the cargo found to be on fire. All hatches were now immediately battened down and the vessel was put about to return to the Falkland Isles. At about 10 p.m. on the 3rd January she had run back about 260 miles when an explosion suddenly occurred, blowing off the main and after hatches and displacing the bulkhead in the cabin. A part of the crew were now directed to prepare the boat, the remainder directed to the fire engine, and a large quantity of water was thrown down upon the cargo, but without avail. At about 10.30 p.m. a second explosion occurred and flames burst up the hatchways. The boats were got out and provisioned, explosions occurring from time to time. At 4 a.m. the 4th January, there being an immense body of fire in the vessel, it was determined to abandon her, the master, the master's wife, the carpenter, four A.B. seamen, and three apprentices got into the longboat. The first mate, cook, steward, sailmaker, and three A.B.'s got into the lifeboat. The second mate, the third mate, two A.B.'s and an apprentice got into the gig. The boats remained by the vessel until 8 a.m., when the weather freshening it was deemed advisable to pull to land. After about four hours the longboat got separated from the two other boats and has not since been heard of. At 2 a.m. on the 5th January the lifeboat and gig arrived at a small uninhabited island inside the Isle d'Alfonso, where water and shellfish were procured. The boats proceeded to sea, returning from time to time to the island for shelter in bad weather, searching along the coast for the longboat, and on the lookout for passing vessels. After 20 days the cook succumbed from exhaustion and was buried on the island. Seven days later, on the 31st January, the lifeboat and gig being at the time about 20 miles S. of Diego Ramirez Islands, a ship which proved to be the "Yorkshire" hove in sight and was signalled. She bore down and the hands from the two boats, 11 in number, got safely aboard. They were treated with great humanity and kindness by all on board the "Yorkshire," and arrived safely in London at 2 a.m. this day. The casualty appears to have been caused by spontaneous combustion.

"THERESA" of London, wooden ship, of 705 tons register, 18 years old, classed at French Lloyds' 5/5 1–6–5 years.

From the wreck return it appears that she sailed from Liverpool on the 25th September 1875, for Rangoon, with 920 tons of coals.

The ventilation, as described by the master, appears to have been complete.

Burnt at sea, about 70 miles west of Cape of Good Hope, by spontaneous combustion of cargo of coals.

"FANNY," barque, of Sunderland, 397 tons register.

Sailed from Swansea, bound to Valparaiso, with a cargo of coals.

When off Valparaiso, on 23rd December 1875, was found to be on fire from spontaneous combustion of cargo; she put into Coquimbo, where with assistance from H.M.S. "Nereus" the fire was got under and the cargo discharged, when she was found much burnt on starboard side by main rigging planks being nearly burnt through, in places to ⅓rd of an inch; was repaired, and left Coquimbo 9th April.

The following letter, received by the Board of Trade from Her Majesty's Consul at Coquimbo, relative to the cases of the "Mabel" and "Fanny," has been received by the Royal Commission from the Board of Trade:—

British Consulate,
Coquimbo, 4th May 1876.

SIR,

I HAVE the honour to enclose Vice-Consul Mark's report on the accident to the 'Mabel,' of Bristol, by fire at Caldera in December last.

"On 31st December and 13th April last, I reported the 'Fanny' O. N. 47667, which put in here on fire, which was extinguished, the vessel repaired, and sailed on 9th ultimo.

"It is worthy of remark that both vessels loaded at Porthcawl, Swansea, both bound for Valparaiso.

"The 'Mabel' arrived and remained a day or two, and the day she left for Caldera to discharge, the fire was discovered; the cargo of the 'Fanny' was discovered to be on fire when in sight of Valparaiso.

"I venture to suggest, from what I have experienced and heard, that if iron bars were placed vertically in different parts of the coal they would tend to throw off the heat, or at least serve as thermometers or guides to find out where the fire is."

I have, &c.
J. Grierson,
H.B.M. Consul.

The following Reports of Statutory Inquiries held by the Board of Trade into cases of Gas Explosions on board Coal-laden Vessels since 1868 have been forwarded by the Board of Trade to the Royal Commission for inquiring into Causes of Spontaneous Combustion of Coal in Ships.

THE "ZENO."

Board of Trade Inquiry into the total loss of the "Zeno," held at the Police Court, Borough of Kingston-upon-Hull, 11th June 1873.

Before Walter Wrangham, Esq., Police Magistrate.
Captain White, R.N.
William Darley, Esq., Naval Architect.

Mr. Saxellye appeared on behalf of the Board of Trade.

Mr. John Hearfield appeared for the owners, Messrs. Thomas Wilson, Sons, and Company, and the Master, Edwin Owen.

The Master's and Chief Officers certificates were handed in.

Official number of ship, 65252. No., date, and port of registry, $\frac{72}{1871}$, Hull; screw. Built at Hull in 1871 by Earle's Shipbuilding and Engineering Company, Limited, Hull. One deck and poop; two masts, brig rigged; Clench built; length, from fore part of stem to aft side of head of stern post, 272 ft. 6 tenths; depth in hold, from tonnage deck to ceiling, 17 ft. 7 tenths; gross tonnage, 1,490·21; registered tonnage, 1,163·96.

Thomas Booth, of 14, Brook Street, Hull, late second mate of the S.S. "Zeno." I joined the ship in November 1872. In December last I was promoted to be second mate, and was second mate at the time of the accident. I believe the ship was a first-class ship; hailed from Hull; was manned by 29 or 30 hands, all told; she was brig-rigged. She carried six boats—two lifeboats, two large boats carried on the bridge, a gig, and a jolly boat. In the early part of May last the vessel was in the Penarth Dock at Cardiff where we took in a cargo of coals about 1,900 or 2,000 tons. I believe they were steam coals. We completed the loading at 1 a.m. on the 10th of May 1873. We got out of dock between 4 and 5 p.m. on the same Saturday afternoon, there being then a dense fog at the time. We went into the Penarth Roads, and laid there until Sunday morning. I believe we got under weigh about 3 a.m. The vessel proceeded slowly in consequence of the fog. The fog continued until the Monday afternoon. At 6 a.m. on the 12th May the vessel was stopped to take soundings, and the course was then shaped to Cape Finisterre. When the fog cleared away and we proceeded at full speed. We had some sail set, and it was so set until the morning of Tuesday, the 13th May, when the accident occurred. At 8 a.m. on the 13th May (Tuesday) I was relieved by the chief mate, the weather being fine, going at full spead, and being under steam and sail. I went below, had my breakfast, and went to my berth; was reading in my berth, which is down below amidships, against the entrance of the saloon. The vessel is divided into compartments: No. 1 was the forehold; before that was the peak—it was divided from the forehold by a water-tight iron bulkhead. The main hold was No. 2, and was divided in the centre by a wooden bulkhead. The aft part of the mainhold is called No. 3, and that is divided from the engine-room by an iron bulkhead and the engine-room is divided from the after hold, which is called No. 4, by an iron bulkhead. The holds were ventilated by the hatches. No. 3 was ventilated by opening the hatches on Monday. The trimmers went down to trim the coals; no lights were taken down, there was sufficient light from the opening. The other hatches were closed on the Sunday, battened down with canvas in the usual manner, and continued so until the time of the accident. No. 1 was battened down the last. We have printed rules on board the vessel, issued by Messrs. Wilson. Rules produced marked "B" are the rules. I have looked at them, but not carefully. They were in the mate's cabin for me to look at if I was so minded. I have seen two copies of the rules on board the ship. I have never heard the rules read over by the captain or chief officer; they may have been. The crew had access to the rules if they chose to ask to see them. I was aware of rule at p. 13 as to naked lights.

While I was reading in my berth as before mentioned, about 9.15 a.m., I heard an explosion in the vessel. There was a tremulous motion, accompanied by a dull rumbling sound. I went on deck forward. I saw that four of the iron hatches of the No. 1 hatchway had been blown away; the remaining or fifth hatch being then laid on the port side of the vessel. There was a derrick boom which was 10 feet above the deck; it had been broken in two pieces. The fore topsail and the stay foresail were both shivered, holes blown into them. I went to the captain to hear what were the orders. He ordered us to get some of the boats ready, and we got three boats ready, and while the men were getting the boats out he ordered other men to get a sail out. I mean by getting the boats out, swinging them over the side, and lowering them to within 3 or 4 feet of the water, so as to be ready. The sail was to be put over the port bow to see if it would stop the water coming in. I had previously looked into the hold, and saw a large hole had been made in the bulkhead between the peak and No. 1 hold. I saw Bennington, a seaman, on board on deck. He was very much injured, burnt black in face, head, and hands. All attempts failed to keep the vessel afloat. We got into the boats. There were two steamers in sight, the "Cairo" and the "Brighton." The "Cairo" was outward bound, and the "Brighton" homeward bound. The enginemen were first put on board the "Cairo." They were subsequently transferred and all hands to the "Brighton," and we were all taken to Liverpool, where we arrived on Friday the 16th day of May. A quarter of an hour after we left the "Zeno" she went down head foremost in deep water in the Bay of Biscay, in about 40 minutes after the accident. Independent of the accident which happened to three or four of the men all the crew were saved. We had on board the usual signal and cargo and hold lamps. The cargo lamps had glass round, and vent holes at the top. They were not what we call naked lights. The cargo lamps were used by the labourers in taking in and discharging cargo. In addition to these we had two or three lanterns, and there were several lamps that had no frame to them, naked lamps, spirit lamps. These lamps (spirit lamps) were used in the engine-room; in the bunkers for trimming, and one in each forecastle—firemen's and seamen. It is common to use that description of lamp in all steamers that I have gone in. These lamps are usually hung up in the forecastle at night. I have been in four steamers, and they have been used in all. When we were loading the vessel at Cardiff candles were used. The trimmers required candles to be used. The trimmers formed no part of the crew. The accident happened in the store room, which is immediately under the forecastle in the bow of the vessel. I have been on board vessels laden with coals several times before, and never knew an accident happen of that description. I have never been on board a vessel laden with Cardiff coals. It has not been usual to take off hatches in vessels I have been in before laden with coals. I have known the hatches remain on, I can say with certainty, for a month.

Cross-examined by Mr. Hearfield.

I have been at sea between five and six years, and have been in ships before the "Zeno," but never in a finer ship. She was well found in every respect. I think it was not possible to suggest any improvement in her. I have sailed with Captain Owen since I joined the "Zeno" in November last. The captain is exceedingly attentive and careful. The ship was clear of the Bristol Channel out at sea when the hatches were battened down. I have never seen the rules of the Penarth Harbour, Dock, and Railway. I find the rules now produced marked E contain a rule, No. 18, as follows:—"The hatches of every vessel laden with coal " must be left open during the whole of the time such " vessel stays in the dock, basin, or harbour, and must not " be closed until such vessel has passed through the sea " gates, or from the harbour on her way to the channel." I say, considering that rule, that the hatches are to be closed after leaving. As a seamen, I know it is necessary to close the hatches as soon as a vessel leaves port. As a rule it would not be safe to proceed to sea without doing so; it would depend upon the weather. As a rule it is rough in the Bay of Biscay. I should close the hatches going into the Bay of Biscay. I consider it to be the duty of the chief officer, mine, and the carpenter's, to see to the closing of the hatches, and the ventilation of the cargo. The vessel left Cardiff on the Sunday, and it continued foggy until the Monday afternoon. It requires constant supervision on the part of the master in a fog in the English Channel. The naked light was in the store room. I consider the rules specially made to provide against fire. In my opinion it would require a "Davy" lamp to provide against an explosion. Sailors could not be got to use a Davy lamp; they will hardly use lamps. We have fires in the forecastle. I don't see why they should not be as dangerous as lamps and candles. At the time the vessel blew up I had no reason to believe there was any escape of gas in the vessel. I was not aware that Cardiff coals had any special liability to emit gas. After the explosion happened the captain did everything possible to save the vessel. After the explosion I saw partly the nature of the injury done to the hull of the vessel. The hull was specially damaged in the forepart, near the stem in the port bow. I saw several rivet holes in the side of the ship.

Re-examined by Mr. Saxellye.

The forepeak is separate from the hold by being divided from it by a water-tight iron bulkhead.

(*By Mr. Darley.*) There was a sluice-valve in the forepeak. I don't know whether it was closed; as a rule it is open. I know there is something to let the water out. I know of no means by which the gas could get into the forepeak. I believe the bulkhead to be perfectly water-tight.

(*By Mr. Wrangham.*) I do not know of any orders having been given to take special precautions on account of this cargo of coals on this voyage.

(*By Captain White.*) I hold a second mate's certificate of competency since 13th August 1872. The crew of the ship consisted of a master, three mates, carpenter, boatswain, lamp trimmer, three engineers, seven firemen, steward, mess-room steward, donkey-man, cook, and eight seamen. When we took coals in at Cardiff he, the captain, was mostly on board superintending the loading of the ship. The coals were put on board by a spout. It took from the Wednesday afternoon to the Saturday morning to put the coals in. There were stoppages, of course. The coal that was in the forehold was, I believe, not put in at one time. I should think there would be from 150 to 200 tons of coal in the forehold. There was a space of 8 or 9 feet between the top of the coal and the deck in the forehold. You could walk along the top comfortably. Besides trying to get the sail over the bows, three or four of the hatches of the main hold were lifted, and we started to throw coals over the side of the ship to lighten her. When we found it was no good, the main hatches were battened down again. I never noticed any damage to the partition between the forehold No. 1 and No. 2. If that partition had been in good order I do not think it would have saved the ship. I know of no ventilation pipes down into the hold. I saved part of my clothes and sextant. The reason why I said you could not get a sailor to use a Davy lamp is because they are too careless. If there were two lamps, and one not a Davy lamp, they would take the first that came to hand.

(*By Mr. Darley.*) Even if their lives were dependent upon the use of a Davy lamp, and they were told so, it would be a question whether they would use it.

(*By Captain White.*) The hatches had been closed since Sunday forenoon to the time of the explosion.

(*By Mr. Darley.*) My captain was most certainly a sober captain, a very sober one. They opened and closed the sluice valves from the deck. The carpenter looks after them. As a rule I believe they open by a screw.

(*By Captain White.*) The main hold was full of coal, and in the after hold the lower part was full, and between decks there were bunker coals.

Thomas Booth.

Taken on oath before
Walter Wrangham.

William Vickery, of No. 45, Blackfriargate, Hull, Seaman (A.B.) I was boatswain on board the "Zeno." I have been to sea between 19 and 20 years. The "Zeno" was a screw steamer, brig rigged. She had 29 hands, all told. I joined her last November as able seaman. I was appointed boatswain late in December last. My duty was to forward the officers' orders in carrying on ship's duty in general. I had no night watch on the "Zeno." In the early part of May we were in Penarth Dock, Cardiff, taking in a cargo of coals. I believe they were steam coals. We took in, I believe, 2,000 tons, including bunker coals. We commenced loading on the Wednesday. We were bound for Port Said, I believe. The loading was completed late on Friday night or early on Saturday morning. I cannot say how the coals were put in. We had to shift the ship very often to let the spout come into different holds. I believe No. 2 hold was completely filled first, but I cannot say it was all filled at one teeming. I expect it was filled first, because it is most in the centre of the ship, and we can trim the other holds afterwards better. I think No. 3 hold was next filled, but I cannot say for certain. No. 2 hold was filled completely. No 3, to the best of my knowledge, was filled. No. 1 was partly full; in the after part of No. 1 there was not much space; the top of the coals slanted down, and at the hatchway I should think there was a space of 5 feet from the top of the coals to the combings. I should think close to the bulkhead there was a clear space of 12 feet, the entire depth of the hold being 16 feet. We hauled out of dock on Saturday at 5 p.m.; there was a dense fog. We drew up in the Penarth Roads. On Sunday morning we got out to sea. We sometimes went "dead slow." It was Monday noon before fog cleared. I know we stopped to take soundings once. I was present. After fog cleared off she was put on full speed. We were also under sail. The wind was easterly. The vessel was divided into compartments. I don't know that there was any ventilation to the cargo, except that leading to the bunker holes. There were iron ventilators formed to catch the wind, a kind of a cowl made to shift according to the wind. There were four altogether, for the purpose of ventilating the stoke-hole. The carpenter has charge of the sluice valves. I know of no other ventilation than that I have spoken of, excepting the hatches. After leaving the dock I believe the hatches of No. 2 were battened down. No. 1 was battened down at 6 on Sunday morning. No. 3 was battened down after leaving the dock. They were opened on Monday morning. It was no part of my duty to attend to these hatches. I have seen the book of rules provided by Messrs. Wilson. I have never read them. Without being a printed rule it is a general rule not to allow naked lights to be used in the hold.

(*By Mr. Darley*). On this particular occasion I knew of no verbal orders having been given to be cautious.

Examination continued.—All went well till Tuesday morning at 9, or little before. I was in the paint locker in the fore part of the ship, over the forepeak. On going there I heard Arthur Bennington, seaman, ask the lamp-trimmer, John Andean, for some waste. Andean is now on a voyage in the "Winestead" to Cronstadt. Andean answered it is in the forepeak. I afterwards saw Bennington with a lamp, a common spirit lamp, a naked lamp, in his hand going into the forepeak. That is such a lamp as is used generally in forecastles and engine-rooms for trimmers. Knowing the rule that a naked lamp should not be used, I did not remonstrate with Bennington for this reason, I knew he was not going into the cargo hold, and of course I thought there was no danger attached to it. I had not previously to this time smelled any escape of gas and had no notion that there was any gas about the vessel that was likely to explode. I saw Bennington at that time take up the trap-hatch; he got down himself and was standing on some gear that was on the first platform of the forepeak, only his head was above the man-hole then; he then took the lamp in his hand, and had to stoop. As soon as he brought the lamp over the hole I saw a flame and felt a shock. I was from 6 to 8 feet at that time from the man-hole. I was slightly burnt on the left side of the back of my head. I am now quite recovered. I never was taken to the hospital. Immediately after the explosion, when I was in the paint locker and lamp room, I felt myself partly buried; the fixtures in the forecastle were all blown in confusion. I tried to get on deck, but was a few minutes in doing so in consequence of the smoke and the things being thrown on me and the passage obstructed. I got on deck eventually. I thought that Bennington was still in the forepeak, and told Mr. Taylor, the chief officer. I saw Bennington shortly after on deck aft; he appeared to be much burnt. The master was on the bridge issuing orders. The chief mate was forward looking after things. We heard a noise, and chief mate said "Is that water rushing in." Orders were given, I believe from the captain, to take off the hatches from No. 2 hold; they were taken off; I assisted to take them off. I heard orders given, I expect from the captain, that we were to throw coals overboard, and some were thrown over. I went into the forehold with the chief mate and the carpenter. I found that the fore-bulkhead was blown into the forehold more from the port side; the greater part of it was blown down. Part of the plates were started from the port side; a midship stanchion supporting the foredeck was down. *I saw a* *quantity of rivet holes.* We attempted to fill the rivet holes up by putting in wooden plugs. We remained doing this until we were driven out by the water rising on us. I did not see any plates blown clean away. No. 1 hatch was covered by five iron hatches at the time of this explosion; they were battened down. The explosion blew the whole of them off the hatchway; I saw one of them on deck, but no more. The fore staysail and fore topsail were both set at that time, and both were torn; a derrick boom, five feet above the hatchway, was broken in two. The hatches had been covered with tarpaulin. I saw one of the tarpaulins lodged by the explosion on the fore topmast stay; that was from 30 to 40 feet from the deck. I saw the chief mate and some hands trying to lower a sail over the port bow. I did not assist. I imagine it was to try and stop the water rushing into the vessel. The captain was on deck attending to his duties all this time. There were no deck pumps going at that time; everything that I can judge of was taken to keep the vessel afloat when we found the ship, notwithstanding our exertions, was fast sinking. I heard an order passed to get to the boats. We all went to the boats; some to one boat some to another. After getting into the boats we were ultimately taken on board the "Brighton." The "Zeno" sunk 25 to 30 minutes after we left her; she sank in about an hour after the accident happened. We had general signal lamps, cargo lamps, glass lamps with vent holes at the top. Inloading at Cardiff, in trimming the ship, candles were used; the trimmers demanded them. The trimmers do not belong to the ship. No remonstrance was made to the trimmers using candles.

Taken upon oath before W. VICKERY.
WALTER WRANGHAM.

The Court here adjourned to the 12th June 1873.

12th June 1873.

WILLIAM VICKERY recalled.

Cross-examined by Mr. HEARFIELD.

I have been in several steam ships. I have no complaint to make against this vessel. I never was in a better found vessel. I have not the least complaint to make against the master. I have been before on board coal-laden vessels, with Welsh coals. In those cases it is the rule to keep the hatches open while in port. I have known ships go to sea with hatches off, and I have known cases where you could not proceed to sea with hatches off. The hatches were battened down the forehold on the Sunday morning, and continued so till the explosion. It frequently happens that vessels laden with Welsh coals must have their holds battened up a longer time owing to the weather. I am not aware that the weather has any particular effect upon Welsh coals in respect of their emitting gas. I am not aware that foggy weather makes any difference to them. This steamer had a half deck. The ventilators into the stoke hole passed though the half deck; they are large iron pipes, 3 feet in diameter, perhaps less. They might be 2 feet in diameter; they had bell mouths. I am not certain there was any means of ventilation from these pipes to the half deck. There is a slide in two of them in the between decks. There were also bunker holes besides these. In fine weather these holes are generally kept open. I believe some were open during the two days the vessel was at sea. I do not think the bulkhead could be made so tight as to prevent gas getting through; I think the working of the ship would make some outlet for the gas to escape through. I should think water would also drain through them; that is, a little hole might let through a great deal of gas and very little water. I went into the forehold after the accident. I observed the but straps; the rivets were out of them; the water was pouring through. When I got down below the water had got up to the top of the coals in the fore part. I went below about 10 minutes at most after the explosion. During that 10 minutes a large quantity of water had got into the vessel. I could not see the whole quantity of water coming into the vessel; I saw it pouring in at different places. From what I saw I should think she was damaged below principally. I saw some of the rivets clean out from the but straps. It would follow from that, that the plates must have been bulged out at the junctions. The forepeak and the No. 1 hold had iron decks formed of iron plates on angle iron formed in the usual way, therefore I should think that the decks would be of the same strength as the sides; at all events the deck would be very strong. I did not examine whether the aft bulkhead of No. 1 hold was damaged. Now, judging from what took place after the explosion, and considering the time we had to do it in, I think that everything was done that could be done to save the ship. I have the charge of the forepeak. I was last in the forepeak before the time of the explosion on the Monday forenoon. I had a light with me then. I did not observe then any particular smell of gas.

Re-examined by Mr. SAXELLYE.

The half deck had coals in it. The slides in the ventilators I have spoken of could be opened. I cannot say whether they were or were not open.

(*By Mr. Darley.*) The holes in the ventilators were closed with slides, about 2 feet broad and more than 2 feet in height.

(*By Captain White.*) I suppose I have been six or seven years in the coal trade. From leaving Cardiff to the morning of the 13th we had thick weather and a rolling but not a broken sea. The hatches were off No. 3 hold. I don't know of any reason why the hatches of No. 1 hold should not have been taken off from the Sunday morning at 6 o'clock to the time of the casualty. On the morning of the 13th at that time I should think the ship was going between eight and nine knots an hour. When we left Cardiff I should think that the freeboard of the ship was about 4 feet. The ship had not a great deal of sheer. I should call it a moderate sheer. I consider the cause of the explosion was a man going into the forepeak with a naked light. I cannot say that the rush of gas would be into the forepeak. The top-gallant forecastle deck was started. When the hatch was taken off the fore peak by Bennington I did not smell any gas. I should think that the reason the master ordered the hatches to be taken off the second hold was to let the gas escape out of that hold.

(*By Mr Darley.*) The master did not explain to me who he ordered the hatches of No. 2 hold to be taken off.

(*By Captain White.*) I did not at any time before the explosion during the voyage hear any orders given that the hatches were to be taken off. I should think that the reason they did not get the sail over the bows was because they could not get weights to sink it in so short a time. The rivet holes I saw open were in the but straps in the forehold. When the plugs were put in they filled the holes partially. I should think it was between 30 and 40 minutes from the time of the explosion before we left the ship.

(*By Mr. Wrangham.*) When we went to the boats, it was all hurry, but there was no confusion.

WILLIAM VICKERY.

The above deposition was taken upon oath before
WALTER WRANGHAM.

JAMES ALEXANDER KIDD. I live at No. 3, Fawcett's Terrace, Hessle Road, Hull, Marine Engineer. I was chief engineer on board the "Zeno" in the month of May last. I hold a first-class certificate for competency as marine engineer. The "Zeno" hailed from Hull. I joined the "Zeno" at London in July last. When we left Cardiff on Saturday afternoon, May 10th, we came to anchor, and banked fires. I know that all coals coming from that quarter are of a most gaseous character, but not to the extent there proved to be. We sailed on Sunday morning, and all went well until the Tuesday morning at 9. I had just left the breakfast table. I had got to my cabin, and felt a shock. My cabin is just over the engine, abaft. I listened for a moment, but hearing the regular beat of the engine, I came leisurely out, and saw some of the men running, who told me it was an explosion. I immediately went below to the second engineer in the engine-room to tell him the cause of the shock, and to tell him to stick to his post while I went and communicated with the captain. I went to the captain and spoke to him under the bridge, where he was issuing orders. The captain ordered all pumps to be put on. I mean the steam pumps. The captain had ordered at the moment of the catastrophe almost, by telegraph, that the engines should be eased. That was before I went on deck. When I returned from the captain, according to his orders I stopped the engines. I ordered the doors, smoke-box, and fire-doors to be opened, and blew off steam, and got all the men on deck. I was the last to leave the engine-room. I told the captain I would remain in the engine-room till he called me, and begged of him if he had to go not to leave me. There was no water coming into the engine-room before I left. We had sluice valves on board the ship. All the valves were closed. It is the rule to keep them closed. We never use them. I have another means of drawing the water off from the holds by means of pipes. The sluice valves were under the control of the carpenter. He would not open them without my orders. I am sure they are all water-tight. I have been seven voyages in that ship carrying wheat, and we never damaged a grain. I left the engine-room when it was determined to leave the ship. The captain, chief officer, and myself were the last to leave the ship. I had merely looked down into the hold. I saw the water coming in. The captain wished me to remain at the engines. I saw Bennington; he was much burnt. Three others were burnt. They were all well attended to. It is a general rule at sea no naked lights are to be used. I never saw the rules of Messrs. Wilson on board the "Zeno." Closed lamps and open lamps were used on board this ship. We had common spirit lamps. The use of a naked light on board a ship depends upon the kind of cargo we have. With some cargos, if a common seaman saw a naked light he would sing out himself. We left the ship 40 minutes after the accident. From the time of the occurrence to the ship going down was 50 minutes.

(*By Mr. Darley.*) We had hand pumps to every compartment, and all were shipped when they underwent survey by the Board of Trade. Two bilge pumps attached to the engine-room, communicating with every part of the room, worked by steam. We had a bilge injection we did not use. The engine-room was dry.

Re-examined by Mr. HEARFIELD.

I have no complaint to make against the ship or the master. I am sure he did everything to save the ship. From habit I have got somewhat accustomed to coals. I can pretty well tell their quality, but may still be deceived. I saw the coals put on board the "Zeno." I had no reason specially to believe that these coals were of an especially gaseous nature. I have before been on board a ship laden with coals at Penarth Dock, I think from the same drop. I have on such occasion laid in Penarth Roads when it has been blowing a gale of wind for more than three days with the hatches on. When we get to sea it is sometimes necessary to keep the hatches on for more than a week together. This ship was ventilated as vessels are usually ventilated, and better than a good many I have been in. When the ship first began to make water, if the sluice-valves had been open, I could have told by the manner in which my pumps threw the water out; the ship being down by the head the water ran down that way. Engine was 200-horse power registered. At the time the ship went down the engine was stopped. She was a strong and good ship.

(*By Mr. Wrangham.*) I have never known anything of the rules of Messrs. Wilson; there was not a copy in my cabin. The strictness as to lights depends upon what cargo we are carrying. With some cargoes common sense will tell a man not to use a naked light. The men themselves often call out, "No naked lights there." I am inclined to the opinion of the second mate that a Davy lamp would be of no use. If I had been captain of the ship I should have had the hatches down on the Sunday and Monday; the weather was such I should not have had the hatches off the forehold. I mean that as regards the cargo the ship was better ventilated than many ships I have been in. The hatch being off, No. 3 hold ventilated the whole of the after part of the ship. That was the only means of ventilating the hold where the cargo was; I mean by taking the hatch off. The forehold was not ventilated.

(*By Captain White.*) The ship drew on leaving Cardiff 15 ft. 16 in. forward and 18 ft. 8 in. aft. The ship had about 4 feet freeboard. If I had seen a man going into the forepeak with a naked light, as my business does not lay in that part of the ship, I hardly know what I should have done. If this accident had not happened and a man had been going down into the forepeak with a naked light I should not have thought that there was any danger, as so short a time had elapsed. The coal was dropped into the ship from a spout; the ship had to be shifted several times. I know they put in the coals very leisurely by driblets. The captain was at the ship when the coals were shipped; he was superintending generally. The Board of Trade survey was made of the "Zeno;" I cannot say when. It was in Hull, and since last July. At the time of the casualty we were averaging about eight knots. The coal I used for my fires was the same coal as the cargo. The coals were of good steaming quality. I did not observe it was gaseous; it was a hard, brittle Cardiff coal; it breaks all to pieces if hit by the hammer; it has the reputation of being of very gaseous quality. I have pipes connected with every part of the ship connected with my steam-pump. I did try to pump the water out of the forehold, but it did not draw freely on account of the head of the ship being down. The pumps were in excellent order; they did not pump as much water as I expected, owing, I expect, to the inclination of the ship. The rose of the pipe must have been covered. The pump was put on.

(*By Mr. Wrangham.*) The agent for the ship at Cardiff, Mr. Bovey, never gave me any caution relative to the coals, or stated that they were green coals. He never said a word to me about them.

J. A. KIDD.

The above deposition was taken upon oath before
WALTER WRANGHAM.

Police Court, 13th June 1873.

JAMES ALEXANDER KIDD recalled.

(*By Captain White.*) I had charge of the pump leading into the forehold from the engine-room. I had not seen the rose for a month or two; it would be two months prior to the accident. I am sure the pipe was clear, because I pump her out regularly every hatch. It is possible the pipe may have got damaged in the explosion. I should think it would be liable to be so. The rose was right down by the side of the kelson, right against the bulkhead. I was pumping from the forehold. I don't think the coals had choked it up. It would have taken two or three minutes to have lifted the sluice valve. The ship sank before the water could have run aft even if the sluice-valves had been lifted. The sluice-valves were about four inches square. It is impossible to save the ship by pumping.

J. A. KIDD.

Taken upon oath before
WALTER WRANGHAM.

GEORGE EDWARD COULTAS, of No. 4, Brigham Terrace, New Adelaide Street, Hull, ship's carpenter. I was carpenter on board the "Zeno." I joined her on the 9th day of October 1871, immediately after she was launched, and have been in her ever since. She was a first-class ship, and well found in every respect. We took in a cargo of coals at Cardiff in May last. No. 2 hold was filled up first, then No. 3 hold was partly filled, and then No. 4 hold was partly filled, then the forehold No. 1 was partly filled (she had no more put into No. 1). After that No. 3 was filled up as much as required, and then No. 4 was filled up, that completed her lading. We began on Wednesday and finished on Saturday morning. After we got under weigh and left the roads the hatches of No. 1 and No. 4 were off; that was on Sunday morning between 3 and 4 o'clock. No. 1 hatch was battened down between 7 and 8 on Sunday morning. No. 4 hatch was battened down just before dinner on Sunday morning. There was a nice breeze, and a bit of splash on. When all the hatches were battened down we had no ventilation in the holds. Nos. 2 and 3 hatches were battened down on the Saturday night after leaving the dock. I asked of the mate if there was anything more to go down, and he said "No." It was then my duty to batten them down, and I did so. No. 3 hatch remained battened down till 8 o'clock on Monday morning, the 12th of May. By the captain's direction I took two hatches off No. 3. It was done for the purpose of giving ventilation, and likewise of giving light to the trimmers. Those hatches remained off until the ship went down. On the Tuesday morning I received orders at 8 o'clock from the mate to take one or two hatches off all the other hatchways. After they had finished washing decks down the accident happened before I could carry out the order. I saw one of the trimmers go down one of the bunker holes on the poop deck on the Sunday night. He had a light with him. It was a common spirit lamp. I was never furnished with a copy of the rules furnished by Messrs. Wilson. I have seen them laid in the mate's berth. I have never read them or heard them read. The rules are not read on board the ship by the captain or chief officer. I am not aware of a rule that no naked lights are to be allowed in the hold for any purpose. I have been six or seven years to sea. I am not not aware of any rule on board ships as to the use of naked lights. I did not think it necessary to warn the trimmer when he went down the bunker hole. The bunker holes have lids, and are watertight. The men when they go below in the bunkers light their lamps between decks by striking a match. They generally leave their lamps below. They blow them out. On the Tuesday morning at 9 o'clock I was seated; I had finished my breakfast. I was seated in my berth in the forecastle. I saw Bennington come past. He was perfectly sober. He came on board at Cardiff in a boat after we got out of dock. Sometime after that he jumped overboard. When Andean told Bennington he could get some waste in the forepeak, Bennington asked Andean if he should require a lamp, and he told him he could either take a lamp or go without one, as he himself could get the waste without a lamp. Bennington took a lamp, a common spirit lamp. I was below, near the lamp locker then; when I got there he had got the trap hatch off, and was stood below. His head and shoulder were above, out of the hatchway. The hole is about 2 ft. by 18 in. The lamp was on the forecastle deck. He was drawing the lamp towards him when I got up to him, and when he got the lamp to the hole the explosion took place in the store room, which is part of the forepeak divided from it by a temporary platform. To get down into the forepeak we have bodily to lift up part of the flooring of the store room. The floor is all loose. I was about 3 feet from the edge of the man-hole at the time of the explosion. I was burnt on the right cheek and left arm. I was afterwards taken to the hospital at Liverpool, and remained there four days. At the time of the accident Bennington dropped down below. Shortly after I saw him on the deck; he appeared to be very much burnt. His clothes were partly singed. He could walk and was conscious, but not take care of himself. We had sluice valves on board the vessel. They were under my charge. They were all closed at the time. They were closed since we left Cardiff. As a rule they are always closed. After the accident I got out of the forecastle as soon as possible and went aft to the main deck. The captain gave orders to get the two quarter boats ready. I went aft and assisted at the port quarter boat. The captain came aft and ordered me to go forward and see what amount of damage was done. When I went forward the smoke the explosion had caused had cleared away. I went down into the forehold. I found that the forepeak bulkhead was blown down into the forehold. It was blown down about level with the box stringer. The box stringer is composed of several plates and bars of angle iron, about 18 inches broad, and it runs fore and aft the ship on both sides of the ship, and the one on each side of the ship is made fast to the side of the ship for the purpose of strengthening her. The forebeam was blown away from the port side, and the plates started from the stem. One of the large iron stanchions were blown down from the deck. I saw some rivet holes. Three of the straps from the buts of the plates were blown clean off. The rivets were blown out of some of the plates, and the plates were started on the stem. There had been five iron hatches on No. 1 hatchway; four of them had been entirely blown away. The remaining one was laid on the deck. I observed the topsail and forestaysail were split. The derrick boom was broken in two. The forecastle deck was started up two or three inches. One of the beams was bent up. I reported the injuries to the ship to the captain He ordered No. 2 to be unbattened, and the coals to be thrown out. Six or seven men went into the hold for that purpose. I went to the forehold and began to saw off wooden plugs, and the boatswain and the chief mate and another man or two who were in the forehold, began to fill up the rivet holes. We succeeded in filing up a good many of them. We remained there a quarter of an hour until the water drove us out. It was coming in pretty fast Coming up out of the forehold we saw some men bringing a sail to put over the bow. We got it over. It was not weighted. It had very little effect upon the vessel. I did not take the draft of the ship on leaving Penarth. I succeeded in saving part of my clothes. My berth was the first berth on the starboard side of the forecastle. Water was at that time in the forecastle up to the anchor lid. Shortly after the explosion I sounded the forehold and found 8 or 9 feet of water. I did not sound the main hold on account of not being able to get to the sounding pipe, as it was covered with coal. Before we went to the boats I and the chief mate made an attempt to put on the hatches of No. 2 hold, but did not succeed. We remained on board after the explosion took place 30 or 40 minutes, We were obliged to leave the vessel in consequence of her rapidly sinking. All the crew got in the boats, and independent of the injuries some of them had sustained, none of them were drowned. I saw the vessel go down. That was about an hour after the explosion. She went down head foremost in deep water. The crew was taken to Liverpool. We had the usual signal-lamps, cargo-lamps, and several spirit lamps. When they went into the hold, to the best of my knowledge, they used cargo lamps generally, either at sea or in port. In loading and unloading it is usual to use cargo lamps. Candles were used in loading with these coals. I do not know why. Labourers load the vessel. It was the case in this instance. No one remonstrated with them for using naked lights. I knew the coals taken on board were called steam coals. It was never intimated to me or in my presence that steam coals were dangerous coals. I have sailed in vessels with coals before, but not Welsh coals.

Cross-examined by Mr. Hearfield.

I have no fault to find either with the ship or captain.

George E. Coultas.

The above deposition was taken upon oath before

Walter Wrangham.

Police Court, 13th June 1873.

George E. Coultas recalled.

(*By Mr. Wrangham.*) The sounding pipe only came up to the between decks; the pipe did not come up to the upper deck. There were coals in the between decks and over the top of the sounding pipe which was covered with a brass cap. That was the reason I could not sound. It was only the sounding of the mainhold that I could not get at. I never heard that steam coal was dangerous. I had no information from the authorities of Cardiff about keeping the hatches off, and I never heard of the dangerous nature of this cargo. I know as a fact that the hatches were open all the time until we got out of the sea gate.

(*By Captain White.*) It was not in every case usual to carry coals over this sounding pipe. On taking in a previous cargo I have mentioned to the mate, Mr. Taylor, that I cound not get at the sounding pipes on account of the cargo being over them, but I did not do so on this occasion. As a carpenter, I do not think it to be a proper thing for a ship to proceed to sea with the sounding pipe covered. I saw the hatch put on No. 1 hatchway on Sunday morning, between 7 and 8 o'clock. To my knowledge it was not lifted from that time to the time of the explosion. There was no ventilation into No. 1 hold at all during that time. I suppose the mate's reason for ordering me to take the hatches off on Tuesday morning was for ventilation. No. 1 hold was partly filled forward; the top of the coals would be 12 feet from the deck, aft about 5 or 6 feet. No. 2 hold was full, No. 3 was full, and No. 4 was full. Over No. 3 between deck we had about 20 tons. We had no other open spaces that the coal could be put in. We could have carried 200 tons of coal more if they had been put in the sides of the ship in the between decks. And there would be about 30 tons of coals over the between decks of No. 4 hold. These coals were not placed in the forehold on account of the trim of the ship. I know all the pumps of the ship. There were two pumps in the forehold, one leading to the deck and the other to the engine room, they both drew from the after bulkhead. There were two more pumps in the main hold, one leading to the deck and the other to the engine-room; and there were two more in the after hold, one leading likewise to the deck and one to the engine-room. These leading to the engine-room were steam pumps. We had not a portable pump in the ship. When we were driven out of the hold by the water there would be about 10 or 11 feet of water in the hold. If the pump in connexion with the engine-room was on it ought to have thrown water. If it was on I cannot account for its not throwing water. The hand pump was never put on to the forehold. I suppose there was not sufficient time to do so, every man being otherwise employed to the best advantage. It was my duty to rig the pump, and I was othewise engaged. I did not rig it because I was ordered to do something else. It would have taken me three or four minutes to have rigged it. I saw the rivets drawn out of the side. I suppose that was done by the force of the shock. If the rivets were good they would draw and not break off. The mate ordered me to put the hatches on the No. 2 hold. I suppose he thought there was a chance she might not not go down, and it was to prevent any water that come on the deck getting into the hold; if she did not sink we might have got a ton. I don't think if the sluice valves had been opened and the water in the forehold allowed to run off into the engine-room, and the pumps put on, that it would have done any good, she was making too much water. The first boat, the jolly boat, was out in three minutes. We got the long boat, the cutter, out; we did not get the life boat out. I was not present all the time when they got the cutter out. I remained there about three minutes. There were three or four men assisting me.

(*By Mr. Darley.*) It was my duty to examine the roses of the pumps leading to the engine-room, and the deck pumps. I did not examine them on this occasion previous to going to sea. They are protected from coal underneath the limber boards. I had examined the roses about 10 or 11 days before going on this voyage. I found them then in good condition. There was no accumulation there likely to choke them. I saw some of the rivets that come out. They were on the box stringer. They were drawn, and I infer they were good rivets. I consider it a proof of good work. The but straps were bent, not broken. None of them split across that I noticed.

(*By Mr. Wrangham.*) There would have only been three or four tons of coal to be shifted to enable me to sound down the sounding pipe of the main hold. I cannot tell how long it would have taken them to have shifted them. There was no other means of sounding. I could not have sounded down the pumps.

George E. Coultas.

The above deposition was taken upon oath before

Walter Wrangham.

Arthur William Bennington, of No. 34, Sutton Street, parish of Holy Trinity, in the borough of Kingston-upon-Hull, able seaman. I was an A.B. seaman on board the "Zeno." I joined between the 12th and 15th of last September. The vessel was at Cardiff in May last. I was paid off on the 8th May. I signed again on the 9th, and joined her again on the 10th, at Cardiff. I went off in a small boat to her in the evening. We got under weigh on Sunday morning between 3.30 and 4. I was not sober when I got on board. I jumped overboard. I suppose I wanted to get ashore. I got in again from the pilot boat. The vessel started in her voyage at 4 a.m.; it was foggy; she went slow. On Tuesday morning, the 13th of May last, I came on deck at 8 o'clock. I relieved the second mate's watch. After washing saloon deck I was ordered by the third mate to clean some brass work. I went into the forecastle to ask the lamp trimmer for his keys of the lamp-room to get some waste and oil to clean the brass with. There was no waste in the lamp-room. I told lamp trimmer. He said there is a bag full down in the forepeak. I went to the peak, took the hatch up, looked down, and saw it was all dark. I went into the lamp-room, and found a spirit lamp burning in the lamp-room. I took the lamp out of the lamp-room, and set it down on the deck whilst I got down the man-hole into the peak used as a storeroom. I was stood in the hole. The The carpenter and boatswain asked me what I was doing down there. I told them I was going to get some waste. It was a naked lamp. They never said a word to me at all about the lamp. I took the lamp, and as soon as ever I got it over the man-hole there was an explosion. When I came to my senses I found myself at the bottom of the peak, some 15 or 18 feet deep. The temporary deck on the storeroom was all blown away and laid about me. When I came to myself I found myself badly burnt on both arms and on the legs just above the boot tops, and all my hair but one tuft was burnt off. My face was burnt. My moustache and whiskers were burnt off. My clothes were a mass of ashes. My jacket fell to pieces. I had no trouble to pull it off. I must have remained insensible a few minutes. I got on my feet, and fell to the side of the ship. I managed to climb up to the upper part of the box stringer of the peak; then I felt my way aft to the starboard side of the bulkhead. I found the man-hole blocked up. I sung out for help, but could not make any one hear. I sat down on some of the gear, and by and by the smoke began to go away, and then I saw a light. I then went along the box stringer, and made for this light. I found the rivets blown out, and I got some fresh air by putting my mouth to them, which revived me. The water was also coming in fast. I went back again on the starboard side to see if I could get through the man-hole again. I could not, and I went into the wing of the bulkhead on the starboard side, and at last I found a plate of the bulkhead blown into the forehold on the port side. I got through and went on the box stringer as far as the hatchway, and then I got on the coals. The hatches were all off. There was plenty of light. I got out by the hatchway by myself. The coals were high enough for me to get hold of the combings, and I got out. My injuries were at once attended to. The steward dressed my wounds. I saw nothing more of the damage done to the ship. I had never tasted drink since Saturday. The lamps I used were those used generally in a forecastle. There were cargo lamps on board. We had signal lamps. Side lights. I don't think we had any lanterns. I have loaded twice at Penarth and twice at Cardiff with coals. I have had no orders given to me about the explosive character of coals by agent or anyone else. Last cargo of coals we never had a hatch off. We went to St. Nazaire. We were about five days on the voyage. I had no reason to suppose in going down to the hold with the lamp there would be any danger. I never smelt anything. The accident took place between 8.30 and 9 a.m. I did not see the "Zeno" go down. I was taken to Liverpool in the "Brighton," and then sent to the hospital in Liverpool on the 16th of May. I was discharged from the hospital a week last Wednesday. I remained an out-patient for a day or two, and then I came down to attend this inquiry.

Cross-examined by Mr. J. W. Hearfield.

In the last ship I went down into the forepeak with a light. I never went down into the hold of a ship carrying coals with a naked light; I never had occasion. The hatch would be off the man-hole about three minutes before I got a light. The man-hole was round; it would be about 18 inches in diameter. A man could easily get down it. After I took the hatch off I went to the lamp-room; the lamp I took was constantly used for going down there. The carpenter was down in the same place with this lamp on the Sunday afternoon. I suppose nobody would think there would be danger to go down into the forepeak with a naked light in that short time. No one but the carpenter and boatswain saw me go down. I never gave a thought about the gas. I did not know that gas coming from coals does not smell at all. All the rivets on both sides of the peak appeared blown out from the stem aft. I could see the water teeming in wholesale through the rivet holes.

(*By Mr. Wrangham.*) The forehatch was battened down between 6 and 8 a.m. on Sunday, and the carpenter went into the forepeak in the afternoon.

(*By Mr. Darley.*) I think the rivets were blown out for 6 or 7 feet, up and down on both sides. The but ends were all started; the buts were separated from the stem an inch or three-quarters of an inch. I did not notice whether it was more so on one side than on the other.

Arthur William Bennington.

Taken upon oath before
Walter Wrangham.

John Bovey. I reside at Penarth, and am a shipping agent. I remember the "Zeno" loading at Cardiff; she completed her loading on the morning of the 10th of May last. She was in the Penarth Dock. I attended as the agent of the owners of the ship. The cargo was taken in their vessel on freight. I am Lloyd's agent at Cardiff, and I have issued a policy for my principals for 2,000*l.* on the cargo in this ship. During the time she was loading I was at the ship, on Friday the 9th of May. I was in conversation with the chief mate and engineer. To the chief engineer my conversation was as to the readiness of the ship. The conversation I had with chief mate was on the spout alongside the ship. It was as follows: I called his attention to the necessity for ventilating the cargo by keeping the hatches off. He was to keep them off from that time to as long as practicable. I stated to him my reason for wishing it to be done. I told him that the coal that was being put into the "Zeno" was fresh wrought (that is fresh from the pits), and pointing to some coal in waggons alongside, I remarked, "Now that coal you see there is very recently from the mine, and will generate gas." I stated that in order to support or strengthen the reason for keeping the hatches off. I know the rules of the Penarth Dock. I refer to Rule No. 18; I have read it. I know why that rule is made. It was made because the parties who made it knew that the coal that would be put in ships at Cardiff would generate gas, and that, unless well ventilated, there was the risk of explosion, which happened in the case of the "Zeno." This rule is framed in the protection of local interests so as to prevent an explosion in the dock. I have been about 16 years connected with coal, and know well the character of Welsh coals. I have known an explosion take place in a ship whilst loading with Welsh coals. The accumulation would only take place where there was no ventilation. I should think there would be great risk of an explosion in a ship that had received coals into her in the morning if a light was used in the evening, if there had been no ventilation. Strangely the gas generated by Welsh coals is inodorous. Fresh-wrought coal will give off more gas than coal that has been exposed if both were stowed away. The engineer did not hear this conversation. It is usual at Cardiff for the trimmers to trim the coals in a ship with candles; they would refuse to use a lantern. Candles are not dangerous, because there is good deck ventilation. They only use candles because they give a better light than lamps. The gas from Welsh coals is rather light than heavy. At the time of my conversation with the mate I explained to him the different modes of ventilating cargoes; that I had not any faith in any other mode than deck ventilation, that is by taking the hatches off and leaving them open. The coals from every colliery in Wales will generate gas, but not one more than another. It is notorious with us that explosions from the gas of the coal are more frequent in foggy weather owing to the depression. When I went first on board the "Zeno" I inquired for the captain. The mate said he was on board, and wished to call him for me to see him. I said it was unnecessary to do so. Considering the mate as being the executive, and in charge of the vessel, superintending the coal being put on board, and in shifting the ship, I could have then seen the captain if I had wished. I never saw the master to have any conversation with him on the subject of the coals being dangerous, or of the same tenor as that I had with the mate. The "Zeno" had exceptional despatch in loading; she was loaded in 50 hours with 1,500 tons, and that would tend very materially to cause an accumulation of gas.

Cross-examined by Mr. J. W. Hearfield.

One of the reasons for my going to the ship was to caution him about the coals. I should have an interest in seeing the ship loaded with despatch. Loading 1,500 tons in 50 hours would be a great despatch with us. I had given the freighters twice as much time to load in. The sailors would see the trimmers carrying naked lights, and it would very likely put them off their guard. I had no conversation on the subject with anyone but the mate.

There is no fear of an explosion without a light. The gas does not affect the sense of smell, or the lungs in breathing. I have no recollection of telling the mate that it was usual for the shippers of coal to give notice that the coal was dangerous. There are no painted notices, or notices of any kind posted about on the dock sides giving notice of the dangerous qualities of the coals. I think it would be very advisable if it were so. I had conversation with the shippers as to their not sending the usual notice that masters of vessels are particularly requested to keep the hatches off to avoid any explosion from the gas emitted by the coals. One of the firm stated that to me. They were not in the habit of giving such notice. The firm was Cory Brothers & Co. What he meant to say was they never printed any notices.

Cross-examined by the mate, Joseph Whalley Taylor.

We had the conversation I spoke about 9.30 p.m. We were on the staith. I had about half-an-hour before that seen you on the deck of the vessel. I do not remember you asking me why I would not go down to see the captain. It is very possible. I should explain to you that I was desirous the ship should be loaded as soon as possible, as I wished to carry out the wishes of Messrs. Wilson. We were in conversation the first time on the deck about five minutes. I possibly did ask you if the ship would get off on the morning's tide, and you said, "Yes, as far as the loading was concerned, but the boilermaker had not done with the boilers." I then inquired for the chief engineer, and you directed me to his berth. I was in conversation with the engineer as you left me. I may have been with him 10 minutes, and then I went on shore, and up to the drop head. You came up at my request. After speaking to you, and cautioning you about the coals, which was the purpose for which I wished to to see you, we had a general conversation. It may be that you came up to me for other purposes than what I wanted you for. I always found you attentive to your duties.

(*By Mr. White.*) I do not consider in reality that there is any danger in loading a ship so quickly as the "Zeno" was loaded.

(*By Mr. Darley.*) Our coal is not liable to spontaneous combustion. The coals from Swansea are liable to spontaneous combustion. I had an interview with the captain every day. He used to come up to my office. I always found the captain attentive to his duties. I have the highest respect for him.

John Bovey.

Taken on oath before
Walter Wrangham.

Police Court, Borough of Kingston-upon-Hull,
8th July 1873.

Before Walter Wrangham Esq., Police Magistrate.
Captain White, R.N.
William Darley, Esq., Naval Architect.

Mr. Saxellye for Board of Trade.
Mr. John Hearfield for Captain Owen (Master).

The mate, Joseph Whalley Taylor, appeared in person.

(Master's certificate handed in again.)

Adolphe Pièrre Vassard, of No. 25, Finsbury Place, City, member of the "Polytechnic Society of Paris" and also of the "Chemical Society of Paris," consulting chemist and professor of chemistry in London. I have for five years studied the qualities and descriptions of English coal. I have been in England four years. I now hand into the Court my opinion as to the explosive qualities of coals. I have been requested by the Board of Trade to examine into the quality of coals, and particularly the Welsh coals. I have read the evidence which has been taken on this inquiry which has been furnished to me by the solicitor of the Board of Trade. I there find the explosion took place in consequence of the emission of an explosive gas. I have come to the following conclusions (see exhibit marked "V." annexed). I mean by sulphate of alumina in its crude state, ordinary clay treated by sulphuric acid. The Cardiff coal—hard, brittle coal—contains light carburetted hydrogen. That was the coal on board the "Zeno." The Cardiff coal gives off at 61° Fah. $\frac{3}{5}$ of a cubic foot of carburetted hydrogen gas per ton in half an hour. Cardiff coal exposed to a temperature of 100° Fah. gives off per ton in 36 hours 4 cubic feet and 138 cubic inches of carburetted hydrogen gas after giving off 2 cubic feet of same gas at the ordinary temperature in 12 hours. This gas is inodorous, and at once ignites on the application of a light. A person going into a forepeak where this gas was would not smell it, and it would not affect his breathing—it is not poisonous. He could not, without a light, tell its presence. To prevent explosions of this sort I should adopt a system of ventilation by shafts. I would recommend that coal should be exposed for more than eight days before it is shipped. It would then be not quite so liable to generate so much gas. According to the liabilty of the coals to emit gas they ought to be exposed for several days. The coal ought to be examined before it is put on board to know what quantity of gas it gives off. This could be done by a properly arranged cylinder; the amount of gas in a given quantity of coal could be thus ascertained and the captan of the ship informed how long he could with safety keep the hatches shut. I have long studied the explosive character of coals. I have recommended ventilation as a means of preventing explosions. Coals are a little injured by being exposed to fog. The fog would moisten the coal and render it liable to heat. The fog has no effect upon the coal to generate gas. The reason why more explosions occur in coal mines during foggy weather is because the ventilation is not so good. The effect of fog on coal has a tendency to cause spontaneous combustion and that only after lengthened exposure, and that on small coal. I have examined the drawing produced by Mr. Darley showing how vessels may be ventilated in various ways, and I certainly approve of that mode of ventilation. If this mode of ventilation could be applied to all vessels it would prevent explosions and would be effectual in allowing the gases to escape. The ventilator shown in the drawing carrying air to the bottom of the vessel is of great importance inasmuch as the carburetted hydrogen being lighter than the air, the air admitted into the hold would force the gas upwards. If one of those ventilators had been introduced into the forepeak of the "Zeno" I do not think the accident would have taken place. The plan produced is complete for the purposes intended.

Cross-examined by Mr. Hearfield.

The emission of the gas in Cardiff coals is caused by the breaking up of the larger fragments, and therefore opening the little cells in which the gas is contained. Each cell contains gas. It is the friction or leakage which liberates the gas, and burning also liberates the gas; that gas can be smelled. The gas contained in the Cardiff coal is carburetted hydrogen, or by a simpler name, "marsh gas," and is the same as "fire damp" in mines. Coals newly taken from a pit will emit a very large quantity of gas. About eight days ought to elapse before Cardiff coals should be put on board ship after being taken from the mine. If shipped earlier I should call them green coals. A captain of a ship would not know what were green coals; he ought to have been informed of this, especially as to Cardiff coals. If these coals had been kept eight days they might have been shipped and the hatches kept down for 30 hours with safety. The captain ought to have known more than the mere fact that they were Welsh coals; he ought to have been told they were green Welsh coals. In my opinion it is not proper to ship Cardiff hard, brittle coal at any time, after any amount of exposure. It would not be safe to ship them at all. I think the effect on these coals was caused by the action of the ship. Any kind of coal is liable to break by the motion of the ship. If there was no motion of the ship there would still be an emission of gas. The ordinary motion of a vessel at sea will pulverise coal. If these coals had been in perfect condition when shipped, I should not have advised the captain to take off the hatches during a fog. In case the coals were shipped in good condition on the Saturday, and the hatches were closed on the vessel's sailing on the Sunday, and a fog came on and continued during Monday, and the hatches were opened on the Tuesday morning, I should say they were opened as soon as they ought to have been. I never heard of a dry fog. There is always fog in the atmosphere, but you cannot see it. As soon as you see it we call it fog. If the hatches were left off the fog would saturate the coal; it would affect the coal worse than rain. It would not be proper for the captain of a ship to keep open his hatches during rainy weather; during foggy weather to do so would be much more improper. In my opinion it all comes to this, that these coals were shipped in an improper condition—too green from the colliery. Independent of experiment or notice given, the captain or owner would have no means of knowing the dangerous nature of the coals. If the light had been encased no explosion would have taken place. If the lamp had been supplied with air from the bottom of the lamp an explosion would have taken place. Practically there would be no difference between an ordinary ship lamp (encased) and a naked light. It would require a Davy's lamp to prevent an explosion, just as much as in a coal mine, if proper ventilation was not given. Even if vessels were properly ventilated as shown by Mr. Darley, I should never advise coals to be shipped until they had been several days from the pit.

(*By Mr. Wrangham.*) If a ship is loading the breakage is very great, and if a man were to go into the bunker with a naked light, even if the hatches were off, an explosion might take place, as the gases would be then escaping very freely. A ship supplied with Cardiff hard, brittle coal should use only "Davy" lamps.

(*By Captain White.*) I have never heard of coal being washed before it has been shipped. If the coal is wet when shipped, it will give out gas just as soon as if it was dry. The effect of wet on the coals is to produce spontaneous combustion, but this very rarely happens when the coal is in large lumps. I consider the hard, brittle coal of Cardiff to be more dangerous than other coals, owing to its explosive character. The effect of fog on the coals would not render them more explosive. I should consider it more prudent to keep the hatches off than to close them down. The Cardiff coals are not liable to spontaneous combustion. I consider it would be much safer if the cargo was divided amidships fore and aft the ship by planks placed Venetian-wise, so as to admit of the gases passing out. The space should be 15 or 18 inches wide. I also consider that a draught might be made by letting the pipes from the hold be led to the galley fire. These gases are all light gases. There is only one explosive gas, carburetted hydrogen. It is half as light as air. If the ship was fitted with these ventilating planks, all the gases would rise to the surface. If each hatchway was fitted with a pipe one foot in diameter that would be sufficient to allow the gases to escape. This explosive gas of carburetted hydrogen will explode at a heat of 123° or 124° Fah. There are no means of telling when this gas is present, except by applying a light. The coal upon which I made the experiments I do not know how long it had been brought from the mine. It would be better not to ship Cardiff coal at all, but if shipped, I recommend proper ventilation on board.

(*By Mr. Darley.*) I think the loss of the "Zeno" was not from spontaneous combustion, but by the application of the flame to the gas. The humidity of the atmosphere would not have a tendency to soften the coal. If the coals were small and wet at the top it would make very little difference in the rising of the gas. I am of opinion that the gas that found its way into the forepeak was generated in the forehold, making the forepeak into a receptacle for it. The deck and bows being both air-tight and water-tight, and the bulkhead not being air-tight the tendency of the gas to be pressed into the forepeak from the body of the coals, there being no agent to drive it back again, would cause the air to gather in the forepeak. I think the captain acted with great discretion towards the cargo by keeping the hatches on during a dense fog. My opinion is that any kind of ventilation that carries off the gas must be good. We must endeavour to find the best mode. I think that the Board of Trade should make it imperative that a good system of ventilation should be enforced in ships carrying coal. I think the coal owners should be compelled to give written notice to the captains and officers of ships as to the peculiar properties of the coals they ship, and the quantity of gas they are likely to give off in a given time, and the length of time the coals have been wrought from the pit. I think iron perforated tubes laid amongst the coals would be injurious as the water might come in contact with the iron, and produce hydrogen by its electric affinity, which is highly explosive. If a cowl had been supplied to the forepeak, and the fog had come in, the fog being heavy would have descended to the bottom, and driven the gas out, provided there was an escape for it.

A. Vassard.

Taken upon oath before
Walter Wrangham.

Official Report of the Inquiry.

The "Zeno" was a screw steamer, built of iron, and was of the following dimensions, viz.: length, 272 feet; breadth, 32 feet; depth, 16 and 18 broken; gross tonnage, 1,490 tons; horse power, 200, with two surface-condensing combined inverted engines; draught of water forward, 15·7; ditto aft, 18·7; freeboard, 4 feet. She was the property of Messrs. Wilson, Sons, and Co., of Hull. Her estimated value was 33,000*l.* She was insured for 11,000*l.* She was built in 1871 by Messrs. Earle and Co.'s Shipbuilding Company, Hull, and classed 18 years Liverpool. She was brig-rigged, and manned with 29 hands all told. Was commanded by Captain Edward Owen, who holds a certificate of competency. She was divided into four compartments for hold, and the bulkheads, with one exception, were of iron and watertight, and fitted with suitable sluice valves, and arrangements for pumping from each compartment. The ship was well found in every respect. She commenced loading on Wednesday, the 7th of May, in the dock at Cardiff, the cargo taken in being 1,500 tons fresh wrought Cardiff hard brittle coal from the pits, all being loaded in 50 hours. The shippers were Messrs. Cory Brothers and Co. Her loading was completed about 1 o'clock on Saturday morning, the 10th May. She left the dock the same day (Saturday) at 5 p.m., weather foggy. The hatches were closed as follows: No. 2 on Saturday night, Nos. 1 and 4 on Sunday, and No. 3 on Monday. They were not battened down till out at sea. There were fires used in the forecastle, and lights as usual. On Sunday afternoon the carpenter went into the fore peak with a naked light (the same lamp subsequently used when the explosion took place). On Monday forenoon, the day before the accident, the boatswain went into the forepeak with a naked light. In consequence of the dense fog that prevailed the "Zeno" lay in the Penarth Roads until Sunday morning, the 11th May, between 3 and 4 o'clock, when she got underweigh, and proceeded slowly, the fog continuing until the afternoon of Monday, when it cleared away. On the same evening (Monday) the captain gave orders that the hatches should be removed for ventilation on the following morning, which was partly carried out. All went well until the following morning (Tuesday) about 9 o'clock, in lat. 46° 45′ N., and long. 8° 11′ W., when one of the seamen, Arthur Bennington, requiring some waste, was directed by John Andean, the lamp trimmer, to the fore peak for some. Bennington took a common spirit lamp (a naked light), and removed the wooden hatch leading to the fore peak. He placed the lamp on the deck, close to the hole, and got down, standing on some gear, in which position as he stood his head was above the deck. He had to stoop to get below the deck, and, taking the lamp in his hand, as soon as he got it below the deck, an explosion took place, bursting the platform below him and precipitating Bennington into the lower part of the bow, a distance of 14 or 15 feet. He was much burnt, and rendered for some short time insensible. The fore water-tight bulkhead which divided the forehold from the forepeak was blown into the forehold, the deck above disturbed three or four inches, and the pillar to beam carried away. The butts of the plates of the bottom were separated from the stem on each side for a distance of 6 or 7 feet below the water, being open about 1¼ inch. Some of the butt straps were ripped from the port bow, and the plate started, and a great many rivets drawn out. The explosion found vent through the fore hatchway, blowing up five iron hatches in its passage, which in their flight broke the derrick boom into two pieces, and shivered the mast, also tearing the foretopsail and forestaysail. The casualty arose from gas being generated in the lower part of the coal in the forehold, which found its way through some small aperture in the fore bulkhead into the fore peak, where a natural receptacle was formed for it, the greater part of which must have accumulated in much less than 24 hours, and the explosion of which was due to the dangerous use of the naked light used by Bennington. Captain Owen was on the bridge at the time of the accident, and immediately ordered the removal of the remainder of the hatches. A sail was got over to endeavour to cover the aperture in the port bow, but failed for want of weights. Plugs were made and driven into the rivet holes, until the men were driven out by the water gaining on them. The captain, seeing danger, ordered three boats to be swung out at the davits to within four feet of the water. A signal of distress was hoisted, which was answered by the "Cairo" and "Brighton." Finding the openings on the sides of the stem and on the port bow were so large, it was thought advisable to keep the sluice valves closed, and the steam pumps being used without effect, steam was blown off. On all hopes of saving the vessel being abandoned, the whole took to the boats about 40 minutes from the time of the accident (the captain being the last to leave), and were picked up by the "Cairo," outward-bound, afterwards transferred to the "Brighton," the weather at the time being fine and clear. From the time of the accident to the "Zeno" going down head foremost in deep water in the Bay of Biscay was about 50 minutes. The Court, upon weighing the whole of the evidence produced, and aided by the scientific information from Mons. Vassard, have come to the following judgment: that no blame whatever attached itself to the owners, Messrs. Wilson, Sons, and

Company. From the very high reputation of Messrs. Earle's Shipbuilding Company they have the utmost confidence in the substantial construction of the "Zeno" and of her general efficiency. They exonerate Edward Owen, the master, and return him his certificate, the usual notice on shipment of dangerous Cardiff coal not having been given to him. With regard to Joseph Whalley Taylor, the chief mate, they consider he neglected his duty in not informing the captain of the dangerous condition of the coal after having been informed thereof by the agent at Cardiff. His certificate is therefore suspended for three months from the 11th of June. The Court feel bound, however, to point out to the Board of Trade the very dangerous properties of the Cardiff steam hard brittle coal, such as shipped in the "Zeno," as shown by the scientific evidence of M. Vassard, with the view of stringent instructions being given to prevent vessels going to sea without some efficient ventilation. Various means were suggested, amongst which were the following: a comprehensive system of ventilation from the upper deck by pipes, cowls, &c., as described in drawing accompanying, observing that had the "Zeno" been fitted with a cowl and ventilating pipe to the forepeak, this accident would not have occurred. The use of iron masts, uptakes to funnels of steamers, bilts, &c. By dividing the coal fore and aft by planks placed Venetian way, with a space between them, against the pillars of the hold. Also the funnel of the galley fire being utilised by means of pipes leading to it from the various parts of the hold. That each hatchway be arranged so as to admit of cowls being fitted to allow of ventilation when the weather prevents the hatches being taken off. It may be remarked here from evidence that it is more important with Cardiff hard brittle coal to have ventilation than to keep out water.

Walter Wrangham,
Stipendiary Magistrate.

Police Court, Hull, July 9, 1873.

We approve of this report,
E. A. White,
Captain, R.N.
Wm. Darley,
Naval Architect.

THE "BERWICK."

Greenwich Police Court, 26th September 1873.

Inquiry into the circumstances attending the casualty on board the screw steamship "Berwick," on the 9th of May 1873, while on a voyage from Shields to London; made by direction of the Board of Trade by Daniel Maude, Esquire, stipendiary magistrate, assisted by Admiral Powell and Mr. Darley, acting as Assessors.

Minutes of Evidence.

Charles Almond sworn.

I was first mate on board the steamer "Berwick" in the month of May last. She was loaded with coal in the port of Shields on the 8th of that month. She took in 732 tons gas coal. We had carried that description of coal before, a great many times, to various ports on the Continent. She left Shields on the 8th of May, bound for London; on the following morning we passed the Dudgeon light at 4 o'clock, and at 5 a thick fog set in, and the engines were eased half speed in consequence. I was on deck at the time, and at 6 o'clock there was an explosion in the fore end of the ship. I was on the bridge. I saw a quantity of smoke, wreck, and dirt fly into the air, and a man I saw fall on the deck on his head; that man was John Morrison, one of the firemen. I did not leave the bridge, there being a thick fog on. I stopped the engines. The man was shot up into the air about 14 feet. I saw no flame with the explosion. The captain and one of the men went forward to Morrison. I had not noticed Morrison before the explosion; I conclude he was below in the forecastle. I saw some men, two, come up from the forecastle after the explosion, hurt. When we came to London an inquest was held on Morrison. I cannot account for the explosion; we never had one before on board. The ballast tank was empty at the time, the water having been taken out before shipping the cargo. There is a store room, about 9 feet wide, between that tank and the cargo; that room was empty also, with the exception of a few stores. There are two hatchways in the store room. There were two holes in the tank for ventilation, which came into the forecastle; one hole was to sound the tank with, and the other hole was to allow any water that might get on to the top of the tank to run into it. There is a pipe from the tank to the engine-room. I don't know how far the pipe runs. I don't know that it ran as far as No. 3 compartment. There was a cock on that pipe. I don't know whether that cock was open or shut at the time of the explosion; it was under the care of the engineer; he would be the person to look after it. I did not leave the bridge till 8 o'clock. When she arrived in London on the following day, I went into the forecastle and examined the damage, and found that the tank top had been lifted, and some of the beams of the tank were broken; that was on the port side. The pipe that I have been speaking of was amidships. There was a little damage done to the deck on the starboard side. That was all I saw. There was a wooden deck above the tank; that was injured also. That wooden deck was a lining over the iron deck. The two men that were injured were also firemen. They were in hospital, one a week, and the other a little longer.

(*By Mr. Crossman*, who appeared as solicitor for the master and owners.) I have been three years in this steamer on the 9th of last June, and always engaged in carrying a similar class of coal. We make from 45 to 50 voyages a year; and this was the first explosion. There is a fleet of steamers employed in the same trade. Morrison and one of the men that was injured were off duty at the time of the explosion; the third man had just gone down into the forecastle to call them out of their beds. The cargo was stowed the same as usual, and we had fine weather. The hatches had not been put on. The forecastle is a very large one. The top of the companion of the forecastle had not been put on, and the doors were not shut. There is an iron bulkhead between the store room and the forecastle, which runs down to the bottom of the ship, and that iron bulkhead cuts off the tank from the store room. The store room is 9 feet in width, and extends from the deck to the floor of the ship; then there is a wooden bulkhead which cuts off the storeroom from the forehold. The pipe from the tank that I have been speaking of runs under the ceiling of the forehold and under the store room.

(*By Admiral Powell.*) We have two tanks; one is in the middle of the ship, in that we put cargo, and that tank at the time of the explosion was full of cargo. I have no reason to believe that the empty tank was leaky. All the holds had the hatches off, in order to let the gases off. It was the duty of the second mate to see to the stowage of the cargo. We have Davy's safety lamps for the store room, but in the forecastle there is an ordinary lamp and a fire. There was no indication of an explosion in the store room. There was an iron lid to the empty water tank which was screwed on.

(*By Mr. Darley.*) There was only one man-hole in the top of the empty tank, and that was fastened on. It was a square man-hole, and the lid was of iron, secured by screw bolts, and india-rubber packing round the lid. There was a wooden top on the top of that again, so as to make the deck flush. There was no access to the tank by any other means. The cock for emptying the tank was close to the bottom of the ship; it was shut off or opened from the store room by a lever. I suppose that the explosion took place in that tank. I have no reason to believe that the bulkhead was not air-tight. The hatches were off at the time of the explosion. The forecastle is on the top of the tank. The sounding pipe of the tank was not shut. That pipe came out under the forecastle hatch.

(*By Mr. Maude.*) There was a hole in the top of the tank to allow water that might be shipped to run into the tank.

(*By Admiral Powell.*) We had no paraffin or combustible matter in the forecastle. I did not see myself the hole on the top of the iron tank. I was only only informed there was one.

(*By Mr. Darley.*) The second engineer generally took the soundings in the tank. The wooden bulkhead I don't believe was water-tight; that runs right down to the ceiling of the ship. It is a wooden ceiling. I don't know how that ceiling was fastened to the floor of the ship. There was nothing shifting but the limber boards; the limber boards were only on one side, the starboard side, close to the kelson. By lifting the limber board you could get under the ceiling. There are some ventilating pipes in the fore hold; they were not filled at the time of the explosion. We have Davy's lamps in general use if we want to go below.

C. Almond.

The within deposition of the said Charles Almond was taken upon oath before me at the police court aforesaid this 26th day of September 1873.

D. Maude,
Stipendiary Magistrate.

JOHN BAXTER sworn.

I reside at the Tyne Dock, and am a coal trimmer. I was employed to trim a cargo of coals in the "Berwick" on the 8th of May last; they were gas coals, about 700 tons, all gas coal. We warn all strange vessels as to the danger of these coals. We are cautious about all gas coals—about using a light in the hold of a ship. We use a naked light in loading a ship. The second mate had charge of the ship; he had nothing to do with trimming the ship. I was trimming for the owners of the ship. The second mate said she was to be full.

(*By Mr. Crossman.*) We warn foreign ships. The North-eastern Company have had boards posted for the captains of ships not to cover up the hatches when partly filled till she has got in all her cargo. That caution has only reference to gas coal cargoes; that caution is not given to regular traders, because the coals in those ships do not remain sufficiently long, and because they know the nature of the coals. The caution is not given to the masters of the vessels.

(*By Admiral Powell.*) I should understand that the caution applies to the ship while at sea, or in docks.

(*By Mr. Darley.*) There is no great smell amongst the the coals. The smell affects you more about the head. If the hatches are blocked up, the danger would be explosion. I have not known of any such explosion while I have been employed.

JOHN BAXTER.

The within deposition of the said John Baxter was taken upon oath before me at the police court aforesaid this 26th day of September 1873.

D. MAUDE,
Stipendiary Magistrate.

GEORGE SAMUEL BAYLEY sworn.

I am a surveyor of shipping. I surveyed the "Berwick" steamer on the 15th of May last. I was requested to survey the ship by the coroner who was holding an inquest on the man who had been killed. When I went on board, the wreck of the woodwork of the forecastle had been removed, and a portion of the main-deck. I found that four beams of the main deck had been broken; and three frames, the angle iron frames, of the ship to which the beams had been attached also broken; the beams were on the starboard side. I think the angle irons were on the port side. The pall bit was broken, the iron deck over the water tank in the forepeak was forced down on the starboard side, and upwards on the port side; some of the rivets on the bulkhead were sheered off on the angle iron. One of the angle irons on the bulkhead was broken, and the corner of one of the bulkhead plates was torn. There was a stove in the forecastle for warming; it was not broken; the paint which remained on the pall bit and in the forecastle scuttle was not scorched or blistered. The only appearance of fire that I could see was a piece of newspaper that was blown in between the plate stringer and the deck planks on the port side close by the scuttle. The length of the forecastle was 29 feet; the store room at the afterpart of the forecastle was 9 feet long; the size of the forecastle scuttle was 2 feet 2 inches by 2 feet 5 inches. There was no communication between the store room and forecastle, except the small crevices in the seams of the deck. There was a pipe at the lower part of the bulkhead of the forecastle which communicated with the ballast tank, amidships; there were taps on that pipe, both open when I examined it. The pipe communicated with both tanks; both with the one amidships that was full of cargo, and the tank that is stated to have been empty. The explosion, I am of opinion, was in the forecastle, above the empty ballast tank. With the forecastle scuttle open, I should have hardly thought the gas could have accumulated sufficiently to cause an explosion. Had the gas accumulated in the empty tank, it could have escaped into the forecastle through two holes in the iron plate at the top of the tank. I cannot form an opinion of the cause of the explosion.

(*By Mr. Crossman.*) The pipe connecting the two tanks was below the flooring of the midship tank, which was a double floor. I am not satisfied that the explosion was that of gas.

(*By Mr. Maude.*) The lid of the empty tank was torn from its fastenings.

(*By Mr. Cottingham.*) Gas in a confined space would force itself through any aperture.

(*By Mr. Darley.*) There was a communication between the fore and after tanks by means of the pipe that I have mentioned. I was informed that coals had been stowed in the after tank. I think the pipe was about 3 inches in diameter in the clear, shewing a communication between the after ballast tank and the forecastle through the pipe and holes in the deck of the foremost tank—supposing the taps of the pipe to have been open. There was an iron deck to the midship ballast tank, with a hatch in the middle of it, open. If that hatch had been on, gas might have generated in the tank and forced itself through the pipe, provided the taps were open, into the forecastle. The depth of the after ballast tank was 7 or 8 feet. With the hatch off I could not say that gas would generate at the bottom of the tank and so force its way through the pipe with the pressure of the coals. The diameter of the sounding pipe for the empty tank was 1¼ inch. The lid of the empty tank had been risen 14 or 15 inches. The explosion had disturbed both decks.

(*By Admiral Powell.*) I am of opinion that the explosion was at the top of the tank, and not in the tank.

(*By Mr. Darley.*) The scuttle to the forecastle was rather on the port side, nearly amidships, close to the forecastle bulkhead, 2 feet 2 inches by 2 feet 5 inches. The length of the forecastle was 29 feet.

GEORGE S. BAYLEY.

The within deposition of the said George Samuel Bayley was taken upon oath before me at the police court aforesaid, this 26th day of September 1873.

D. MAUDE,
Stipendiary Magistrate.

WILLIAM FOX sworn.

I was chief engineer of the "Berwick," and was so on the 9th of May last. It was my duty to look to the pipe that leads from the fore tank to the after tank. I know it was all right on the voyage in question; the cock was open. It is the practice to keep the cock open when the ship is loaded. The cock is at the end of the pipe near the fore tank. I have not known gas to accumulate in the bilges of a coal ship; have never seen an accident from such a cause. There was a slight explosion of gas in this ship in 1867, but no damage was done; that was in the fore part of the ship, in a partition between the forecastle and the bulkhead that separates the cargo—the store-room as it is now. I was not on board at the time, although I was the engineer. It occurred in port in the Tyne. The cock would not be open at the time, I feel sure of that; as a general rule we open the cock on going to sea. I feel certain that the explosion was caused by gas. I think the weather has a good deal to do with it. At the time of the explosion the weather was very heavy. I was in my berth at the time. I saw the deck blown up; the lining of the forecastle was all torn, and the tank-top broken; there was a smell of burning. One man was killed and two injured.

(*By Mr. Crossman.*) There was a light and a fire in the forecastle at the time.

(*By Mr. Maude.*) There were marks of burning on the men.

(*By Mr. Darley.*) The pipe is not intended to communicate with the after ballast tank; it ends in the bilge underneath the tank. We have portable limber boards in the after tank; they are short, one is nearly over the end of the pipe. The limber boards are 5 or 6 feet long. There is a strum in the after part of the tank with a cast-iron cover to it. I do not think it possible for gas to have passed up through this strum. The hatches of the after ballast tank were off at the time of the explosion; all the hatches were off. There were coals stowed in the flat above the after ballast tank, pretty nearly up to the upper deck. From the top of the tank to the upper deck is about 6 feet.

WM. FOX.

The within deposition of the said William Fox was taken upon oath before me at the police court aforesaid, this 26th day of September 1873.

D. MAUDE,
Stipendiary Magistrate.

Greenwich Police Court, 27th September 1873.

JOHN HYDE sworn.

I am a seaman and was one of the crew on board the "Berwick" on the morning of the explosion in May last. I was aft on the quarter-deck when the explosion happened. I first heard the report and then I came forward and saw one man lying down dead, and some men were trying to get up out of the hatchway. I did not go into the forecastle then, not till ten hours afterwards. Previously to the explosion, about half an hour before, I lighted a piece of a paper by the fire in the forecastle; the lamp being out, I lighted the paper to make a drop of coffee. Morrison was lying on the port side along the timber heads, about 12 feet from the scuttle hatch. I did not notice any peculiar smell in the forecastle when I lighted the paper. Two other men were severely burnt.

(*By Mr. Crossman.*) When I was below lighting my piece of paper, Morrison was in bed. I then threw the piece of paper into the ashpan; it was nearly out.

(*By Mr. Maude.*) There is a sounding pipe leading from the tank below, which extends 3 or 4 feet above the deck of the forecastle, about a foot from the cupboard and 2 feet from the ladder. Where I threw the piece of burnt paper was 8 feet from the sounding pipe; that was half an hour before the explosion. I did not see anybody smoking in the forecastle. I did not light the lamp, I lighted the piece of paper to look for my pot to make the coffee. Morrison who was killed had a great habit of smoking his pipe of a morning before he came on deck. My impression is that Morrison must have struck his match to light his pipe upon this sounding pipe.

(*By Captain Powell.*) One hatch was off of every hatch, it being such fine weather.

(*By Mr. Darley.*) There are limbers in the fore hold. I have seen them up and have assisted in getting them up; they simply ship and unship; they are 4 or 5 feet long. I did not notice a pipe under these limbers. I have not seen the limbers taken up in the after tank. There was some paints and oils kept in the storeroom and some turpentine in a bottle, a tin bottle. The naphtha was kept there in a bottle also. There are two places in the after tank, one on each side, to let the water out of the tank. In one place it was 2 feet long, perforated with holes. It looked like brass. I know it was not wood; it was about 1 foot long, and the holes were the size of a little finger. On the other side of the tank the holes were about the same size, but the form of the plate was different—one projects above the flat, and the other is in the flat. I never saw that plate up, and cannot say where it goes to. There was an iron bulkhead on the fore side of the after tank; the aft bulkhead was also of iron. The limbers aft are from 2 to 3 feet of the bottom of the ship. I have never been down in those limbers, only to put my arm down to get the dirt out. If the hatches were on and the tarpaulines there would be no means, that I see, of ventilating the vessel whatever. I have assisted to load her on many occasions.

JOHN HYDE.

The within deposition of the said John Hyde was taken upon oath before me at the police court aforesaid, this 27th day of September 1873.

D. MAUDE,
Stipendiary Magistrate.

FREDERICK ROGERS sworn.

I was a fireman in May last on board the "Berwick," and was on board on the morning of the 8th. I was in the forecastle when the explosion occurred. I did not hear the explosion. I was sitting down in the middle of the forecastle. I was burnt suddenly about the neck and arms. I did not smell anything. Morrison was coming out of the forecastle at the time; he was on the forecastle ladder. I did not see him light anything. I did not see him smoking his pipe. The third man who was hurt was in his hammock, 10 or 12 feet from the scuttle on the port side. I know that Morrison often lighted his pipe; he was in the habit of lighting his pipe before he went on deck of a morning; he mostly struck a match. I know where the sounding pipe is in the forecastle; I never saw Morrison strike his match on that pipe. I have seen him strike a match close by that pipe on a match box, as he kept his matches in a locker close alongside this pipe. I have seen him light his pipe at that locker; he would throw his match on the forecastle floor. I did not see him light a match on the morning of the explosion. He might have lighted one without my seeing him on that morning. When I got on deck Morrison, I found, was dead on the deck. I did not go into forecastle any more.

(*By Mr. Crossman.*) I went into the forecastle to call Morrison. I did not smell anything, nor did I see any flame.

(*By Mr. Maude.*) A man going up the ladder would pass by the pipe, which would be on his right hand, and he might easily have rubbed his match upon that pipe while passing up.

(*By Admiral Powell.*) I did not see much smoke in the forecastle.

(*By Mr. Darley.*) I had only been a week in the ship, one voyage. I was never in any of the holds. I never cleaned out the limbers, either aft or forward.

FREDERICK ROGERS.

The within deposition of the said Frederick Rogers was taken upon oath before me at the police court aforesaid, this 27th day of September 1873.

D. MAUDE,
Stipendiary Magistrate.

WILLIAM CLARKE sworn.

I was second mate of the "Berwick" in May last, and was present at her loading with gas coal. There is a warning posted up not to batten the hatches down while the ships are loading. I complied with that notice. The "Berwick" has five water-tight compartments, four holds for cargo; the first forward is a ballast tank, and is always used for that purpose; No. 2 is a store room; No. 3 is the fore hold for cargo; No. 3 is not used as a ballast tank; No. 4 is used as a ballast tank; there is a difference in the ceilings of Nos. 3 and 4. The ceiling of No. 4 is double all over. No. 4 is used for cargo as well as ballast. No. 5 is used for cargo only; the ceiling of No. 5 is the same as No. 3, a single lining, but since the explosion a double lining has been put on, making it the same as No. 4. That was done because the ceiling was bad and allowed the coal dust to get into the bilge. There is a pipe leading from the ballast tank under the forecastle. I do not know how far it leads aft; it leads into the bilge. When the bilges get water in they are pumped out by the engine. I was in the ship in 1867. There was a slight explosion then in the store room, but no injury was done worth speaking of. That occurred in the Tyne Dock. That occurred by a man taking a naked light into the store room. The gas must have got in through the wooden bulkhead. Ever since that time it has been strictly forbidden to go into the store room with a naked light, that is to say, if the ship is loaded. Our hatches were off at the time of the last explosion. I was in my bed at the time and felt the shock.

(*By Mr. Crossman.*) The ceiling of No. 5 was repaired in consequence of injury received in unloading the ship.

(*By Mr. Cottingham.*) No alteration has been made in the ventilation of the store room since 1867. The hatch of that compartment was off at the time of the last explosion.

(*By Mr. Darley.*) I have been seven years in the ship. Before the explosion the only way of ventilating the ship was by taking the hatches off. Since the accident two ventilating pipes have been placed in the fore hold, about 5 inches in diameter. There are four pipes been fitted to No. 4 compartment, the ballast tank, and two in No. 5 compartment; also two in the store room and two in the forecastle, which go down into the tank. There are also two ventilating pipes in the forecastle which do not go into the tank.

WILLIAM CLARKE.

The within deposition of the said William Clarke was taken upon oath before me, at the police court aforesaid, this 27th day of September 1873.

D. MAUDE,
Stipendiary Magistrate.

WILLIAM PARKER sworn.

I am steamship surveyor to the Board of Trade. On the 2nd August last, by direction of the Board of Trade, I surveyed the S.S. "Berwick," which was lying in the Regent's Canal Basin; her cargo was out. I made a report of the survey which I now produce, and it is correct. (Report read.) I am of opinion that the bilge would hold a sufficient quantity of gas to fill the tank and more; a portion might be drawn into the engine-room; from what I saw my opinion is that the explosion took place in the tank, and would rush out through the scuttle of the forecastle. I traced the pipe into the bilge. If gas accumulated in any of the compartments it would flow through the entire bilge.

(*By Captain Powell.*) The sounding pipe when I saw it went up to the upper deck. I would not pass a passenger ship with the sounding pipe coming into the forecastle where the men lived, unless it came to the main deck.

(*By Mr. Crossman.*) In colliers it is the general rule for the sounding pipes to end in the forecastle. We have no jurisdiction over colliers in that respect.

WM. PARKER.

The within deposition of the said William Parker was taken upon oath before me at the police court aforesaid, this 27th day of September 1873.

D. MAUDE,
Stipendiary Magistrate.

ADOLPH VASSARD sworn.

I reside at No. 25, Finsbury Place, London. I am a member of the Chemical Society of Paris, also of the Polytechnic Society, Paris. I have analysed a sample of coal sent to me by Mr. Johnasson, the owner of the "Berwick." I produce a portion of that sample. I have sent in a report of that analysis, and now produce it. (Read.) I have no doubt that the explosion took place at the mouth of the pipe in the forecastle under the ladder. The report of the explosion would be very low.

A. Vassard.

The within deposition of the said Adolph Vassard was taken upon oath before me at the police court aforesaid, this 27th day of September 1873.

D. Maude,
Stipendiary Magistrate.

Report of Adolph Vassard (referred to in his evidence).

I have no doubt that the cause of the explosion that took place on the "Berwick" was due to light carburetted hydrogen gas, otherwise metham, fire damp or marsh gas. This gas is but too often disengaged in coal mines from freshly cut surface or from apertures or blowers where it exists in a state of compression, or from the cells of the coal thrown open by the breaking of the coal. It produces all the accidents resulting from explosions in collieries. It is the result of the decomposition of wood or brom coal into mineral coal and is constantly formed by and disengaged from coal that has not yet attained the highest degree of decomposition represented by anthracite or stone coal. Of course it is so formed and disengaged in more or less quantity according to the actual state of the coal, that is to say, according to a graduate scale beginning with cannel coal and ending with anthracite.

The specific gravity of that gas is 0·559 or little more than half that of air. It is colourless and inodorous, and can be respired to a great extent without danger. When it comes into contact with air and a naked light, it at once explodes and produces carbonic acid gas, with the same effect as gunpowder.

Its illuminating power is very low, and it burns with a pale yellow flame.

It is a mixture of that gas and of carbonic acid that is given off when the bottom of pools in which water plants grow is stirred up.

I have examined the coal furnished to me by the owner as being similar coal to that which was on board the "Berwick" when the explosion took place. It consists of about one-third of highly bituminous coal and two-thirds of fine cannel coal. It well deserves its name of gas coal.

Some pieces of the coal are very curious, and show to actual demonstration that it is actually in course of further decomposition. It must be here remembered that cannel coal is the intermediate state of coal between wood coal and bituminous coal. Its decomposition is actually going on with large emission of light carburetted hydrogen.

The bituminous part of the cargo on board the "Berwick" was brittle and easily broken, and it may have been broken by the attrition of the ship; opening some cells and giving off its share of gas; but the great source of carburetted hydrogen was the cannel coal.

In four days a weighed quantity of that coal, broken up, gave me a quantity of the said gas equal ¾ of a cubic foot per ton.

Upon analysis the mixed coal gave me—

Carbon - - - -	64·72
Hydrogen - - - -	21·56
Oxygen and Nitrogen - -	11·92
Ash - - - -	1·80
	100·00

It gives 13·549 cubic feet of illuminating gas per ton, or nearly 462 lbs.

Its specific gravity is 1·272. The nature of the gas that to my belief produced the explosion having been explained, it can be understood that on a steamer loaded with a large quantity of such a kind of coal so liable to emit it, a rather large amount of carburetted hydrogen will be given off, and if no or inadequate escape is provided for it, it will find its way into any empty part through any leakage or pipe to be found.

Moreover, if, as I understand it to be the case, on board ship carrying coal all available room is used, if the bilge was not actually filled up with coal, at all events some was close by the side of it or immediately over it or part of it, and though the gas is lighter than air it may nevertheless have been forced sideward or downward into the bilge where there, finding the leaden pipe leading into the ballast tank, of course it took that way. Although that pipe is provided with a cock to prevent the escape of water when the ballast tank is filled, it may certainly have been left open, not being actually wanted for the purpose which it was fitted for, because the tank was empty.

Therefore, it can be assumed that that cock was left open; that the carburetted hydrogen gas went through the lead pipe into the ballast tank, and there finding its way through the air pipe it went through it. That pipe, the evidence shows, had its opening immediately under the ladder of the forecastle, therefore a current of gas, carburretted hydrogen, was continually existing under that ladder.

The first man who went down the forecastle may have lighted his newspaper when on the bottom, without thinking, however, that he was not doing the best for his safety. The deceased, very likely, lighted his match, piece of newspaper, or whatever it may have been, while actually going down the ladder, whereby his naked light came into contact or nearly so with the opening of the air pipe, therefore with gas (carburetted hydrogen), and necessarily an explosion took place which was communicated at once to the inner part of pipe and tank, and did all the damage.

In conclusion though, there are anthracite and chemically agglomerated coal dusts that are not liable to give off carburetted hydrogen, it is necessary on board ship carrying coal of any kind to provide for a thorough ventilation of the whole cargo, and to take every precaution against the escape of inflammable gases through pipes or into places where they ought not to be found.

(Signed) A. Vassard.

Exhibited before me at the police court aforesaid, this 27th day of September 1873.

D. Maude,
Stipendiary Magistrate.

Report of Mr. William Parker, Board of Trade Surveyor.

Board of Trade Surveyor's Office,
St. Katherine's Dock House, Tower Hill,
August 5th, 1873.

I have inspected the ss. "Berwick," official No. 6131, and have the honour to report as follows:

The vessel is 183 feet long, divided into six compartments by five iron bulkheads. In the first and third of these compartments iron 'tween decks are laid, and the lower parts used for water ballast.

In the first compartment and directly under the lower forecastle the explosion took place, tearing away from frames the whole of the iron deck from bulkhead to stem on the port side, bending and breaking nearly all the beams, and breaking three of the main deck beams above.

I have carefully examined the bulkhead dividing th tank from the hold and cannot conceive how gas could escape through it; further, I had it filled with water and found it perfectly tight. But I beg to call your attention to the pipe for running the water out of this tank; it is a 2½-inch pipe leading from lower part of tank to bilge in No. 3 compartment, and can be opened or closed by means of a cock attached to the bulkhead. The tank is always empty when the vessel is loaded, and if this cock had been left open, all the gas that accumulated and found its way into the bilge would flow into the tank, and considering it would not be required to be closed until the vessel was discharged and tank ready for running up, I think it likely it was left open.

I have further to observe that the only air pipes in the ship for the escape of gas were the sounding pipes, 2 inches diameter, which as usual were covered over on deck. The one that led from forward tank being alone open to the forecastle, and in such a position that a man going up the forecastle ladder could easily drop a lighted match down.

Since the explosion each compartment has been fitted with two 4½-inch air pipes, and the sounding pipe in forward tank carried above main deck.

This morning I intended examining the inside of forward tank so that I could satisfy myself there had been no other air pipes, and that bulkhead had not been repaired since the explosion, but when I got to the dock found vessel had sailed yesterday.

(Signed) William Parker,
Surveyor.

Exhibited before me at the police court aforesaid, this 27th day of September 1873.

D. Maude,
Stipendiary Magistrate.

The Report.

The "Berwick" was an iron screw steam ship, built at Cartsdyke, in the county of Renfrew, in November 1855. She had two decks, three masts; was schooner rigged, of 365 tons register, and the joint property of Messrs. George Elliott and David Johnessohn, of Usworth Colliery, in the county of Durham, colliery owners.

The "Berwick" was one of a fleet of steam ships solely employed in conveying coal cargoes, and made on an average from 45 to 50 voyages a year from the port of Shields to London and elsewhere.

She left Shields for London on the 8th of May last under the command of Mr. Robert Brown, who holds a certificate of competency as only mate, dated the 4th of July 1868. She had a cargo of 732 tons of gas coal on board. On the following morning (the 9th) at 4 o'clock the Dudgeon light was passed; at 5 a.m. a thick fog set in and the engines were eased to half speed. At 6 a.m. an explosion took place in the forecastle, and one of the stokers, John Morrison, who was at the time coming up the forecastle ladder, was blown some 14 feet into the air and killed, and two other firemen who were in the forecastle were much injured.

From the evidence it appeared that the "Berwick" was 183 feet long, and divided into six compartments by five bulkheads. In the first and fourth of these compartments iron 'tween decks were laid and the lower parts were used for water ballast. The first compartment, which was immediately under the forcastle, was used for water ballast only, the second compartment was used as a store room, the third for cargo, the fourth was a ballast tank and used also for cargo, and the fifth for cargo only. On this voyage the forward tank was empty, as was usual when the ship was laden; from this tank a discharge pipe led to the bilge under the after cargo hold; the cock of this pipe was, when the tank was empty, always left open. There was also a sounding pipe to this tank which terminated in the forecastle about 3 feet from the deck, and by the side of the hatch; this was an open pipe. There was also a rivet hole left open in the deck-flat to allow water to pass from the deck into the tank. The Court thus traced a direct communication between the bilge through the tank into the forecastle, where the explosion took place. It appeared in evidence that the coal shipped emitted a gas, although inodorous and not detrimental to life, and was highly explosive when brought in contact with flame.

Although the evidence is not entirely conclusive on this point, the Court have every reason to believe that the man Morrison, either by a lighted pipe or match, ignited the gas that came up from the sounding pipe, thus blowing him up through the hatchway and causing a lesser injury to the other two men who were at the further side of the forecastle.

The Court, after a careful consideration, exonerated the master of all blame, and returned to him his certificate.

It appeared by the evidence than an explosion took place in the forward store room of the "Berwick" in 1867, and there is every reason to believe that it arose from the ignition of similar gases; no one was then injured, and the damage done was trifling. The owners, however, very properly ordered the Davy lamp to be used in future in the holds and store room.

The Court would suggest that the following precautions should be taken in vessels carrying coal cargoes, viz.:—

That the discharge pipes from the water ballast tanks should not terminate in the compartments where explosive gas is likely to collect, and that all sounding pipes should be carried to the upper deck.

Also that arrangements should be made by pipes or other means to ventilate the lower part of the holds of vessels where gas is likely to accumulate.

The Court also attach the utmost importance to excluding fire damp or gas from the compartments where the crew live. This requires not only great care on the part of those who survey the ship, but the constant supervision of the master.

D. Maude,
Stipendiary Magistrate.

We concur in this report,

R. Ashmore Powell, } Assessors.
Wm. Darley,

The following Memorandum on Gas Explosion, drawn up by Mr. Darley, Nautical Assessor in cases of the "Zeno" and "Berwick," at the request of the Board of Trade, has been forwarded to the Commission by that Department.

To T. H. Farrer, Esq., Secretary of the Board of Trade,
&c. &c.

12, Batsford Road, St. John's Road,
Deptford, S.E.,
14th August 1875.

Sir,

In compliance with your request I have great pleasure in submitting the following memoranda and suggestions with reference to casualties from gas explosions on board colliers, observing that I shall be happy to aid you or the Royal Commission in any way you may desire.

I am, &c.
(Signed) W. Darley,
late Nautical Assessor.

To prevent Gas Explosions.

In a few words, to prevent accumulation of gas in confined spaces, and to command a free current of air to liberate it, has hitherto proved the most successful, and as a general rule explosions have taken place where this has not been attended to.

Great danger arises from the too hasty transfer of *green* coal just taken from the mine to the hold of the vessel; in this condition it is most gaseous, and hence the greater the danger; and although this applies to coal generally, it does particularly so to the Welch coal from Cardiff.

In order, therefore, to reduce the danger, it should be exposed to the atmosphere some seven or eight days previous to being shipped, but kept free from rain.

On being shipped, it will continue to emit gas, and more from heavy motions of the ship than when at rest; the tendency in the former case being to break the coal, and thus open the cells, and allow the escape of gas.

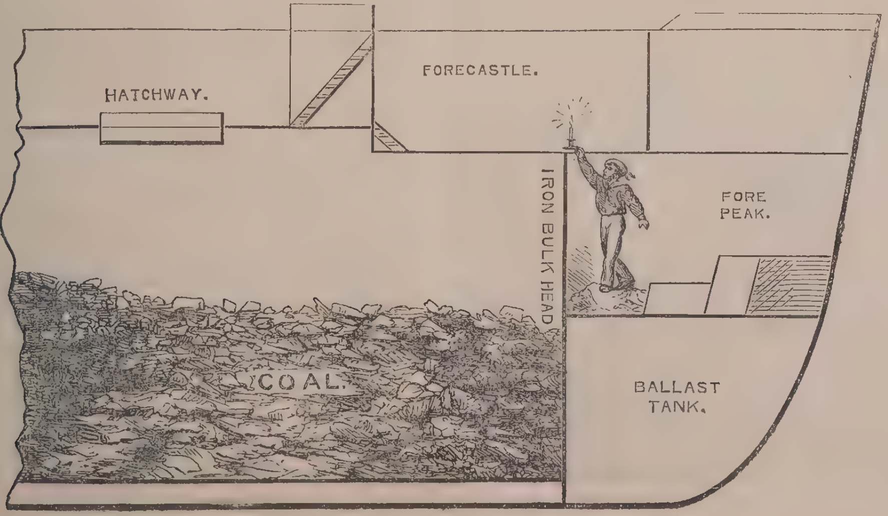
The specific gravity of gas being considerably less than atmospheric air, it naturally rises to the deck, and fills the space between the coals and deck; it also forces its way through any bulkheads to compartments adjacent to these in which coals are stowed, naturally in every case rising to the upper part of the vessel.

This was shown in the case of the "Zeno," where the forepeak, the fore side of the coal-hold, was made a receptacle for the gas, and in which the explosion took place, bursting the deck, the foremost bulkhead, and the bows of the vessel, by which in 50 minutes 33,000*l.* worth of property was destroyed, and 29 men placed in peril in the Bay of Biscay.

The accompanying sketch will give an idea of how it occurred.

The explosion in this case occurred through a man going to the forepeak with a naked light. He removed the hatch and went down and took the light in his hand. As soon as it was brought below the deck the explosion occurred, blowing him into the bows of the vessel.

"Zeno."



I would remark that it appears to me that an action takes place from the point of explosion as a centre. In this case the man was blown down into the bows; in the case of the "Berwick" the man who was the cause of the explosion was blown into the air and killed. This appears to arise from the act of stooping to strike his match to light the pipe, when the explosion took place below him and blew him up.

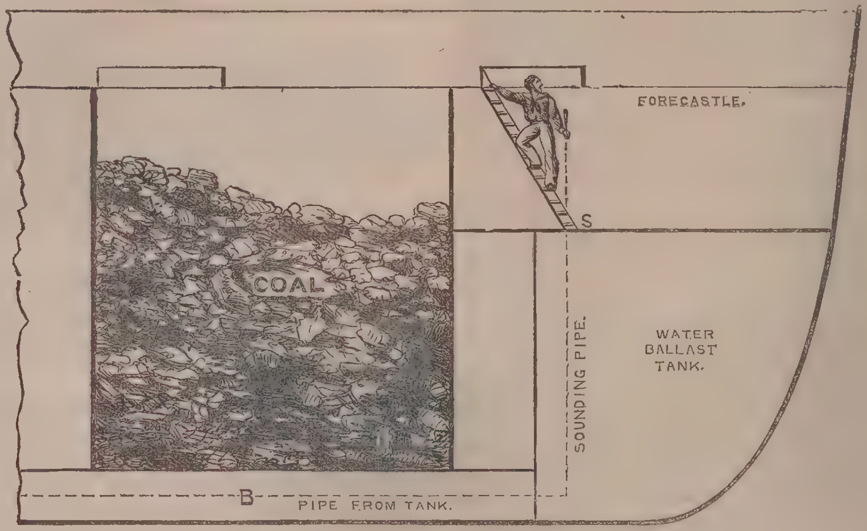
In each of these cases the gas was confined and no ventilation provided.

From the explanation given at the court at Hull on the "Zeno," she was built for a general cargo, and in this exceptional case was used for a cargo of Cardiff hard, brittle coal, shipped very green and under unfavourable circumstances, the weather being very foggy at the time.

The case of the "Berwick" was one in which the fore ballast tank was made the receptacle for the gas, fed by a pipe leading from the bilge in midships. At the upper part of the tank was fixed a sounding pipe, from which the gas as it escaped came into contact with the light and produced the explosion, as the man descended the ladder, of which the following sketch will give an idea, the light being produced from a match with which he was about to light his pipe.

There were two evils connected with this vessel, viz., the dangerous termination of the sounding pipe, S, between decks, instead of being led to the upper deck, and the pipe from the tank to the bridge, B, terminating in midships, instead of being led aft to the bilge of the engine-room, clear of the gases.

"BERWICK."



On the sounding pipe, S, ending between decks it would lead the gas into the forecastle, and make that a receptacle.

The pipe, B, leading into midships brought it in contact with the gas generated from the coal, formed a passage for the gas to the water ballast tank, and hence the fatal consequences.

The fact of the vessel being thus attended with such results suggest that no coal vessel should be allowed to proceed to sea in such an unseaworthy condition as the best means to prevent casualties from a similar cause.

The drawing which I forwarded to the Board of Trade, by request of the court at Hull, contained a comprehensive system of ventilation, which is applied with such success in the Royal Navy that such a thing as gas explosion is unknown in the service, and not only applies for the purpose of dispersing gases to prevent casualties, but also for sanitary purposes, to carry off foul air.

I would remark that it is necessary at least two cowls be fitted to each compartment which it is desired to ventilate, the one as a downtake, the other as an uptake. One drives the cold air down, the other allows the heated air to ascend.

The gas and impure air will thus be incessantly driven out as it is generated.

Whilst these shown in the drawing are the most effective, others less expensive may be fitted to answer the same purpose; but the fatality of so many vessels from this cause renders it imperative, for the safety of life and property, that some such means should be resorted to, which, however expensive, is cheapest in the long run.

In wood vessels the openings between the timbers may be used to great advantage for the purpose of ventilation, and a strake of plank left open or hinged, so as to admit of being opened at pleasure, the fillings between the timbers and chocks being left out in some cases, will afford means also for ventilation.

Perforations or holes cut through the bulkheads at the upper part will allow of a free passage of air from one compartment to another.

The bitts, mast wedges, mast (iron) hatchways, funnel casing, ballard heads, and other fittings present facilities for ventilation, which, if used, provide excellent means for liberating the gases and preventing danger.

I have hitherto referred to the means of escape aloft; I would now direct attention to those which may be resorted to below, in order to prevent the heating of the coals, and to admit of a freedom of the escape of gas from the lower part of the coals.

The cargo may be divided in midships by two planks, one on each side of the pillars, the plank being fixed Venetian way, or perforated, so as to allow the gases to escape between them.

The same course may be taken athwartships, where it is desirable, by placing chocks or quartering between the plank, allowing the gases to escape.

Particular care should be taken to well ventilate those compartments adjacent to the coal-holds, which from the leakage of bulkheads, are likely to mislead into danger by the impression that because there is no coal there is no danger; the cases both of the "Zeno" and "Berwick" showing that the danger lay where there was no coal.

I cannot speak in too high terms, nor do I think too much importance can be attached to the use, of the Davy lamps on board colliers. There is the same gas and the same danger on board ships as in the mine, and therefore I think it would be both wise and humane to make it compulsory to use them.

They were used on board the "Berwick."

I would recommend that testing tubes be fitted leading from the deck to the lower part of the hold, so that periodically, by the aid of a thermometer, the heat of the coals may be ascertained, the lower parts of these being kept a short distance from the bottom will allow of much of the gas escaping; these may also be perforated at the sides, which will further liberate the gas.

There is an objection to use wood pipes, which are commonly fitted, from their tendency to forward spontaneous combustion, from the fact that the wood will take fire at a much less temperature than coal, and being the practice to put them in the main hatchway where the small coal is deposited, the pipes first get dry, then the point of ignition is reached.

They convey air to fan the smouldering wood into a flame, and afford means for the fire to spread.

In the absence of the wood pipe, the coal disposed to heat would take a longer time.

If pipes of iron, perforated, be placed in the coals, they should be fitted vertically, and not at the lower part of the cargo, to come into contact with water.

Earthenware pipes may be used to advantage, laid horizontally at the lower part, and in the centre of the cargo.

The galley funnel, from its heat, would act favourably upon a pipe led up at the back of it, the heat having a tendency to draw the gases up.

Advantage should be taken of fine weather to open the hatches, and wind-sails may be used.

Arrangement should be made to ventilate the limbers and under the flats of the holds, and that good arrangement be made for pumping in the event of a leak. It is a matter of importance, with the view of preventing spontaneous combustion, that the cargo be shipped dry, that is, in dry weather, and that it be kept dry during the voyage.

There is much more risk with small than large coals, and it will be found that fires originate generally in the middle of the cargo under the main hatchway, where the greatest portion of the coal dust lies.

Coal having large quantities of iron and sulphur in their composition, being deposited in large quantities and not kept perfectly dry, are liable to spontaneous combustion, as a chemical action takes place, and ignites the coal, from which many vessels are lost.

(Signed) W. Darley.

"MIRANDA."

Official Inquiry held at the Town Hall, Cardiff, on the 13th September 1875, before R. O. Jones, Esq., Stipendiary Magistrate for the borough of Cardiff, assisted by Captain Castle and Captain Pryce as Nautical Assessors, respecting a casualty which happened on board the steamship "Miranda" off the coast of Cornwall on the 26th August 1875.

The Evidence.

Peter Peterson.

I am chief mate of the "Miranda"; joined her at Waterford in the beginning of June. I went from thence to Cardiff and Malta. Arrived at Newport on Friday, 20th August last, in ballast. Began to load on Saturday, the 21st. It was my duty to see to the loading. When we began the holds were quite clean and all swept out; no damp or wet in them to my knowledge. Her forehold, mainhold, and afterhold were divided by iron bulkheads. Completed loading on Tuesday evening, between 5 and 6. The coal was all supplied by the same merchant, so far as I know. There were the same marks on the waggons. P. D. were the marks on the waggons. While loading we had wet weather; it was raining on Tuesday, and was raining on Tuesday night. The hatches were not on. The three hatches were loaded at different times; the afterhold was loaded first; part of the mainhold was loaded first, then the afterhold was finished, the forehold being loaded last. It was showery all day on Tuesday. We were not altogether engaged on the forehold on Tuesday, but finished loading between 5 and 6 on Tuesday evening, and went out of dock at noon on Wednesday. When we left the dock the hatches were put on, but one or two on each hatchway was left off. The main hatches and fore hatches went right across from coaming to coaming. The after-hatch was divided. The weather when we got out channel was a strong breeze going down channel from the west. At half-past six p.m. we battened down the hatches. I have been for the last five years in coal-carrying vessels, and for some time before. This cargo was properly stowed. There is no ventilation in the vessel in the fore and main hold. The main hold was full, close up to the deck, and the forehold there was a portion left out on account of trim, a space between deck and cargo of, perhaps, 2 feet; it was not trimmed flat. There was no communication between forehold and forepeak; they were divided by collision bulkheads, which I considered water-tight. It extended up to the forecastle floor. There was a hatch in the forecastle to get down into the forepeak—a small hatch, right forward. From 6 p.m. till we got to St. Ives Bay it looked like bad weather, and I thought it prudent to batten down the hatches. There was a strong wind and heavy sea. Spray came over. We removed one of the hatches of the mainhold on Thursday, at 9 o'clock a.m., to let out any gas. None of the crew had complained. The boatswain told me in the morning it was dangerous coal, but did not complain that the hatches were battened down. When we took off the mainhold we did not discover any smell of gas. The other hatches were kept on. On Thursday, 26th, my watch began at 8 a.m. up to 12. Nothing particular occurred. There was a ventilation to the afterhold of iron. I noticed during my watch that the carpenter was repairing the bell. When my watch ended I went on the bridge. There was no light to my knowledge in the forecastle at that time; did not order any down into the forepeak. There was a lot of stores there—rope, oakum, &c. There was nothing but oakum and pitch there for the carpenter. Soon after 12, when on the bridge speaking to the captain, there was an explosion. When I saw the flame and pieces flying I went behind the funnel. The vessel was going at that time about 5 knots, her full speed. After the smoke cleared I saw the whole ship was blown up forwards, and we at once went to get the boats ready; that did not take long. The vessel was on fire. The captain ordered the engines to be stopped at once, that was the first order he gave; then we got out boats and called for the carpenter to mend the pumps. There was no reply. I asked the chief engineer to set on the donkey engine to put out the fire, which was at once done. We were about half an hour extinguishing the fire. After the report the flame went up as high as the topsail yard, and everything went before it. First saw the carpenter on the poop. The foredeck was entirely blown off. I think nine beams were broken, some in two, and the deck blown up past the foremast, and the forecastle blown up. The damage did not extend quite to the mainhold. A small hatch belonging to the mainhold was off—a separate hatch. I saw some pieces of tarpaulin burning aloft. Some of the ratlines blown off the bulwark was blown off, the first plate down. The explosion had opened the ship. The explosion went up and lifted the stronger plate and bulged the ship out. The bulkhead between forehold and mainhold was damaged; was blown aft 4 or 5 inches, bent. The ship is now in the Hill dry dock. After the explosion, first saw the carpenter on the bridge; he was very much burnt. His hair and beard were burnt off; his clothes were in rags. I spoke to him. I said, "Lie yourself down on the poop." I said, "Have you had any light." He said, "No." I asked him where he had been first. He said he had been down forward; whether forecastle or forepeak I do not remember. I afterwards heard the chief engineer ask him where he put the light. He said, "On the seat, and when I took off the hatch it blew him down the forepeak." When we called over the crew Benjamin Davies was missing. The carpenter told us there was one man missing, he did not say whom. Davies was in my watch. There was one more of the crew hurt. I can form no opinion of the cause of this accident. I should think it would be the naked light. I suppose gas got into the forepeak. No notice or instruction about ventilation had been given to us before leaving Newport Dock. There are no regulations there that I know of. I have loaded a cargo of coals at Cardiff. Never observed any notice there about the explosive nature of the coal, or about ventilation. Of my own experience it is always necessary to take off hatches when the weather permits. The ventilator passes through the deck in front of the poop, and goes up about 8 feet. That was the only means of ventilation.

(*By Mr. Vachell.*) It was in my opinion a prudent thing to batten down hatches at 6.30. We shipped water during the night. The look-out was stationed that night on the bridge, not on the forecastle, because of the sea. There was rain, and it was blowing hard during the night. The next morning we took off one hatch. If we had taken one off the fore hatchway we would have shipped water down. At the time of explosion vessel was heading S.W. ½ W., and the wind was about W.S.W., a head wind, a little on the starboard bow. At the time of explosion we were off St. Ives Bay. It would have taken about one hour and a half to get round the land to the open. Having rounded the land the vessel would have headed about S. by E. We should have had a fair wind. Having a fair wind we should have taken our hatches off. The ventilator goes right down to the bilge. It goes to the pump and down. The ventilator is the pump itself. There was a Davy lamp on board the vessel. It was not prudent to take the hatchway to the forepeak off with a naked light close by. There was no light to my knowledge in the forecastle up to 12. When the men go to bed they would have a naked light. I had been on board this ship when she had taken a cargo of coal.

Re-examined by Captain Castle.

We had mooring bits, two forward and two aft, and two on poop, of iron. They were not fitted as ventilators. I have seen them so fitted in new ships; there was a sluice valve in the collision bulkhead, shut when I saw it; carpenter had charge of it. The hatch was left off the night we loaded to let any gas accumulated go off. It was against all orders to take naked lights below; no one goes down without orders. If I had sent anyone down I should have given him orders not to take a naked light. We had no lock to our forehatch. Anyone could, I suppose, go down at any time they pleased. I had not given them any orders then. The hatch in forecastle was flush with the deck. There was nothing to stop it going up if there was any pressure.

(*By Captain Pryce.*) Our engines were amidships, and our foremast was immediately before the bulkhead separating the fore from the main hold, the end was parted off under the hatchway, probably trimmed into the wings, but at the fore and after ends there was a space. The seat on which the carpenter stood the light was 20 inches above the deck and about 2 feet from the after coaming of the fore scuttle. The forecastle did not require a lamp in the day time; at night it hung nearly amidships in the sailors compartment of the forecastle; but a little abaft the forecastle. The deck over collision bulkhead was of wood. The carpenter kept his nails &c. aft.

Edward C. Downing.

I am a coal merchant and shipbroker at Cardiff. I have had upwards of 30 years' experience of the nature of steam coal, and give attention to the stowing of steam coal and its dangerous qualities. Coal of the Lower Duffryn, Middle Duffryn, Abergavawr, Fontaman, Cwm Rheol, Aberuman collieries are of the same quality. Coal recently taken from the colliery contains more gas than that longer exposed to the air, and coal recently taken would require more care in shipment and ventilation. Coal stowed in a vessel in a wet state would not be liable to give out more gas than coal in a dry state. In coal liable to spontaneous combustion the damp would produce it, but such a combustion has never taken place in coal of the kind. In a vessel loaded as this was it would be most dangerous to put to sea without proper precautions as to ventilation in the case of coal battened down.

[Mr. Downing's evidence was interrupted in order that the witness Dunn might be called to speak as to the collieries from which coal had been raised.—(Signed) D. Rees, Justices' clerk.]

John Dunn.

I am foreman at Newport of the Powell Duffryn Company. I know what coal was shipped in August last in the steamship "Miranda."

	Tons.
Fontaman - - -	136·6
Lower Duffryn - -	395
Abergwawn - -	121·13
Middle Duffryn - -	405·1
Aberaman - - -	228·4
Cwm Rheol - - -	414

Altogether, 1,694 tons 4 cwts. 115 tons of screening were extracted. The nett quantity put on board was 1,579 tons 4 cwts. I saw part of the coal put on board. I believe the coal was very dry when put on board. From the invoices I have got, I can say that the coal had been about 24 hours out of the colliery, a little of it might have been longer.

(*By Mr. Vachell.*) I do not know what seam the coal came from like this, the air would in a very few hours be in a most dangerous state. It is usual to ventilate the holds of vessels loaded with coals of the description. The precautions are necessary in every hold. I should not think that gas would accumulate more readily where there is a space left in loading above the coal. The usual means for carrying off gas is to put the hatches open and pipes passing through the deck so as to create a current of air over the coals. I should think it most incautious in a master to batten down his hatches in a few hours after going to sea.

(*By Mr. Vachell.*) I know what kind of ventilators are put in vessels whose loading I superintend. I recommend the captains not to put wooden pipes. I might say the majority have iron ventilators of some kind. The ships I superintend generally take long voyages. A ship going to Bridgewater would equally require a ventilator with those going long voyages. I have known many explosions in dock in my experience. It is not usual to give notice to captains whether coal is recent or not from the collieries.

(*By Captain Castle.*) The tubes to take off the gas would be small 9-inch. I am not aware that mooring bollards are used as ventilators; iron masts are often used.

(*By Captain Pryce.*) Shown the plan. I should think it imprudent to have the hatches battened on before 12 hours, the coal being stowed as pointed out.

Thomas Dunn.

I am A.B., living at Newport, and shipped in the "Miranda" on the Monday before she left Newport. I shipped as fireman. She was loaded in the Alexandra Dock. There was one bit of a shower while loading. The forehold was the last loaded. The after-hatch was loaded first. I did not join till Wednesday about half-past 11. When she was in the basin the loading of the forehold had been completed, and was chock full up to the deck. All the hatches were battened down except one hatch on the forehold. Before we got outside the mouth of the river the forehold was battened down. That was done by order of the second mate. After that was done, order was given to wash decks. At that time all the hatches were fastened down. The carpenter secured them by order of the second mate. They were all battened down just as we got outside of the river. While I was on deck not one hatchway was opened, not even the little hatchway forward. I heard the boatswain grumbling about the hatches being battened down; we all grumbled. On the day after we left Newport my watch was from 8 to 12. During the watch I saw the carpenter about 10 o'clock; he was going to fix my bunker boards. At 12 o'clock I came up on deck, and just as I got to the forepart of the bridge away she went like the shot out of a gun. I was going forward. When the explosion took place we went to the boats, every man for himself; no one gave orders; all were cutting away the boats. I first saw the carpenter on the poop; the captain was putting oil over him and Jim James. After the smoke cleared, the captain gave order to put the hose on to dout the fire. After that I saw the captain putting oil on the men. Seven of the crew were English, the rest were Swedes. Benjamin Davies was killed by the explosion. Jim James was hurt, and the carpenter. We did not get our boats into the water.

(*By Mr. Vachell.*) The coal in the forehatch was shipped between 4 and 5 in the morning. We were there about 6, and then were told that the ship was not going out, so we went away. It was not raining at all. I was on deck when the ship went out of the basin. I have had no row with the chief engineer. After we got outside the Newport light there was no hatch off at all till it was it was blowed away. I did not complain to the captain about the hatch, it was not my place. I cheeked the boatswain about it.

(*By Mr. Waldron.*) I was sitting on the forehatch while we were going down the river. It was all down then. We got from the forecastle to the forepeak by a trap hatch; it was open two or three times. A man named John went down to the forepeak to look after his knife.

William Smith.

I was boatswain of the "Miranda." I signed articles on the 23rd August, Monday. I went on board on the Wednesday about half-past 10 in the morning. The vessel had completed loading, and was in the lock. She left Newport about half-past 11. We went down the river, it was blowing rather fresh. All the hatches were on except one hatch of the forehatch which was left off. That was fastened down about half-past 7 that evening. We were then below the Nash. When we got out of the river orders were given to clean the decks. I assisted. We began at the fore part of the vessel; we coiled the ropes away first. The middle hatch was entirely closed; the forehatch was open then, and I went down to the forehold to get some lines. That forehatch was not fastened down till we got below the Nash. The hatch was put on about 4; it was not battened down till half-past 7. From that time none of the hatches were opened. On the Thursday my watch was from 8 to 12 in the forenoon. I did on that day sound the bell at 12 o'clock by order of chief mate. The carpenter was working at it; the bell was lying on the middle hatch, and the carpenter said to himself, "I'll get a bit of wire to fasten the bell." He went to the forepart of the vessel. I went to my berth amidships, and was there about a minute when I heard a report, and went out and saw pieces of wood falling about. I was struck. I and all hands getting the boats, and went up and assisted. The forepart of the ship was blown up, and the hatch, and started the winch and blew the beams away, and the main deck as far as the foremast was started. There was a leak on the port side, and I shoved a sail in. I saw the carpenter come on the bridge holding his hands out and singing out. He went on the poop, and they put an awning over him. I had seen Davies that morning, the last time about two minutes before I went to my berth. He went to the foremast of the ship. After the explosion I picked up pieces of his cap on the poop. I heard the captain ask the carpenter if he had a light, and he said "No." Thomas James also was injured, and another, Gillmore, had his arm burnt and his hair singed. We landed those men at St. Ives. I have been in a great many ships laden with coal. The gas accumulated in the hold was cause of the explosion Never heard the sailors complain of the hatches being battened down; I did not complain to the mate. The carpenter told me he was going to batten all down for the night, and I told him not to be foolish. Thomas Dunn never spoke to me about it.

(*By Mr. Vachell.*) Going down the river the middle hatch was battened down. The main hold has two hatchways, a small one and a big one; both were battened down. I cannot say whether the after hatch was battened down. On the morning of the explosion the hatchway of the forehold was battened down. The night before the explosion it was blowing a fresh breeze at 12, but it calmed away before morning. We had shipped a great deal of water in the after-part of the ship during the night. When shipping water it is usual to batten down the hatches. I do not consider there was much sea on; there was pretty heavy roll, but the forehatch could have been opened very well.

(*By Captain Castle.*) The height of the covering of the forehatch was about a foot or a foot and a half, and the deck would have to be half full of water before it ran down the hatch. The night before the explosion the sea would open where they put out ashes. When I went below the day before I observed no smell of gas.

(*By Captain Pryce.*) I came on deck on Thursday morning at 8. She would have borne her fore hatches off at that time.

The "Miranda" adjourned Inquiry.
Town Hall, Cardiff, 14th September 1875.

JULIUS SCHALYE.

I am second mate of the "Miranda." I joined her two years ago in London. I have been six or seven voyages in her. She had coal on board every voyage. We arrived in Newport in August. I was on board while she was loaded. The holds were all swept and clean before she began taking in cargo. She was three or four days taking in cargo. During the time there were some showers. I was on board when the coals were put in forehold; they were not wet. There were some showers. Finished loading the day before we sailed (Tuesday). On the 25th we got out of the dock about 12. We were about a couple of hours going down the river. The after hatch was open when we left dock. It was battened before dark; the middle hatch was battened down about the same time. The fore hatch was closed about 7 o'clock. I was on the bridge at that time. The decks were cleaned up during the afternoon, about 2 o'clock. I did not notice the boatswain coiling up rope on the middle hatch. We had a strong breeze on getting into the channel and it continued during the night. During my watch, from 4 to 8, there was less wind, but the same sea. I came on deck at 12 at noon on the Thursday; as I came down the explosion took place, but I had time to notice that the small hatch of the main hold was open. I did not notice whether the after hatch was open. At 6 o'clock on the morning of Thursday I gave orders to one of the A.B., John Hansen, to go down the forepeak and take out some paint scrubbers. I told him, "Don't take any light with you." The forepeak is a dark place. I watched him from the bridge. I could see into the forecastle. He did not take a light with him. There was no light in the forecastle. I saw none during my watch. When the explosion took place I had just come down the foredeck to my berth. When the explosion took place I saw a lot of flame and smoke come out of the forehold. We went to get the lifeboats ready, and then put water down the forehold, because the coal was on fire. There was a lamp in the forecastle, hanging up. It was lit at 8 in the evening, when the sailors turned in. I do not know whether there was a Davy lamp on board. While going down channel I never heard any complaint from the crew about the hatches being battened down. I think the accident was the consequence of naked lights.

Cross-examined.

It was prudent to batten down the hatches on that night, because we shipped any amount of water over the bows. The crew could not keep a watch on the forecastle. On the morning of the explosion I went below at 8. It would not have been prudent at that time to take the hatch off, because there was a heavy head sea. I know the scuttle into the forepeak. It was similar to those I have seen in other vessels. I have never seen a padlock to any such hatchway. The mode of ventilation on this voyage was similar to that adopted on previous voyages. The carpenter had been two voyages in the vessel before this. I have been on board two or three steamers before. None of them had been ventilated otherwise than by the hatches. It would have been possible to have coiled rope on the main hatch if one or two of the hatches had been open. I am sure the main hatch was open while we were going down the river.

(*By Captain Castle.*) There was any amount of water over the bow after daylight. The forecastle was lighted during the day from the companion and bulls' eyes in the sides. I have never seen ships fitted with iron bits ventilators.

(*By Captain Pryce.*) The chain cables were bent to the anchor. The pipe to the locker was covered.

EDMUND SQUIRE ROBERTS.

I am agent at Cardiff for the Peninsular and Oriental Company, and have been here since 1870. We ship about 100,000 tons a year. Always endeavour to get the best description of coal, what is called the Cardiff steam coal. We ship a good deal of Powell's Duffryn steam coal. In shipping the best Cardiff steam coal only the usual precautions are necessary, that is, to keep a free current of air through the ship. There are other modes used, but I think them prejudicial. A great many vessels have large wooden ventilators coming through the deck. They are to prevent combustion. If coals are shipped dry we do not expect combustion at all. The coals from the pits named are likely to generate gas quickly. Gas would be generated in the hold of a vessel from such coal in a very few hours, particularly if shipped in wet weather. Coming fresh from the colliery, and put at once on board, it would be more liable to generate gas more quickly. Having on board such a cargo a master should then have his hatches open as long as weather would permit; he certainly should not batten them down for 24 or 36 hours after taking in the cargo, if it be possible.

(*By Mr. Vachell.*) There are times when the hatches must be battened down. It is very important to keep the coal dry. I have seen a great many steamers here loading with coal; none that I have seen have had ventilators; nothing but the hatches, and cowls at either end of the vessel. I am speaking from what I have seen and from what I have to do with. I have heard of explosions in the dock.

Re-examined.

It is absolutely necessary that a ship should be fitted with the cowls I speak of. If I did not see them in the vessel I have to do with, I should certainly speak about it.

(*By Captain Castle.*) It is necessary to have a free current of air in a ship built in compartments, I should think it necessary to have ventilation in each compartment.

(*By Captain Pryce.*) I should think it imprudent to fill each compartment with this kind of coal, and to cover up the hatches for 24 hours without some means of ventilation. And it would be madness to take a naked light after the hatches had been down for 17 or 18 hours near any orifice by which the gas might escape. I do not approve of loading the coal right up to the deck. The diameter of the pipe of the ventilation of the ship I have had to do with would be 14 inches, but smaller pipes would do. A gas pipe of 3 inches would carry away a great deal of gas. I have seen a chain pipe used as a ventilator. I have seen bits used as ventilators.

MOSES SHORES.

I am chief engineer of the "Miranda." I have been over three years in her, and during that time she has been engaged in carrying coal. On Wednesday, 25th August last, we were in Newport. I was very busy with the machinery below. I did not notice when they finished putting in the cargo. We passed Cardiff about 4. I came on deck while they were sweeping the deck. I did not notice the hatches; they were open while we were in dock. On Thursday my watch below commenced at 8. We were going about six knots full speed. We had head winds and sea which began the night before I went off watch at 12 at night. It was blowing and raining then. It got a little finer towards the morning. At 12 on Thursday, when the explosion took place, I was in the engine-room. I was on deck shortly before. I felt a terrific shock, and in a few seconds I had the order to stop her. I did stop her. I went on deck. I saw the captain after the explosion. I saw the carpenter and dressed his wounds. The captain was standing by. I said, "Carpenter, where did you place the light." He said, "I placed it on the seat locker till I lifted the hatch, and the moment I lifted the hatch, the explosion took place and blew me up;" that was all he said. He said to me, I afterwards found the bottom and lid of the lamp. It was the lamp used in the forecastle. I found it under the forecastle amongst the wreck; it was a common oil lamp.

(*By Mr. Vachell.*) There is a safety lamp on board. It was the carpenter's third voyage. I do not know whether he was warned about carrying a naked light. But he asked me on another voyage why he had to take the hatches off, and I explained to him about the gas, and he seemed quite satisfied.

(*By Mr. Castle.*) When I went forward I found the ship damaged; the beams, nine of them, were broke away, the forecastle blown up, the berth all blown to pieces, the forecastle bulkheads blown out, the main deck blown up right away to the foremast, and the sheer plate bulged out.

The bulkheads blown in so far as the mainhold, the mainsail split from the rigging right on the forecastle; the front of the deck and the poop is broke; the deck ends on the poop are broken; the stringer plates were started. I missed Davies about half an hour after the explosion. The second mate was, I believe, sent down the forepeak to look for him. I do not know whether a boat was lowered, or whether anyone was sent aloft to look out for the body.

JOHN HANSEN.

I am A.B., lately of the "Marinda." On Wednesday, 25th of August, she completed loading. I was then on board. We left dock about 12; the hatches were all open. They were fastened down at 8 o'clock that night. There was a head wind and strong breeze, and we shipped a deal of water that night. On the Thursday morning I relieved the wheel at 12 o'clock. At 6 o'clock that morning I went, by order of the second mate down the forepeak, for paint scrubbers. He said "You don't want a light." I said I did not want a light because I had put the scrubbers there myself. I did not notice any smell of gas. When I came up I put down the hatch. There was no light when I put it down. I put out the light myself at 4 o'clock; it was an oil lamp, and kept just by the forecastle door. It was a tin plant with a spout. At 12 I went to the wheel, and had been there from 5 to 10 minutes before the explosion took place. I saw smoke and flame and pieces of wood blown about in all directions. Davies was in or near the forecastle. I found a piece of his cap on the deck. When I took the wheel I saw the carpenter working at the bell on the middle hatch.

(*By Mr. Vachell.*) A dinner time it was fine, but a heavy swell from the N.W. At 8 o'clock, I could not say whether it would have been prudent to open the hatches. At 8 o'clock there was a deal of spray to windward. There was a safety lamp on board kept in the store room. I never used it, but I have seen it used. I do not know of an order that it should be used by anyone that went below.

(*By Captain Castle.*) The wheel was right aft on the poop after the explosion. I ran to the bridge to the boats. I expected to go down directly. I saw nothing floating on the water.

THOMAS FOSTER BROWN.

I am a mining and civil engineer at Cardiff. I am acquainted with the coal raised by the Powell Duffryn Coal Company. Freshly worked coal from their colleries put into the hold of a vessel would, if barometrical pressure were lowered, have a tendency to give off carburetted hydrogen gas. It would only be a reasonable precaution to ventilate the hold of a vessel in which such coal is stowed. The hold of a vessel in which a vacuum was left would be more liable to combustion than where the coal is filled up to the deck.

EVIDENCE given on behalf of the OWNERS and MASTER.

JAMES STYLES.

I was for a long time a master mariner and am now a ship agent. I have commanded three steamers carrying coal cargoes, and since I have been agent at Cardiff. I have seen many more. The holds are usually ventilated by the hatchways being left off. In some cases assisted by small ventilators. I do not know of a number of vessels coming to Cardiff that have no ventilators. I have commanded one that had none. There are times in this channel when it is necessary to close the hatches shortly after leaving port.

(*By Mr. Waldron.*) I should think it negligent if I did not see to the proper ventilation of my cargo. It would be partly my duty as master.

(*By Captain Castle.*) The two vessels I commanded had for ventilators upright tubes in iron about 6 inches bore. Plain tubes, without cowls, and about 4 feet from the deck. I recollect, however, having some with cowls and some without. We had one only in each compartment. It was put there to liberate something rather than to create a current. But I agree that there ought to be one at each end. If there had been one at each end we should have kept the cowls on. I imagine the tubes would be better larger than 3 inches. The ship that had no ventilators I had no trouble with, but I felt uneasy sometimes.

(*By Mr. Jones.*) If I had left port, say, at 12 o'clock today, and the hatches remained closed from 6 or 7 this evening until 12 the next day, I should have felt uneasy; but I should have gone on as the captain in this case seems to have done.

HENRY CLOAKE.

I am agent for Messrs. Spire, steamship owners, and was formerly a master mariner, and I have had considerable experience as to vessels coming to Cardiff. I have known many ships coming here for coals which have not been fitted with ventilators. There is a steamer now in dock recently built which is not fitted with ventilators. I have never put a steamer about when she was going against a hard wind because of gas, or heard of its having been done.

(*By Mr. Waldron.*) It is decidedly necessary to have some means of ventilation over the cargo, and I should think wise to take such precautions. I should think I was, to some extent, responsible for the safety of my crew. If a ship is going head to wind gas would get to the fore part of the ship.

(*By the Court.*) If I had had coal battened down for 17 hours I should have endeavoured to prevent lights being used.

Report of the Court.

The "Miranda" was an iron screw steamer belonging to Hull, official number 60,140. She was built at Stockton-on-Tees in 1868, her registered tonnage 865·73 tons, and she was owned by E. Leetham and others, at Hull.

She left Newport about noon on the 25th August, with a full cargo of 1,579 tons of smokeless steam coals, bound to Kiel, and had a crew of 23 hands, all told, including the master, Mr. Wilhelm S. A. Van Deurs, who holds a certificate of competency as master, No. 18,491.

After clearing the Newport River she met with strong south-westerly winds and a considerable sea, which caused her to ship some water, but not sufficient to necessitate the hatches being put on.

All proceeded well until about 6.30 p.m., when, as the wind was increasing and night coming on, the whole of the hatches were put on and battened down that time, until after daylight the following morning, the wind continued fresh, with a high sea, and the vessel shipped such large quantities of water that the look-out man on the forecastle had to be removed to the bridge.

After 6 a.m. the weather moderated, but the sea still continued so high that it prevented the hatches being taken off, without the risk of wetting the cargo. Nothing further of importance occurred until shortly after 12 p.m. The forenoon watch had just been relieved, and the first officer and the master were on the bridge, when about 12.15 a terrible explosion took place in the fore-part of the vessel, which was instantly enveloped in smoke and flame. The engines were at once stopped and boats got ready for lowering; meanwhile it was noticed that the whole of the fore-part of the vessel, from the foremost forward, had been blown up by the explosion. As some fire was seen, the donkey engine with hose was at once set to work, and in about twenty minutes the fire was extinguished.

The extent of damage is described as follows:

Nine beams broken.
Forecastle deck and all the butts blown to pieces.
Main-deck blown up as far aft as the foremast.
Iron bulkhead to main-hold bent aft some four or five inches.
Stringer plates on both sides lifted.
Main-rail split, and sheer-strake, and bulwarks much disturbed.

The well was sounded, but the vessel made no water.

On mustering the crew it was found that Benjamin Davies, one of the crew, was missing, and is supposed to have been blown overboard, as portions of his clothing were found in the rigging.

The carpenter was most seriously burnt, and Thomas James, seaman, was also hurt. These two men were landed at St. Ives to receive medical help, about two hours after the accident, and were left in the infirmary there.

The "Miranda" then bore up for Cardiff.

The Court, on the evidence given before them, attached no blame whatever to the master, and they therefore returned him his certificate.

They find that this casualty is clearly to be attributed to an explosion of gas, caused by the use of a naked light near the scuttle of the forepeak by the carpenter, previous to and when he opened the scuttle, the gas having found its way from the hold through the collision bulkhead.

The gas had accumulated in the fore-hold from the time the hatches were battened down on the evening of the 25th until the explosion took place, and had no means of exit.

It was stated in evidence that the whole of the cargo consisted of freshly wrought coal, none, with perhaps the exception of a few waggons, having been above ground more than twenty-four hours. In this state it was more dangerous than it would have been if exposed longer to the atmosphere.

After the loading was completed on the evening of the 24th August, the hatches were left off all that night until 6.30 p.m. on Wednesday evening, when they were battened down. From that time till the explosion, a period of 17 hours, the gas had been accumulating in the holds, which had no ventilation whatever.

After hearing competent and experienced mining engineers and other experts on the subject, the Court feel it their duty to offer some suggestions for future guidance with regard to these dangerous cargoes, and therefore respectfully ask the attention of the Board of Trade to the following points :

1. That all vessels about to carry cargoes of gaseous coal should be fitted with ventilating pipes or shafts at each end of either compartment or hold, that is, that there should be two ventilators in each hold or compartment, so fitted as to be constantly kept open.

2. That it is advisable in carrying this description of coal to have a small space between the deck and cargo, so as to insure a free circulation of air between the ventilators.

3. That the master should be instructed to have the hatches off whenever practicable during the voyage.

4. That naked lights be strictly prohibited.

Dated this 15th day of September 1875.
(Signed) R. O. Jones,
Stipendiary Magistrate.

We concur in the above Report,
(Signed) John S. Castle, } Nautical Assessors.
Chas. E. Pryce, }

In addition to these, the following Cases of Explosion of Gas in Coal-laden Vessels, in which no Statutory Inquiry was held by the Board of Trade, have also been reported by that Department to the Commissioners :—

"THOMAS PARKER," of Sunderland, an iron steamship of 739 tons register, four years old, and classed A1 Lloyds.

Was lying in South Dock, Sunderland, and bound for Cronstadt with 1,350 tons of gas coals; the hatches were left off, and also a hatch in store room, for ventilation.

On the 9th September 1875, about 9 a.m., four coal trimmers went into the after hold, each taking an unlighted candle, when one of them struck a match to light the candles and an explosion immediately followed. The flames reached nearly up to the mixed cross trees, but the fire went out shortly after the explosion. One of the crew was slightly burnt, and four coal trimmers burnt, one very seriously. The damage done altogether to ship and cargo amounted to about 85*l.*

"LOUISA THERESA," of Vannes, France, a wooden ship of 200 tons register, 20 years old, and classed 3/3–1–1, French Veritas.

Sailed from Cardiff for Mataro with a cargo of coals on the 16th January 1876, and on the same date, when in the Penarth Roads, an explosion of gas took place on board, owing, it is believed, by the gas coming up through the hatch in the Lazarette and igniting by the fire in the master's cabin.

"ATALANTA," of Greenock, O.N. 67914.

The Board of Trade forwarded to the Commission the following report, with its enclosure, from one of the Board of Trade's surveyors at Cardiff, relative to the explosion of coal gas on board the "Atalanta :"—

Board of Trade.
Cardiff, June 22nd 1876.

Explosion of Coal Gas on board ship "Atalanta" of Greenock, O.N. 67914.

In forwarding the account of above-mentioned catastrophe, I respectfully beg to suggest for the consideration of the Board the advisability of making it compulsory for *all* vessels to have a ventilator forward and aft, as it is at the *ends* of the vessel that gas always accumulates. There have been six or seven explosions of coal gas on board vessels here since I have been stationed at this port, and in every instance the explosion has been either at the forward or after end of vessel.

More elaborate means of ventilation may be suggested, but the use of an ordinary iron cowl ventilator at each end of vessel would, I think, be both efficient and simple, and would keep the space at ends of vessels clear even if the hatches should be accidentally closed, thus preventing much loss of life and property by such explosions as this.

Of course this plan is for sailing vessels only. Steamers would require a ventilator in each compartment.

(Signed) W. H. Neate,
Prin. Off. S. Wales.

The Asst. Secretary,
Marine Department,
Board of Trade.

Extract from "Western Mail" of the 20th of June 1876.

Terrible explosion at Penarth. A ship shattered. Four seamen killed and many injured. Marvellous escape of a rigger.

The most disastrous explosion that ever occurred on board a ship at Cardiff took place at 9 o'clock on Monday morning. The inhabitants of Penarth and the Bute Docks were thrown into the utmost consternation at the intense force and report made by the explosion. So great was the shock that many believed an earthquake had shaken the foundations of their dwellings. Persons, however, about the dock at Penarth and on Cardiff pier-head observed the flames and smoke issuing from on board a large ship lying in Penarth Dock, to the height of at least 200 feet, and on hastening to the scene found that a frightful explosion caused by an accumulation of gas from the cargo of coal had taken place on board the barque "Atalanta" of Greenock. The decks were blown up fore and aft from stem to stern, the rigging, sails, and spars broken and enveloped in a black and dense smoke. Immediate efforts were made to get on shore the injured members of the crew.

The "Atalanta," being outward bound and ready for sea, had most of her crew on board.

She completed loading on Friday last, having shipped 1,600 tons of the Glamorgan Coal Company's large steam coal.

The ship was so much damaged that fears were felt lest any of the crew could survive the accident, but the exploring party nobly pursued their labours, descending into the ship's hold, cabins, and forecastle, in search of the dead and dying. Men were found shockingly burnt about the head, arms, and body. These were speedily landed, and placed in the store of Mr. Clode, butcher, who kindly lent his premises on Penarth Wharf for the use of the wounded. Dr. Nell was soon in attendance, and ordered some of the cases to be removed without delay to the "Hamadryad," hospital ship. Three cases, however, he retained, viz., that of Matteo, an Austrian seaman, Donald McKerrel the carpenter, and Captain William Brown. The Austrian succumbed to the injuries he had received at 10.30 a.m., his body being removed to the Penarth mortuary. The carpenter was subsequently forwarded to the hospital ship very badly hurt, and Captain Brown, who also was much burnt about the arms and head, was conveyed to a private residence at Penarth.

While the wounded were being thus attended to, and that in a most efficient manner by Dr. Nell, it was discovered that Mr. Whitworth, chief officer of the "Atalanta," had been killed; also an ordinary seaman, named John Hamett. Both bodies were carried to the mortuary.

At a short distance from Mr. Clode's store another crowd was assembled around one of the dock lodges, wherein was found the mate of another ship, the "Killochan," of Greenock, most dangerously wounded. He was in the act of shaking hands with Mr. Whitworth, of the "Atalanta," on that ship's deck when the explosion occurred. Mr. Whitworth was killed, and Mr. Alexander Muir, mate of the "Killochan," was thrown about 40 feet into the air. In alighting, his head came in contact with a spar, contusing the skull. His right leg was at the same time broken. He lies, as stated, in a hopless condition.

As to the cause of this direful event, it has been clearly ascertained to have arisen from the accumulated gas from the cargo being ignited by a match lit by one of the seamen, named Antonio Pedro. He accompanied H. Ross, second officer of the "Atalanta," into the after hold. Mr. Ross carried a naked candle, and no sooner had Pedro struck the match to light the candle, than the explosion took place. Both of these men, who are badly burnt, are lying at the hospital ship.

The "Atalanta" is a ship of 1,116 tons register; built at Richmond, United States, in the year 1857. She is the property of Mr. C. S. Caird, of Greenock, and was bound from Cardiff to Hong Kong. Strange to state, the ship that was alongside of the "Atalanta" at the time, viz., the "Killochan," likewise belonged to Greenock.

APPENDIX III.

ADMIRALTY CASES.

THE following Papers have been received from the ADMIRALTY relating to cases of SPONTANEOUS COMBUSTION or of EXPLOSION in the COAL BUNKERS of HER MAJESTY'S SHIPS, and in certain of the GOVERNMENT COAL STORES.

H.M.S. "EUPHRATES."

(Spontaneous combustion in starboard coal-box.)

The following is an extract from the report of the captain of the "Euphrates," of the spontaneous combustion which occurred in the coal-box of that ship on the 9th April 1868.

H.M.S. "Euphrates,"
Bombay, 10th April 1868.

SIR,

I HAVE to report for your information that an occurrence happened last night on board H.M. Ship under my command which might have been attended with very serious results, but for the watchfulness of the engineers on duty in the engine room at the time, and the measures which were adopted on my being informed of it.

The circumstances are as follows:—We had sighted Kolarba and Kautari Lights about 9 p.m., and had stopped the engines so as to wait till daylight this morning for coming into harbour to our moorings. Shortly before midnight the chief engineer (Mr. Eyers), reported to me that smoke was observed to be issuing from the starboard coal boxes, and he had no doubt that spontaneous combustion had taken place:—Fortunately there were only about 20 tons of coal in that particular portion of the bunkers. A hose from the donkey engine, and also from the foremost pump on starboard side of main deck, were immediately led into the coal box in question, and very shortly the place was flooded; and whatever combustion there was about the coals were completely put out. * * * *

In conclusion I beg to inform you of the cause of the combustion, viz., that Spencer's patent cement round the boilers is not properly secured, in fact is not secured at all; and from the great heat has got so dried up that it has fallen off, consequently leaving nothing between the great heat of the boilers and the coals behind them. I would therefore suggest that some means (iron bands or otherwise) may be adopted for boilers coated with the same composition for the purpose of keeping it from falling off.

I have, &c.,
MONTAGU B. DUNN.

H.M.S. "MINOTAUR."

The following is an extract of the Report received at the Admiralty of an explosion of gas which took place in the coal bunkers of H.M.S. "Minotaur" on the 25th June 1869:—

"On the 25th instant, while taking in shell, an alarm of fire was reported in the compartment in which the shell are stowed.

"The ship's company proceeded to their stations, and it was immediately ascertained that a slight explosion of gas had taken place at the starboard after coal bunker, opening into that compartment. The bunker has two manholes in the compartment, and three shoots from the maindeck.

"The temperature had been taken at midnight and showed 78° at the foremost and 68° at the after end, which were the same as on arrival at Spithead after steaming.

"A leading stoker had been sent to examine the boxes and had opened a manhole in the compartment. As he lowered his light it was extinguished by a rush of gas, and he, as well as the gunner's mate at the shell-room hatch, believe that they saw a slight explosion. An A.B., who was sitting on the lid of a shoot on the main deck, felt the lid move under him. He got off and it was lifted six inches, and he also believes that he saw flame.

"The rush of gas brought with it a good deal of coal dust, and there was for a moment an impression amongst the men working at the shell whips, that a fuse had ignited.

"The bunker was cleared and the coal transferred to other places, but no trace of fire could be discovered among them, or anything which would appear to give rise to fire damp. They appeared dry and in good order."

The Inspector of Machinery on being called on for his opinion as to the cause of the explosion, reported as follows:—

I have the honour to report that in my opinion the explosion alluded to as having occurred on board H.M.S. "Minotaur" on the 25th inst. was caused by an accumulation of gas (carburetted hydrogen) being pent up in the coal bunkers, and having no means of escape by which, on being released by the stoker with the light, on his removing the cover caused it probably to ignite. This gas is constantly being generated and evolved more or less with all kinds of coal, but accidents are usually guarded against by having the bunker covers removed daily for a few hours, when the gas passes off and mixes with the atmosphere in such small quantities as to be imperceptibly felt; but as the covers are mostly in the cable tier in this ship, it would not be safe to remove them daily to allow the gas to escape; therefore means should be adopted to carry it at once into the atmosphere.

I would suggest in vessels of this class, where large quantities of coal are stowed (remaining as it does a long time in the ship), to have a ventilating pipe fitted from the coal bunkers leading to an outlet on the upper deck where the gas might freely escape; this, together with removing the coal bunker covers daily when in harbour, would prevent a recurrence of such an accident (when at sea under steam, the furnaces will keep the coal bunkers clear of gas).

I beg most respectfully to bring to your notice the evil of allowing water to pass into the bunkers, whilst washing decks before time has been given to replace the covers after coaling ship. This practice is very dangerous, more especially with Newcastle coal, as moisture causes heat and gas to be evolved very rapidly, and in this kind of coal, where iron pyrites are present in considerable quantities, the danger is all the greater (it does not however always follow that the temperature is increased when the gas is given off).

The Admiralty then issued the following circular to the Fleet, inclosing the extract quoted above from the report of the explosion on board the "Minotaur."

(Precautions against accumulation of gas in coal bunkers.)

Circular No. 44. Admiralty,
July 29th, 1869.

The accompanying report having been received from the "Minotaur" of an explosion of gas having taken place in the starboard after coal bunker of that ship, on a light being lowered through one of the man holes, the attention of all officers commanding Her Majesty's ships is called to the necessity of opening the lids of coal bunkers occasionally so as to allow of the escape of foul air, and prevent the accumulation of gas, which is constantly being evolved, more or less, as regards all descriptions of coal.

Care should be taken to prevent water passing into the bunkers whilst washing decks, before time has been given to replace the covers after coaling, such practice being very dangerous, especially with Newcastle coal, as moisture sometimes causes the rapid generation of heat and gas.

By command of the Lordships,
VERNON LUSHINGTON.

To all Commanders-in-Chief, Captains,
Commanders, and Commanding Officers
of Her Majesty's ships and vessels.

H.M.S. "MEGÆRA."

(Heating of coal in the bunkers.)

The following is an extract from a letter dated 7th January 1870, reporting the proceedings of H.M.S. "Megæra":—

On December 26th the coal in the starboard wing bunker was found to be heated, temperature 130°, which I ordered to be used at once; but the temperature still increasing to 145°, I deemed it advisable for the safety of the ship to clear the bunker, which was done by getting up and starting the coal on the main deck, and also by increasing the revolutions of the engines and the expenditure of coal. A moderate breeze from the southward having sprung up on the 27th December, the ship had been placed under sail and the fires burned down, but as the coal in the port bunker also began to heat temperature 130° to 132°, I gave directions for the steam to be got up again, so that the coal might be used and the heated place arrived at, as from the quantity of cargo stowed on the main deck as well as the coal already got up, there was no more space for stowage there.

The wing bunkers are now quite clear of coal, and the temperature of all the bunkers has assumed its normal condition.

Commodore Dowell, with a view to ascertain the cause of the high temperature of the bunkers, ordered an inquiry, of which the following is the report:

H.M.S. "Megæra," Ascension,
January 11th, 1870.

SIR,

IN compliance to your memo. of this date we came on board this ship to inspect the bunkers and endeavour to ascertain the cause of their dangerously high temperature, and have now the honour to inform you that we are of opinion that the temperature 145° while steaming and that immediately behind the boilers is not a dangerous one, but it being a sudden rise from that of the usual temperature, viz., 90° had been we are of opinion that the removal of the coal from that part was advisable.

Every proper precaution to prevent undue heating by opening the lids, &c., appears to have been taken.

The bunkers appear to have sufficient ventilation, and we do not see that any alteration or improvement can be made.

We cannot attribute the increased and sudden rise in the temperature of the coal to any other cause than its nature (Hirwain's Diamond Coal) which is of a softer texture and appears to emit gases more freely than the other, viz., North Country, as the different temperature in similar heated parts of the bunkers where these coals were stowed were Hirwains 146° and North Country 90°.

We have, &c.
(Signed) J. BURGESS,
Commander H.M.S. "Pandora."
A. MILLS,
Chief Engineer H.M.S. "Rattlesnake."
J. DALTON,
Engineer H.M.S. "Pandora."
H. M. SMALL,
Engineer H.M.S. "Rattlesnake."

The Commodore forwarded these reports to the Admiralty with the following letter.

"Rattlesnake" at Cape Palmas,
February 18th, 1870.

SIR,

THE officer commanding the "Megæra" having represented to me that on his passage from England to Ascension, the coal in the bunkers became excessively heated, and that he had deemed it necessary for the safety of the ship to clear the two wing bunkers, I ordered an inquiry into the particulars of the circumstances with a view to ascertain the cause of the high temperature of the bunkers, and whether anything could be done to prevent it again happening.

The report of the inquiring officers is forwarded herewith, and it would appear that although the rise in the temperature of the bunkers was sudden, and sufficient to create some anxiety, it never reached a dangerous pitch. The bunkers are reported to be well ventilated, and precautions were taken to prevent any undue heating.

If the statement made to the inquiring officers in reference to the excessive heat generated in the bunkers from Hirwain diamond coal is correct, it would appear that that description of coal is not fit for the service; but their Lordships are doubtless in possession of information from other sources which will enable them to form a correct opinion as to the qualities of the kind of coal supplied to the "Megæra."

I have, &c.
W. M. DOWELL, Commodore.

On receipt of these reports the Admiralty called for a report from Sheerness of the particulars of the coal supplied to the "Megæra," from whom they were received, &c.

The following are copies of this report and its inclosures:

Sheerness Yard,
April 4th, 1870.

SIR,

WITH reference to your minute on Superintendent of Stores, Order N.S. $\frac{2108}{2769}$, of the 1st instant, directing us to report the particulars of the Hirwain Welsh coals supplied to the "Megæra" on the 12th October last, from whom they were received, &c., we have the honour to report that these coals were received from the Hirwain Coal Company under agreement of 5th, and Superintendent of Contract's letter of 15th May last, observing the coals are not called Diamond, that being the name of the ship from which they were delivered.

The cargo was carefully inspected prior to breaking bulk in accordance with Admiralty Order S.T. of the 28th April last, and the following report inserted on invoice of receipt, viz.:—

28th July 1869. The coals, Welsh S.V. have been surveyed in accordance with the instructions, and there is nothing in their appearance to prevent immediate delivery.

In addition to which, the cargo was examined during delivery and found to consist of the same quality of coals as seen on first inspection.

During the delivery of the coals a trial of their evaporative power was made on board the steam-tug "Otter" attached to this port, and the result reported in officer's letter of the 4th August 1869, a copy of which is forwarded herewith.

Subsequent trials of this coal mixed with North country coals were made at the coal testing boiler in this yard in accordance with Mr. McCulloch's wishes. See officer's letter of the 12th August 1869, copy herewith.

Their condition when issued to the "Megæra" was the same as at the time of their receipt.

We have, &c.
R. COBBETT, Storekeeper.
A, B. STURDEE, Master Shipwright.

Sheerness Yard,
August 4th, 1869.

SIR,

WITH reference to your memorandum on Superintendent of Contracts, Order 1591, of the 19th ultimo, directing the officers to make a very detailed and particular report as to the quality of the Hirwain coals lately delivered at this yard, under purchases made by Mr. McCulloch. We have the honour to report that a cargo of the Hirwain coals has arrived at this yard, and upon making the usual trial at the coal-testing boiler in the course of its delivery, it was found that the rate of evaporation in cubic feet per hour was very much below the average, and the quantity of refuse produced excessive, it was therefore deemed expedient to make a further trial in the boiler of one of the harbour vessels attached to this port, and in conformity with your directions this trial has been made on board the "Otter."

We find under these circumstances the rate of evaporation extremely low, the fires burning very sluggishly, requiring the constant use of the pricker to produce even the rate of evaporation obtained, and the quantity of cinder and refuse proportionately large.

A trial with Nixon's Navigation coals in the same boiler has been made for special comparison, and the results show the Hirwain coals to be of very inferior quality, unfit for the service, and subject to the rejection provided for by the letter of 6th May last, from the Hirwain Coal and Iron Company to the Admiralty.

The coal per "Diamond" has been rejected accordingly on the invoice.

A tabular statement of the result of the trials of the Hirwain and Nixon's Navigation coals for comparison is forwarded herewith.

(Signed) A. B. STURDEE, Master Shipwright.
J. BANNISTER, Engineer.

(Copy.)

SHEERNESS YARD, 4th August 1869.

Description of Coal.	Name of Vessel.	Date of Trial.	Pounds of water evaporated per lb. of coal consumed.		Cubic feet of water evaporated per hour.		Percentage of			Smoke.	Remarks.
			Calculated at the temperature of the feed.	Calculated at 100° constant temperature of the feed.	Calculated at the temperature of the feed.	Calculated at 100° constant temperature of the feed.	Ashes.	Clinkers.	Both ashes and clinkers.		
Hirwain coals -	"Diamond"	1869 2nd Aug.	6·273	6·432	34·40	35·27	34·87	·73	35·60	Very light.	Fires burnt very sluggishly, although every effort was made to facilitate the combustion of the fuel. The number of cubic feet of water evaporated per hour per foot of fire grate was only 1·07.
Nixon's navigation coals.	From store	3rd Aug.	8·295	8·513	81·66	83·81	8·00	1·02	9·02	Very light.	Fires burnt briskly, ordinary firing. The number of cubic feet of water evaporated per foot of fire grate per hour, 2·15.

Note.—These trials were made in an ordinary marine boiler; area of fire-grate 33·3 feet.

(Signed) A. B. STURDEE, Master Shipwright.
J. BANNISTER, Engineer.

The Captain Superintendent.

Sheerness Yard,
August 12th, 1869.

SIR,

WITH reference to our letter and report of the 4th instant on the quality of the Hirwain coal, lately delivered at this yard under purchases by Mr. McCulloch in which we stated that the rate of evaporation was extremely low and the amount of refuse excessive.

We have the honour to report that in obedience to your directions of Monday last, when Mr. McCulloch visited the yard with reference to the above report, we have made further trials in the boiler used for testing coals at this yard of the Hirwain coal mixed with North Country, the result, as will be seen by the annexed report, shows a much greater rate of evaporation and a reduced amount of refuse.

We are of opinion that this coal may be used mixed with bituminous coal, although it is not adapted for burning in ordinary furnaces (without blast) in its unmixed state.

(Signed) A. B. STURDEE, Master Shipwright.
J. BANNISTER, Engineer.

(Copy.)

SHEERNESS YARD, 12th August 1869.

Description of Coal.	Name of Vessel.	Date of Trial.	Pounds of water evaporated per lb. of coal consumed.		Cubic feet of water evaporated per hour.		Percentage of			Smoke.	Remarks.
			Calculated at the temperature of the feed.	Calculated at 100° constant temperature of the feed.	Calculated at the temperature of the feed.	Calculated at 100° constant temperature of the feed.	Ashes.	Clinkers.	Both ashes and clinkers.		
Half Hirwain Half Cowpen Hartley.	"Tam O'Shanter" From store	1869 10th Aug.	7·341	7·574	31·37	32·37	1·96	1·90	3·86	Very light	Wind, W. & N.W.
Three quarters Hirwain One third Cowpen Hartley.	"Tam O'Shanter" From store	11th Aug.	7·942	8·194	31·90	32·91	1·88	2·00	3·88	Very light	Wind, W. & N.W.

Memo.—The front of boiler and the door of boiler-house face the north.

(Signed) A. B. STURDEE, Master Shipwright.
J. BANNISTER, Engineer.

The Captain Superintendent.

Subsequently the Admiralty wrote the following letter, directing the Superintendent of H.M. Dockyard at Malta to call upon the commanding officer of the "Megæra" to state whether any steam pipe passed through the bunkers in question:—

Admiralty, S. 88.
June 18th, 1870.

SIR,

I AM commanded by my Lords Commissioners of the Admiralty to request you to call upon the commanding officer of H.M.S. "Megæra," to state with reference to the report of the excessive heating of Hirwain coals in the bunkers on board that vessel, contained in his letter of proceeding of the 21st March last, whether a steam pipe passed through the bunkers in question.

I am, &c.
(Signed) T. WOLLEY.

The Admiral Superintendent,
H.M. Dockyard, Malta.

To this letter the Admiral Superintendent at Malta forwarded the following reply from the officer in command of H.M.S. "Megæra," from which it is clear that no steam pipe passed through the bunkers of that ship.

H.M.S. "Megæra," Malta,
July 4th, 1870.

SIR,

IN compliance with directions from my Lords Commissioners of the Admiralty contained in your memo. of this day's date, relative to the excessive heating of Hirwain coal, as reported in my letter of proceedings of 21st of March last, I beg to inform you that no steam pipes pass through the bunkers of this ship.

I have, &c.
H. D. SARRATT,
Staff Commander in Command.

Rear-Admiral A. C. Key, C.B.,
Admiral Superintendent.

H.M.S. "MALABAR."

(Spontaneons combustion in coal bunkers.)

The following is the report of the captain of H.M.S. "Malabar," of the fire which occurred in her coal bunkers on 24th February 1872.

H.M.S. "Malabar,"
Bombay, 2nd March 1872.

SIR,

I BEG to report that on the morning of the 24th February the coal in the bunkers was discovered to be on fire.

At 4 a.m., a strong smell of sulphur was observed by the men working in the bunkers behind the boilers, on both sides about the same time. These bunkers were immediately closed, and also the watertight doors and all ventilation stopped.

The doors in after end of the stoke-hole were opened, and the coal got out as fast as possible; much of it was found to be smouldering and heated and only requiring a little air to burst into a flame.

When the coal was sufficiently cleared away, a hose was got into the bunkers and water played on the coals: the fire on port side was soon got under, but on the starboard side the fire seemed to have got more hold of the coals, and it was found necessary to flood this box entirely.

I am unable to give any cause for this occurrence; the coal was received at Malta from under cover only 16 days before (on the 8th February), and was perfectly dry and apparently well mixed, half Welsh and half North country.

The test tubes did not show any sign before the discovery and afterwards they never showed more than 111°, which is very little above the usual temperature when in the Red Sea.

Coals were described as—

Cowpen Hartley, received at Malta	-	April '71
And Fothergill's, Aberdare ,,	-	Sept. '71

It is fortunate the state of the coals was discovered so soon as it was, or the consequences might have been serious.

Those engaged worked well and quietly. I did not deem it necessary to stop the ship, and I believe at the present moment most of the officers and troops are unaware of the occurrence. No damage has accrued to the ship.

I have, &c.,
SHOLTO DOUGLAS.

H.M.S. "RATTLESNAKE."

(Spontaneous combustion in coal bunkers.)

The following is the report made by the chief engineer of H.M.S. "Rattlesnake" of the case of spontaneous combustion which occurred in her coal bunkers in February 1872.

H.M.S. "Rattlesnake," at Elmina,
7th April 1872.

SIR,

I HAVE the honour to make the following report on the Cowpen Hartley Coal (142 tons) received on board this ship from the Naval Depôt at Sierra Leone.

At 6 a.m., on the 20th February, smoke was discovered to be issuing from the coal boxes at the back of the starboard boilers.

The coals were removed into the stoke hole until the origin of the fire was discovered on the removal of about 20 tons.

The origin of the fire was to be attributed to spontaneous combustion, as shown by the discoloured appearance of the coal.

Every precaution was taken to ensure its being stowed quite dry in the boxes, and as it was observed to possess a large quantity of sulphur and iron pyrites special care was taken.

There being very little coal on the station, it became necessary to complete from the quantity in store at Sierra Leone, and care was taken that this coal was first consumed.

The coal was also indifferent for generating steam, and on being put into the furnaces, it burnt bright and brisk, but soon become dull and dead, causing extra work to the men from continued firing and pricking, thus showing it to be not only dangerous, but totally unfit for tropical climates.

I have, &c.,
H. W. ELGAR.

This Report was forwarded to the Admiralty by Commodore Commerell with the following memorandum:—

Forwarded for the information of the Lords Commissioners of the Admiralty, observing that I quite concur in the remarks of the Chief Engineer, that this coal was quite unfit for use in a tropical climate.

Fortunately the combustion in the bunkers was discovered in time to prevent any serious disaster and no damage was done.

As a preventitive against a similar occurrence, I have forwarded the particulars of this spontaneous combustion to the Naval Agent at Sierra Leone, for the information of the Officers Commanding H.M. ships coaling there, pointing out that this coal is by no means fit to supply to H.M. ships, and that every precaution is to be taken to prevent danger.

H.M.S. "GLASGOW."

(Spontaneous combustion in coal bunkers.)

The following is a copy of the report of the captain of H.M.S. "Glasgow," of this case of spontaneous combustion, which occurred on 22nd June 1872:—

H.M.S. "Glasgow," at Sea,
25th June 1872.

SIR,

I HAVE the honour to inform you that an alarm of fire was reported to me about 4.15 p.m. of the 22nd instant in the starboard after coal bunker, of H.M. ship, under my command under the following circumstances:—

On Mr. Pearson, the engineer of the 4 to 6 watch passing the bunker door in the stoke hole, he perceived a strong smell of smoke, and on entering the bunker found the smell stronger and smoke arising in considerable quantities, he immediately acquainted the chief engineer, and on its being reported to me the ship's company were assembled at their fire stations, and after a personal inspection of the bunker, where I found much smoke and heat, a considerable quantity of water was pumped into the bunker.

Immediate steps were taken to remove the coal, which was done as fast as possible, the pumps being kept going at the same time.

On removing the coals to the depth of about six feet, the greatest heat appears to have been reached, extending for a space of about four feet athwartships, the temperature shown by thermometer being about 135°, this great heat extending to a considerable depth.

By 11 p.m. the heated coals had all been cleared out, and the bunkers became cool, and on further examination no damage appears to have been done to the wood-work (sheathed with copper) which was in immediate contact with it.

The temperature of the bunkers, tested every four hours during the day, showed 98° the maximum.

I beg to enclose a list of the quantities and description of the coals received on board the ship since arrival at Suez, when the bunkers were empty, and, as far as I can ascertain, the heated coal in the bunker was a mixture of that supplied at Aden, Bombay, and Trincomalee, that of Bombay being the smallest and worse quality; but I am unable to say in what proportions, the total quantity in this compartment being about 82 tons.

The coals were received on board in a perfectly dry state, in tight lighters, the weather very fine and the sea perfectly smooth.

The bunker lids fit close, and no water has ever been discovered to pass through from the deck into the coals.

The door of this bunker in the stoke hole had been open since the last coals were received on board, and there being a run of coals in it for charging fires and culinary purposes, a large space had been consequently for some time free for the escape of gas and heated air.

I am unable to assign any cause for this overheating of the coals other than the result of spontaneous combustion.

I have, &c.,
T. MORTON JONES.

The Admiral Commanding-in-Chief forwarded this report to the Admiralty with the following remarks:—

"I do not believe that the fact of the coal being Welsh had anything to do with this occurrence. The 'Glasgow' called at Bombay immediately after the squadron, then under my command did, and I think it will be found that a large proportion of what she received there was North Country."

HER MAJESTY'S COAL STORES AT JAMAICA.

(Cases of spontaneous combustion.)

The following is a copy of the Report of Survey, dated 29th August 1872, on certain coal stored in H.M.S. Yard at Jamaica, from which it appears that two lots of North country coals (180 tons and 240 tons respectively), shipped to Jamaica in the colliers "Sophia and Jane," and "Mary Ann Johnson," (which arrived there 8th May and 9th July respectively,) were condemned as unserviceable in consequence of spontaneous combustion.

H.M.S. "RACOON," 29th August 1872.

A REPORT of SURVEY held on the following STORES, in HER MAJESTY'S YARD at JAMAICA, in pursuance of an ORDER, dated the 28th August 1872.

—	Species.	Estimated quantity tons.	Condition.	Cause of decay or unserviceableness.	Convertible or not, if the former, to what purpose.	Whether they should be sent to England, or sold in the country.	Remarks.
Lot 1.	Coal - - North Country. Ex "Sophia and Jane." (In store.)	270	Good and serviceable.	—	—	—	It is desirable that ships taking this coal should be informed that spontaneous combustion has occurred in portions of the same cargoes, and that it should be mixed in stowage with two thirds Welsh, as usual.
Lot 3.	Ditto - - Ex "Mary Ann Johnson." (In store.)	261					Ditto.
Lot 2.	Coal - - North Country. Ex "Sophia and Jane." (In the open.)	180	Unserviceable.	Combustion and heat and admixture with sand, &c., which last will inevitably be much increased by removal from its present position.	Not convertible.	Sold in the country.	This coal is of a highly bituminous nature. It has been stored in considerable quantities rather close under a roof of corrugated iron only, the temperature there having been as high as 97° in the day-time, while in the centre part of the store, the roof of which is additionally protected with boarding, it is seldom over 84°. From the quantity of coal in store there was but little ventilation. It may have become heated by getting damp at some time, but there is no evidence of such being the case.
Lot 4.	Ditto - - Ex "Mary Ann Johnson." (In the open.)	240		Ditto.			

The following is a report from the Commodore at Jamaica, of a further case of spontaneous combustion which occurred in the same stores.

"Aboukir," at Jamaica,
September 5th, 1872.

I HAVE the honour to report for the information of the Lords Commissioners of the Admiralty, that a portion of the North country coal contained in No. 1 store was again found to be spontaneously heated on the morning of Sunday the 25th instant, this time on the north side of the store.

2. The coal which heated this time is part of a cargo of "Cowpen Hartley, North Country Steam Coal," which was landed ex "Sophia and Jane," in April of this year.

3. The heated coal was fortunately not in the central store, and could therefore be removed, but for this purpose it was requisite to take down a portion of the wall.

4. It was found necessary to remove about 240 tons of coal which, with the amount which caught fire on the previous occasion, as reported in my letter No. 63 of the 24th ultimo, being reported unfit for issue to Her Majesty's ships will be sold by auction.

5. In case of erecting coal stores in this or similar climates in future, it would be in my opinion desirable to have wood or some non-conducting material underneath the roof sheathing, whatever it may be. The officers who surveyed the heated coal report that whilst the temperature in the upper part of the store roofed with zinc sheets over wood was 84°, in the stores roofed with corrugated zinc without wood underneath it was 97°, and it was in the latter store that in both cases the coal caught fire.

6. I am inclined to think that the last-named cause, together with the inflammable nature of the coal, (for it appears to contain a considerable quantity of sulphur and iron) and the want of ventilation are sufficient to account for the spontaneous combustion.

7. The better ventilation of the coal stores has been in progress since the first fire occurred.

I have, &c.,
A. F. R. DE HORSEY.

On the 7th November 1872 another case of spontaneous combustion in the Jamaica coal stores was reported to the Admiralty. The following is a copy of the report:—

H.M.S. "Aboukir," at Jamaica,
7th November 1872.

SIR,

I HAVE the honour to report that a portion of the coal contained in No. 2 Store was on the 31st ult. found to be in a state of spontaneous combustion.

2. The coal which took fire this time (the third time in three months) is part of a cargo of Cowpen Hartley's North Country Steam Coal which was landed ex "William" in April of this year.

3. It was found necessary to remove about 74 tons, most of which will probably be serviceable.

4. The vicinity of one of the large doorways (which I had lately caused to be made in the walls of the depôt), enabled the heated coal to be removed without difficulty.

5. It will be observed that the coal which heated this time, as on former occasions is North country, the supply of which, received in the spring of this year, appears to have been of an inflammatory description.

I have, &c.,
A. F. R. DE HORSEY.

The following is a memorandum dated 2nd November 1872, drawn up by Mr. Rowsell, Director of Contracts, with special reference to the foregoing cases of spontaneous combustion in H.M. ships and coal stores:—

Several cases of spontaneous combustion having been reported within the last few months, noteably in the "Glasgow" "Rattlesnake" and in the coal store at Jamaica, and varying opinions having been expressed as to the cause, and as to whether Welsh or North Country coal is more liable to spontaneous combustion, it seemed right to ascertain the opinion and experience of the trade upon the subject.

As far as could be done during a hurried visit lately to South Wales and to the North, this has been done, The results are as indicated in the following extracts from notes of conversations at Cardiff and Swansea :—

"No. 1. No spontaneous combustion with best Cardiff coal; there is with some of the Swansea coal, especially with that class of coal which is shipped to South America for copper smelting. Damp promotes combustion.

"No. 2. None in best Welsh coal; no sulphur in it, sulphur necessary to spontaneous combustion; damp no doubt assists. All coals get damp in coming from mine to vessel in open trucks, or they get damp on board; some Welsh coals are certainly sulphury *e.g.*, Dynevor; but never knew a case of spontaneous combustion with best steam coal. Firedamp attaches to even the best coals, which should not therefore be hurried on board ship. Open question as to ventilation of coals after shipment, but hatches should not be closed sooner than absolutely necessary.

"No. 3. There is spontaneous combustion as well as firedamp with some coals; 20 tons of some coals will defile a whole cargo; some coals having much sulphur are bound to ignite though sent no farther than Barcelona. Own (Resolven) coal would not, nor would any of the best steam coal; never knew of it. Has known of explosion with the best coal. That is quite a different matter, and preventable.

" At Newcastle, Sunderland, and Seaham.

" No. 4. Never knew of one case of spontaneous combustion with Cowpen Hartley coal. That was the case of a large cargo for Bombay, which got wet."

" No. 5. Never had a case of spontaneous combustion, combustion does happen when damp gets in.

" No. 6. Has not heard of spontaneous combustion with Wear river coal. Gas coal has sometimes given rise to explosions in contact with flame.

" No. 7. Lord Durham's agent never knew a case with their coal. 'Fiery heap,' and 'duff heap,' has been known to take fire of itself.

" No. 8. Never knew of spontaneous combustion of North Country coal, except with gas coal on long voyages."

The small coal seems to be more liable than lumps.

2. From these notes it would appear that danger from firedamp or the gas which clings to all coal whatever, is serious and certain to manifest itself where the coal freshly wrought has been hurried on board and quickly confined and flame has been allowed to come in contact with the escaping firedamp. This seems however to be a preventable danger. It has not shown itself in any of H.M. ships or depôts. It is, moreover, quite distinct from spontaneous combustion.

That occurs with coal. Welsh or north country, in which any large quantity of sulphur may be contained, is attested by much experience. It seems to be unknown in connexion with the best Welsh steam coal, and nearly so in connexion with best North Country, though Mr. Fothergill, the fitter of the Cowpen Hartley Colliery, furnished the only instance given in these notes of combustion which took place in some of their coal accounted the best in the North. Liability to spontaneous combustion seems to turn upon the greater or less amount of sulphur contained in a coal. Sulphur in some degree and in some shape exists in all coal. Its presence in a large degree is indicated by yellow patches of sulphide of iron, commonly called "brasses," whereof 3,500 tons were stated in a recent letter from Mr. Hunt, of the School of Mines, to have been picked out of coal in the northern district in one year. These "brasses" exist also in Welsh coal, and it is reasonable to attribute their absence in cargoes shipped on Government account, as much to the vigilance of the inspection by the Government Inspector there (now Mr. Traill), as to any original purity of coal, or of intention on the part of coal owners. The elimination of "brasses" from north country coal shipped on Government account, and prevention of shipment of sulphur-bearing coal, would be one of the most useful and desirable functions of a coal inspector in the North.

Damp or wet seems to be indispensable to the process of chemical change, whereof one effect is spontaneous combustion. With every care it is impossible to guarantee absence of wet on board ship, or in transit of coal to ship. Even in depôts, especially abroad, it is practicably impossible to ensure perfectly dry storage.

5. It would appear, therefore, that too much care cannot be taken to secure coal for store abroad, and for use in ships, which shall contain a minimum quantity of sulphur.

6. Would it not be as well to ask Mr. Hunt if he can state what is the maximum amount of sulphur permissible in coal, so as to render it safe from spontaneous combustion?

7. Endeavours should perhaps also be made to secure analysis of all the coals used by the Admiralty.

The analyses have doubtless been made, if so, all that will be needed will be to collect the results.

F. W. Rowsell.

H.M.S. "DEVASTATION."

(Explosion of gas in coal bunkers.)

The following is a copy of the report received by the Admiralty of an explosion of gas which took place in the coal bunkers of H.M.S. "Devastation," on 1st June 1874:—

H.M.S. "Devastation," Portland,
1st June 1874.

Sir,

I have to report that this morning an explosion of gas took place on board Her Majesty's ship under my command in the starboard centre upper coal box, under the following circumstances.

At a quarter before 5 a.m., the two cook's assistants named in the margin surreptitiously removed the lid of the escape man-hole of the coal-box, which is immediately abreast of the officers' galley, in the breastwork deck. Hill carried a naked light, and was seated on the deck with his feet in the bunker resting on the coal, and was, it appears, stooping with his light to pick out lumps, when there was an explosion which severely burnt him about the face and neck, rendering his removal to sick quarters necessary.

Frederick C. Captain's Cook's Assistant, and Joseph Curtis, Ward Room Cook's Assistant.

The coal in the bunker was a mixture of two thirds Welsh to one third of North country; it was shipped here on Tuesday last.

The bunker was nearly full, and the explosion was confined to the aperture.

The temperature was 66° Fahrenheit, and there was no indication of heat or damp when the box was searched immediately afterwards.

I have, &c.

Frederick W. Richards, Captain.

The captain of the "Devastation," on being called on to state what precautions were taken to prevent accumulation of gas in the bunkers with reference to Circular No. 44 of 29th July 1869, issued after the case of the "Minotaur," reported as follows:

H.M.S. "Devastation," Portland,
3rd June 1874.

Sir,

The system pursued in the coal boxes in the "Devastation" is as follows:—

Every morning after the decks are finished, the escapes are opened and a Clanny's safety lamp lowered into every lower box; the boxes remain open during the day, and are closed after evening quarters.

For keeping the lids watertight, a soft gasket, soaked in oil and coated with tallow, is fitted to each manhole; these gaskets are removed when coaling, and afterwards the grooves are cleaned, and they are carefully replaced; these precautions were taken after coaling on 26th May before any water was used on the deck, and there was no indication whatever of water having got into the box in which this accident occurred, the coals were and are perfectly dry.

The chief engineer, accompanied by an engine-room artificer, had thoroughly examined the box and gauged the coal for quantity on Friday the 29th, when there appeared to be no indication of gas being evolved.

The "Devastation" has two tiers of coal boxes, the lower are all fitted with water-tight escapes, which are open daily, as I have stated, for about 12 hours.

The upper boxes are fitted with the usual circular manholes, only these run the length of the breastwork decks, and, as a rule, these boxes are not opened on Saturday or Sunday, nor have they been on this occasion since they were closed on Friday evening.

Having in view the rapidity with which the gas must have been generated on this occasion, it would be well if escape valves were fitted to these boxes when opportunity offers, as it is manifest that the lids of coal bunkers cannot always be off on a ship's deck,

Frederick W. Richards, Captain.

The papers were then referred to the officers of the yard at Portsmouth.

The master shipwright's and engineer's department reported as follows:

In compliance with the directions contained in your memo. of the 11th on Comptroller's letter of the 10th inst., S $\frac{5225}{5839}$, and accompanying reports of the officers of H.M.S. "Devastation," respecting an explosion of gas in the upper coal bunkers of that vessel.

We have the honour to report that we are of opinion that had the gratings been used frequently in the deck-plates instead of hermetically sealed covers, as stated, (by putting gaskets soaked in oil and coated with tallow,) the explosion would not have taken place.

In order to prevent a like occurrence, and to avoid the inconvenience of having the covers removed, in the deck within the breastwork, we would suggest that ventilators may be fitted between the beams, as shown in red on the accompanying sketch.

The captain of the steam reserve made the following report:—

In compliance with your minute of the 11th instant on Comptroller's letter, dated 10th June 1874, S. $\frac{5225}{5839}$, desiring me to report my opinion upon the accumulation of gas in the bunkers of the "Devastation," I have the honour to report that it appears from Captain Richards' letter that the bunker lids were left on from Friday to Monday, during which time the gas accumulated.

2. I am of opinion that all bunkers should be fitted with means for the escape of gas, independent of the bunker lids, which should be taken from the crown or highest part of each bunker into the uptake, mast, or ship's side, as might be most convenient in each case, the principle being that each bunker should be fitted with independant means of relieving itself from gas; and I would submit that such means should be fitted to the "Devastation" as soon as possible, and be provided for in the drawings of all ships in future.

The Admiralty then issued the following order dated 27th June 1874.

The Superintendents at Chatham, Sheerness, Devonport, and Pembroke, are informed with reference to the enclosed papers respecting the explosion of gas in the coal bunkers of the "Devastation" that provision is to be made in the ventilating system in all ships in future for carrying off any gases generated in the coal bunkers.

H.M.S. "DAPHNE."

(Explosion of gas in coal bunkers.)

The following is the Report of the commander of the "Daphne" of an explosion of gas which occurred in her coal bunkers on the 11th August 1874:—

H.M.S. "Daphne," Trincomalee,
13th August 1874.

SIR,

I HAVE the honour to inform you that about 6.50 a.m. on the 11th instant, one lid of port after and one of starboard after bunker were blown off, and a flash of flame was observed. Smoke was then seen ascending.

The officers and crew at once assembled at fire stations, the hoses being ready in a very short space of time; but it was not deemed advisable at first to wet the coal.

Parties of men were organised to clear out the coal into a lighter, the smoke continuing to increase, accompanied by heat, and a stoker in shovelling out the coal to the stoke hole, came upon a small portion of burning coal. I then ordered the pumps to be hove round, and in ten minutes the men were able to resume work, occasionally assisted by the pumps.

The temperature of the bunkers reported during the first and middle watches, and also at 6 a.m. was, port bunker, 95°; starboard, 92°; this was close to the seat of fire, and was not considered beyond the usual average, as the main steam pipe runs through the port bunker causing an increased temperature of several degrees to that of the starboard side.

The port bunker is always highest.

The coal (Nixon's Steam Navigation and Davidson's West Hartley two thirds to one third), was got in at Aden in a perfectly dry state, and appeared so on the 11th.

This combustion appears to me to have been caused by some peculiarity of the mineral, and may possibly have been acted on by the temperature of the steam pipe.

I decided to land the greater portion of this coal, viz. 130 tons, and shall give the bunkers a thorough ventilation, embarking only a portion of it for present use in the foremost bunker when I again complete.

I have, &c.
C. E. FOOTE, Commander.

A report having been called for by the Admiral Commanding-in-Chief on the station as to "when the bunker "lids were last taken off" previous to the accident, it appeared that through inadvertence, the bunker lids previous to the accident, had not been lifted off since the 31st July, although it had been the custom to do so every Friday.

The explosion was thus accounted for.

APPENDIX IV.

INFORMATION collected by CIRCULARS issued by the COMMISSION.

A. SHIPOWNERS.

About 250 of the following PAPERS of QUESTIONS were issued to SHIPOWNERS connected with the COAL TRADE.

QUESTIONS TO SHIPOWNERS.

1. Has spontaneous combustion or explosion ever occurred in any of your coal-laden ships?

2. If so, state all the circumstances of each case as fully as you can, on one of the inclosed forms.

3. What system, if any, of ventilation do you adopt for the holds of your coal-laden ships generally?

4. What proportion does the tonnage of your coal cargo usually bear to the N. R. tonnage of the vessel?

5. What kinds of coal have been shipped in your vessels? State the names of the collieries, with seams, and whether double or single screened.

6. How long has the coal generally been taken from the pit before shipment?

7. What precautionary measures, generally, do you adopt in your ships against spontaneous combustion or explosion, and what provision do you make for dealing with fire from these causes should it occur?

8. Have you any remarks to make, or suggestions to offer to the Commission on the subject of their inquiry?

Please write "Confidential" against any of your answers which you desire to be so treated.

The following are some of the REPLIES received to these QUESTIONS.

From Mr. THOMAS HENDERSON, South Shields.

"Columbia." (*See* p. 85.)

1. Only on one occasion, and the report by the commander accompanies this.

3. Never put any ventilators (except on the occasion above referred to, Article No. 1) in our ships' holds, but instruct the captains to keep the fore and after hatches off when at sea whenever the weather allows, so that any gas which may be evolved may escape.

4. From one fourth to one third over and above the net register tonnage.

5. We have shipped all kinds and descriptions from the best Hartley steam coals (from nearly all the north country collieries) down to the smallest smithy coals. Cannot give

the name of any particular seam, as when the ship goes to the spout only know that the coals are coming from a certain colliery, and do not know what shaft they are worked from.

6. Cannot answer this positively; but of late years should say that very few hours elapse between the time of the coals being brought to bank and being shipped in the vessel.

7. Never take any further precautions than named in Article No. 3. With regard to dealing with fire, we are so satisfied that without ventilation it will not occur that we have not given it particular attention, except to point out to any young captains the measures taken by Captain Ramsey, whose report accompanies this.

8. None further than I think ventilation is unnecessary and dangerous, as by bringing the atmosphere in contact with the gases given out of the coals, the agent that is required for combustion is being introduced.

From Messrs. T. Beynon & Co., Newport (Mon.).

"Usk." (*See* p. 86.)

1. Yes, on board the barque "Usk" in 1861. I think this vessel loaded a cargo of smelting coal at Swansea in the usual way, and took fire when at sea, 98 days from loading port.

3. My own opinion is that ventilation in ships laden with smelting coal is of very little use; the above-named ship was ventilated with shafts from each hatchway.

4. About seventy per cent. of the register tonnage over the register tonnage would be about the ship's cargo; thus a ship of 400 tons N.N.R. would carry 680 tons of coal.

5. Both smelting and steam; steam coal generally is double screened on shipment, smelting coal not at all.

6. My experience goes to show that the coal is shipped as soon as possible after being taken from the pit.

7. In steam coal-laden ships I ventilate them; in ships loading smelting coal I endeavour to get the cargo shipped dry and in dry weather.

8. To prevent the spontaneous combustion of coal I would recommend that the cargoes should be shipped in casks or barrels, which would in my opinion be a sure preventative.

From Messrs. T. Beynon & Co., Newport.

"Usk." (*See* p. 86.)

1. Yes.

3. The plan that has been in use at Liverpool.

4. About 60 per cent. over the net register tonnage.

5. Small coal or smelting coal; no well authenticated case has come to my knowledge of ships having taken fire through spontaneous combustion of large double-screened steam coal.

6. Shipped almost immediately it can be sent down from the pit.

7. In ships of mine loading coals liable to explosion from gases, I have always given orders that the hatches are not to be put on until 24 hours after the ship has been completely loaded, and never put on during loading. I never had an accident with this description of coal in any of my ships.

8. I had a barque called the "Usk," 338 tons register, which loaded a cargo of smelting coal at Swansea and sailed from that port for Valparaiso. In July 1863 she took fire when she was out from Swansea 94 days, and at the time distant from Valparaiso about 200 miles. I attributed the fact at the time to the coal being shipped in wet weather, but I forget now the exact circumstances of the case; the captain, however, stated that the first intimation the crew had of the ship being on fire was the great heat that was felt in the forecastle one morning, and upon opening the forehatch to ascertain the cause, the flames burst up as high as the fore top, and the crew had to abandon the ship at once, taking to the boats, not having time to save their effects; they were picked up and taken into Valparaiso; the ship was fitted in the usual manner with wooden ventilators, as was usual then at Swansea.

Since the "Usk" was lost I have sat as magistrate to investigate two cases of ships that were destroyed by spontaneous combustion of their coal cargoes. Both ships were loaded in this port with house or smelting coal; it was shown during the investigation that the weather had been wet when the coals were shipped, and samples of coal similar to their cargoes produced in court were proved to contain a large quantity of iron pyrites; about the same time I had a ship of my own that I loaded here myself with a cargo of smelting coal, made the passage to Coquimbo in 98 days, and in perfect safety, and the captain wrote me that the coal was quite cool during the whole time the ship was discharging. The reason my ship did not take fire, I think, was that I loaded a superior kind of coal that contained very little, if any, pyrites of iron. The two ships that were burnt caught fire when they were out from their loading port between 90 and 100 days. In the course of the investigation alluded to above, the chemist that was sent down by the Board of Trade stated that sulphurous acid gas (I think it was), if introduced into the hold of a ship that was on fire (from spontaneous combustion of a coal cargo), under certain conditions, would extinguish the fire; he described the method, which to my mind appeared cumbrous and expensive; the chemist undertook to demonstrate in a practical manner the value of his recommendation by putting the fire out in the court house, but this he failed to do.

I have thought over this subject of spontaneous combustion of coal cargoes very carefully, and I believe that although ventilation is of great value in cargoes of coal liable to emit gases that are explosive, it is of very little use in cargoes of coal that are liable to take fire from spontaneous combustion; my plan to prevent the latter (and I think it is worthy of consideration) is this:—Stow the coal in ships' holds in barrels, old flour barrels would do; one of the causes of spontaneous combustion in my opinion is that the coal is stowed in a mass, it lies close, and no system of ventilation could be of use; but in the plan I propose of stowing the coals in barrels it would be stowed in small parcels, each parcel isolated, while the air that would circulate between the interstices of the casks would tend to keep all cool; but the very fact of stowing the coal in small parcels, each distinct and protected from each other, would in my opinion be a sure preventative against ignition. I have mentioned my plan to several of my friends who are deeply interested in this question, and they agree with me that the method I propose is an effectual one to prevent spontaneous combustion; they demur, however, as shipowners to the expense that would be incurred in finding the barrels, and the loss that would be sustained by the ship taking a smaller cargo of coal in barrels than she would in bulk. My answer to this is that the loss of one ship (to say nothing of the consequent risk to life) would go a long way to providing barrels, and if merchants are desirous of shipping a dangerous class of goods such as coal, liable in its nature to spontaneous combustion, they should pay a higher freight for its carriage in proportion to more harmless goods, so as to enable the shipowners to take similar precaution to those I have pointed out in order to enable him to carry the coal without risk to life or property, and without entailing a serious loss to himself.

From Mr. William Thomson, Craig Binning, Uphall.

"Joanna," "Wanderer," "Annie Laurie," "May Queen," "Palmyra." (*See* p. 87.)

1. Yes; in five cases.

3. When the quantity of coal is a mere ballasting no ventilation; when a full cargo is taken the ships are carefully ventilated with wood, up and down each hatch.

4. When ballasted under register tonnage a full cargo about 25 per cent. above the register tonnage.

5. Duke of Buccleuch's Midlothian coal, all screened; Cowpen, Hartley, and Lancashire screened.

6. Unknown.

From Messrs. Charles Hill & Sons, Bristol.

1. No case of explosion; a cargo of smelting coals heated very much, but did not ignite.

3. All our ships intended to carry coals are fitted with a platform, 18 inches or more off the bottom, and 2 to 3 feet at bildges, and have ventilators as follows, viz., one large galvanized iron, one through the top gallant forecastle down to lower hold with bell-mouth at top, and one, sometimes two, smaller ones as far aft as they can be placed. Wood ventilators through each hatch with lapping battens (similar to venetian blinds) so that they cannot get choked.

4. 35 to 45 per cent. over net register tonnage.

5. All the different kinds of steam coal loaded at Cardiff and Newport, also some cargoes of smelting coals, and a few cargoes from the North of England.

6. Coals are generally shipped shortly after leaving the pits, and it frequently happens that that the loading is interrupted waiting for coals to come down from the pits.

7. Each ship supplied with a Davy's patent safety lamp (same as used in coal mines,) and instructions given that no other light is to be used in the ship's hold or store-rooms under the main deck; a portable fire engine and plenty buckets supplied each ship. The captains are instructed

to go below frequently and examine the cargo and test the heat of same with thermometer.

8. I attach much importance to the keeping the *bottom* of a coal cargo *dry* and *cool*, and hence consider a platform most essential. Cargoes of coal are looked upon as not liable to damage, and are generally loaded right on the bare ceiling as no other cargoes are, and the bottom of the cargo is often wet before leaving the docks, strict attention to the pumps not being considered necessary with a coal cargo; ventilators, too, are much neglected, an impression prevailing amongst shipmasters that they are of no use, and they are often fitted in a carelessly and inefficient manner just to comply with the charter, frequently only a few feet in length so as to show at the hatchway. They should be fitted from the bottom and to project above the hatchway, and be piled round with knobs of coal as the loading progresses, to keep them from being choked with small coal. I may be permitted to mention that during the last 10 years we have carried 75,000 tons of coals south of the Equator, mostly round the capes, in ships 400 to 1,700 tons register, and the before-mentioned is the only approach to an accident connected with the cargoes. Last year one of four vessels took a cargo of 1,750 tons Merthyr steam coals from Cardiff, called with it at the Cape and Point de Galle (for a market,) and finally it was discharged at Singapore after being six months on board the vessel.

From Mr. E. S. HILL, Cardiff.

"Her Royal Highness." (*See* p. 87.)

1. Only one case of inconvenience by heating ever occurred with all the cargoes our ships have carried.

3. The cargo is stowed on a platform from 18 to 24 inches from the skin. We place a wooden shaft in each hatchway, and an iron ventilator in each end going down into the main hold. The captain is instructed to keep the hatches open as much as possible.

4. Something over a tonnage and a third.

5. Mostly steam coal double-screened from Nixon's Navigation, Powell's Duffryn, and others of the principal steam collieries, with a few cargoes of smelting coal, and some from the north.

6. It is not possible to answer this question as a matter of knowledge; but I believe, as a rule, large coal is shipped within 36 hours from its being taken from the pits, sometimes much quicker; small coal would usually be longer.

7. Vide reply to No. 3. In addition, the ships are all provided with Davy's safety-lamps and portable fire engines. The captains are told to carefully notice the temperature of the cargoes.

8. I think that the losses by reason of fire in coal-laden ships have been very much exaggerated, and in support of this opinion I have the honour to enclose for the information of the Royal Commissioners a paper which I compiled bearing upon this subject, and which was adopted, printed, and circulated by the Cardiff Chamber of Commerce.

It is an established fact that the Welsh steam coal is not liable to spontaneous combustion. No case has ever been known, but it is very liable to generate gas of a highly explosive character. To prevent accidents it is only necessary to observe the precautions already described.

I have known of several cases of explosion, but I believe in each instance a naked light has been incautiously used.

In the case of small coal and smelting coal I do not consider ventilation desirable. It may be desirable to stow the cargo on a platform for the sake of keeping it dry, but I attach more importance to preventing it getting wet whilst shipping, and of taking care to keep it dry afterwards. The practice of throwing greasy slops on top of the cargo is very much to be deprecated.

From Mr. WILLIAM BUCHANAN, Greenock.

"Royal Family." (*See* p. 87.)

1. In one ship.

3. No particular system. Generally when ventilated in the shafts down each hatch, and sometimes with a trunk along the keelson; never had any heating when no ventilation used.

4. Fully register tonnage and third.

5. Do not know the names of the collieries, but have carried coals to India from Newcastle, Cardiff, Birkenhead, Glasgow, and Greenock, both single and double screened.

6. Don't know.

7. None excepting carrying a fire engine.

From Mr. J. H. WATT, Glasgow.

"Persia." (*See* p. 88.)

1. I may have owned about 30 ships at different periods which have frequently loaded coals, and have had only one case of combustion.

3. Wood ventilators.

4. About 140 p.c.

5. Chiefly from Lanarkshire, also Ayrshire and Wales.

6. Immediate shipment.

7. As a rule my ships were not insured. I have studied the subject carefully, and though the ships were ventilated I rather objected to it, and cannot give any suggestion to prevent such accidents.

From Messrs. J. BRYMNER & CO., Greenock.

"Glaramara." (*See* p. 89.)

1. Only in one about five years ago.

2. See form.

3. Ventilators in each hatch.

4. Varies say about register and third.

5. All kinds, Welsh, English, and Scotch.

6. Generally fresh.

7. As thorough ventilation as possible, and supply fire engines.

8. Except that great care should be taken in loading and proper ventilating.

From Messrs. STODDART, BROTHERS, Liverpool.

"Vanda," "Mondora. (*See* pp. 8 95.)

1. Yes.

2. "Vanda" and "Mindora" herewith.

3. Wood trunk along lower hold, and connected by upright trunks to the hatches.

4. Various. About one third to one half over the registered tonnage.

5. See paper herewith.

6. Cannot say.

7. Fire engines.

8. Cannot give any reason for fire breaking out. Do not think present system of ventilation is right. Water gets down the shafts. Becomes heated and the draught carries smoke up the shafts and feeds the fire.

From Messrs. HINE, BROTHERS, Maryport.

"Cereal." (*See* p. 90.)

1. Yes.

3. Very shallow ships, none. Deeper ships, on each side keelson with shaft communication through each of the three hatches.

4. In steam ships it depends upon the quantity of bunker coals shipped, and this depends upon the length of the voyage; including bunkers about double net register. In sailing ships 40 to 60 per cent. above net register.

5. Various kinds, in nearly all cases double screened.

7. Fire engines.

8. Coals (South Wales) have been shipped in my vessels under the same circumstances of weather, &c., as in the case of the "Cereal," but no symptom of fire showed itself, although the voyage was 40 days longer. I am therefore inclined to think that you must look to the nature of the coals themselves for the cause of so much spontaneous combustion. I am disposed to think there are some kinds that will take fire however dry they may be shipped, and however well ventilated. I never heard of a case of spontaneous combustion of coal shipped from this district (West Cumberland), and such a thing as a colliery explosion is rarely if ever heard of here. I merely name this to substantiate the opinion I have given.

I do not approve of the present mode of ventilation as I think the ventilators in the case of fiery coals act more as flues or draughts for the flames. In my judgment the most effective mode would be to divide the cargo into lots or sections entirely separate from each other by means of bulkheads extending from the bottom of the hold to the upper deck or to the top of the coal, such bulkheads to be double, say a foot apart, with sufficient cross pieces to prevent their being crushed together by the weight of the coal.

The objections to this plan would be its great costliness and the difficulty in carrying it out in ships with 'tween decks laid. The great advantage would be instead of having a large cargo lying in one mass in a ship's hold there would practically be two, three, or it might be four distinct lots with an open space of one foot between each.

I think the next best plan is to lay the fore-and-aft ventilators not in the very bottom of the ship on each side the keelson, but about halfway to the deck, say under the 'tween deck beams, thereby carrying them right through the centre of the mass of coal, where the heat always generates.

I also think it unwise to ship the coal as is now frequently done straight from the mines.

From Mr. William Laing.

ivouac.”
e p. 90.)

1. Yes. One instance of explosion, and one of spontaneous combustion.

2. See enclosure.

3. Permanent galvanized iron ventilators are fitted into each hold or compartment, one at each end of it.

4. Variable, about 11 to 7, exclusive of bunkers.

5. Nearly every kind shipped either at Newcastle or Cardiff has been carried.

6. Generally fresh from the pit.

7. The complete system of ventilation as above mentioned, and my ships all being steamers, are fitted with the usual engine pumps and ample length of fire hose.

8. I consider no coal-carrying ships safe, unless permanent iron ventilators with cowled tops are fitted into each hold or compartment, one at each end of it, forming down and upcast channels; further that naked lights should not be used in the holds on the voyage, as the gas has a tendency to accumulate in any empty or half-filled hold, especially at the fore end of the ship.

From the Secretary of the Swansea Merchant Shipowners' Company, Limited.

Alpha,”
Henry
ath.”
See p. 90.)

1. Yes, in the barque “Alpha.”

2. She was laden with smelting coals for Valparaiso and orders. The coals fired when she was near Cape Horn. The ship was taken with difficulty to Stanley and partially discharged. An agent was sent from England who repaired the ship, and she proceeded to her destination.

3. To fill in between the keelsons or platform risers and level a little below them, lay the platform and place boards (trunk boards) fore and aft on either side of the midships stanchions, allowing of ventilation in both cases.

4. Generally the full cargo of coals, about register and half gross tons.

5. Smelting and steam.

6. Shipowners as a rule do not know.

7. The above-named for ventilation to comply with conditions of charter, relying not on that, but on the subdivisions of the cargo by same means. Steam coals are explosive, and should be ventilated, or at least the hatches should not be closed for, say, 48 hours. After that there is little escape of gas to be feared. Ventilation will not arrest the tendency to spontaneous combustion, but it will aid it when once set in. In that case the ship is closed up generally as a first attempt to smother the fire, but water has to be resorted to almost invariably.

8. Not more than is embodied in the above replies to questions.

From Messrs. R. & J. Craig, Glasgow.

“County of
Nairn,”
“County of
Perth.”
(*See* p. 90.)

1. In two spontaneous combustion, the “County of Nairn” and “County of Perth.”

2. Fully stated as requested.

3. Ventilation through the main deck and one to bottom of hold.

4. Never more than N. R. tonnage of the vessel.

5. Roughrigg and Hamilton coal is always double-screened.

6. Direct from the pits.

7. Ventilation only. Leather fire hose in connexion with two rocking centre fire-engine pumps.

8. The only suggestion we can offer with a view to prevent spontaneous combustion is to build the ships in compartments of such size as to carry not more than 300 or 400 tons in each.

“Prince
Alfred.”
(*See* p. 91.)

1. Two vessels, one total loss, the other put into Monte Video and took out cargo.

3. None heretofore, but would certainly prefer trunks at certain places.

4. About 75 per cent.

5. Both Welsh and North country, but have never known but one case of my own of North country, a cargo for Hull.

6. Shipped immediately.

7. Have not previously adopted any, but should do so if loading these coals again.

7. I shall be glad if my evidence would in any way assist to prevent such destruction of property, but would suggest that the captains of these vessels should be examined. I believe their evidence of much more importance than mine.

From Messrs. J. Kerr & Co., Greenock.

“Isabella
Kerr.”
(*See* p. 92.)

1. Yes, in ship “Isabella Kerr.”

3. A wooden ventilator up each hatch, and one along keelson.

4. About 1·40 per cen

5. Main ell and splint coal.

7. Ventilation as above, and captains have instructions to open hatches in fine weather, and try heat by thermometers twice every day.

From Mr. Charles Beades, Lancaster.

“Channel
Light.”
(*See* p. 91.)

1. Yes; “Channel Light,” at Rio, October 1873.

3. Trunks fore and aft. Ventilation through deck and hatches.

4. 553 N. R., cargo 700 to 750.

5. Welsh or North country coal.

6. A short time only.

7. Ventilation. Force pump, hose, &c.

8. Allow no coal to be shipped until sufficient time has elapsed to allow of the gases to escape.

From Mr. J. S. Greive.

“Walter
Baine.”
(*See* p. 92.)

1. Yes, on board ship “Walter Baine,” lately destroyed by spontaneous combustion. Crew saved.

2. See Rundell's book, page 46.

3. Ventilators in each hatch running along keelson by a tunnel, but I do not approve of it.

4. Generally 100 to 200 tons less than tonnage and half of net register.

5. Wishaw and Sunderland.

6. Cannot speak as to this.

7. Ventilators in each hatch; have not employed any other means.

8. Doubt very much whether the ventilators are not positively injurious. I have a strong opinion coal should be shipped dry. High-valued ships are practically shut out from taking coal charters on long voyages, because of the high rate of insurance on the ship; as a rule low-classed wooden ships must be employed, probably lengthening the passage, and consequently the risk of spontaneous combustion will be increased.

From Mr. George Duncan.

“Persia.”
“Indian
Empire,”
“Khersonese,”
(*See* pp. 91, 92, and 97.)

1. Yes, in three cases.

3. Longitudinal shaft on each side of keelson and upshafts connected therewith coming out of each hatchway.

4. About 30 per cent.

5. Have carried all principal Hartley steam coal from Tyne and Wear, and from the chief collieries in Cardiff.

6. Cannot say, but believe much less time than formerly.

7. Cargoes ventilated, and ships provided with fire engine and head pump.

8. Being member of Commission can do so orally.

From Messrs. W. & R. Wright.

“Mooresfoot.”
(*See* p. 93.)

1. Yes.

3. In all our wooden ships we have a wooden platform under the coal, and venetianed upright shafts which extend from under platform to top of each hatch.

4. This depends upon the model and age of the vessel and the voyage. Our last built wooden vessels are built with a view to carry large dead weight cargoes, and as a rule carry fully tonnage and half. Iron ships usually carry about tonnage and one third.

5. Wishaw coals from Dundee, West Hartley, from Shields, Lancashire, from Liverpool, North and South Wales, from Birkenhead.

6. We believe three or four days.

7. None but the above system of ventilation, which we have adopted for a long time now; it has acted well, and is in favour with underwriters. Engine hose, and buckets.

8. Merely this, that if our plan of platforming and ventilating was universally adopted the number of cases of spontaneous combustion would undoubtedly be diminished.

From Mr. W. R. Price, London.

“Oliver
Cromwell,”
“Calcutta.”
(*See* p. 94.)

1. Have had two cases of spontaneous combustion and total loss, say, ships “Oliver Cromwell” and “Calcutta.”

2. Both ships sailed from Shields for Aden. The “Oliver Cromwell” on the 18th June, and the “Calcutta” on 2nd July 1874. The “Oliver Cromwell” was burnt on 6th September 1874, in latitude 37° S., longitude 14° E. The “Calcutta” was burnt on 17th September 1874, in latitude 35° S., longitude 7° E., being respectively 80 and 77 days out. In both cases the fire was discovered in the main hatch ventilator.

The captain of the “Calcutta” was lost, so no protest giving exact particulars has been obtained; but from some of the men saved, I learn that there was no indication of fire till immediately before it broke out.

In the case of the “Oliver Cromwell,” I have been told that there was no smell of gas prior to the fire, but that a peculiar vapour was noticed from time to time coming up he ventilator some days previous to the fire, which, however, was considered as simply indicating the ordinary sweating of the cargo.

3. Both ships were well ventilated by 4-feet ventilator from the top of keelson to the ceiling, and two horizontal ventilators of wood on top of beams a foot square midway between 'tween deck stanchions and the side of ship. These were connected with three 18-inch upright shafts going through each hatch, the sides of which were arranged like a venetian blind when two thirds open. This is the same system I have adopted successfully for 17 years with coal shipped from Birkenhead to the East.

4. The "Calcutta" was 1,372 tons net register, and had on board 1,802 tons coal. The "Oliver Cromwell" 1,112 tons register, 1,616 tons.

5. The coal in these two cases was, I believe, Howard's West Hartley, shipped on account of the Peninsular and Oriental Company. I cannot say whether it was screened or not, but should think it was shipped as screened, although I noticed a considerable amount of small coal in the main hatchway in the lower hold of the "Oliver Cromwell."

6. I believe the coal to have been taken recently from the pit, but I have no exact evidence on the point.

7. My captains have always been instructed to keep the hatches off whenever the weather permitted, and have always been supplied with plenty of good hose and a force-pump for working same.

8. Having carried coal largely during a period of 17 years from Birkenhead to the East without even a heated cargo, and having shipped it by tips as well as by barrows, I am led to the conclusion that in the losses above referred to there must have been some speciality in the coal rendering it more liable to spontaneous combustion than the North Wales coal, and this view is strengthened by the fact that at the same time two other vessels loaded in the same dock, I believe with the same coal, one of which, the "Euxine" was also burnt, making three out of four ships. The matter is specially referred to in a report from one of the officials of the Peninsular and Oriental Company to the directors (copy of which I enclose*), and I commend to your notice the fact that the fourth ship the "Cora" was not ventilated through the coal, and arrived at her port, although a portion of this cargo got wet with rain and hail during loading, and was therefore more liable to heat.

I have been told by the members of the crews of the "Oliver Cromwell" and "Calcutta" that the seat of the fire in each case was well down in the lower hold under the main hatchway, and as the bulk of the cargo is shipped in this hatchway, there is necessarily a greater density in the mass and a larger accumulation of small coal.

The height of the tips from which it was shipped in the Tyne has a tendency to break the coal, and make the mass less impervious to ventilation in that region. The captain of the "Oliver Cromwell" told me that his ventilators were perfect in their action, as he frequently examined them and found that a current of air passed through them up the shaft.

I entertain a decided preference for the mode adopted at Birkenhead of loading largely by barrows instead of by tips, as the coal is more carefully handled and it is stowed more gradually over the hold, besides which the labourers employed have the opportunity when shovelling it into the barrows of picking out all objectionable looking pieces, which I believe they do. I have shipped it from Birkenhead at all seasons of the year, and when it has been saturated with rain in the railway trucks, without any detriment except loss of weight on delivery.

I have had no previous experience of coal from the Tyne, never having had ships loaded there before.

I would pointedly call your attention to the close approximation in the number of days from sailing to when the fires occurred, say, "Oliver Cromwell" 80 days and "Calcutta" 77 days.

From Messrs. W. & J. Smith, London.

"Eeta," "Enthusiaste," "Try," "Crystal." (*See* pp. 94-97.)

1. Yes, in two partly owned by ourselves, and two chartered vessels. Our ships were "Crystal" and "Eeta" chartered were "Try" and "Enthusiaste."

3. Our vessels being employed in the sugar trade, have ventilators fore and aft. We know of nothing better for coals, but we do not generally carry full cargoes of coal.

4. Never take full cargo except purchases and charters not under our management; by this we mean in our own ships.

5. Given on separate papers.

6. Six to ten days.

From Messrs. Agacio & Co., Liverpool.

"Alexandre." (*See* p. 94.)

1. Spontaneous combustion has occurred.

3. See enclosed form.

From Mr. J. E. Burgess, Swansea.

"Admiral Fitzroy." (*See* p. 95.)

1. Yes.

3. When loading smelting coals I generally have the main body of the cargo, say from abaft the mainmast to the foremast, divided by running up deals on either side of hold stanchions from keelson to main deck, leaving it open at the top.

4. About 45 per cent. over N. R. tonnage.

5. Smelting coals through and through as worked from the collieries.

7. Nothing more than as given at No. 3.

From Mr. Thomas Law.

"Dumbartonshire." (*See* p. 95.)

1. No, only in "Dumbartonshire" with 540 tons balance of cargo, general.

3. 'Tween deck beams longitudinal; also keelson ditto; and the three hatches perpendicularly.

4. 50 per cent more.

5. All kinds from Cardiff, Birkenhead, and Clyde.

6. Don't know.

7. We take no precautions except ventilating.

8. No.

From Messrs. Hughes & Co., Menai Bridge.

"Glen Tilt. (*See* p. 96.)

1. Yes, combustion by one ship.

3. Our wooden ships invariably carry platforms open at both ends, and the opening kept clear to main deck ventilators, and our iron vessels are so constructed that they afford ample ventilation.

4. Once and a fourth of the register tonnage.

5. Mostly steam coals.

6. Cannot reply.

7. Ventilation as described above. Answered in the enclosure.

From Messrs. Seed, Armstrong, & Co., Liverpool.

"New Lampedo." (*See* p. 97.)

1. Yes, in one.

3. Platform and deck ventilators.

4. Register and third. Sometimes a little over; varied according to ship I am loading.

6. Cannot say.

7. Caution master to leave hatches off at all times when practicable. Have had nothing but fire engine and hose.

From Mr. D. Stuart.

"Staffordshire," "Monmouthshire." (*See* p. 96.)

1. Spontaneous combustion has occurred twice.

3. The ordinary system wooden ventilators.

4. In the first case the quantity of coals was two thirds of the registered tonnage.

In the second case it was the entire cargo.

5. Lancashire steam coals. The White Moss Colliery single screened.

6. I do not know, but I fancy immediately.

7. This I have left to the management of the captains.

8. None.

From Messrs. Balfour, Williamson & Co., Liverpool.

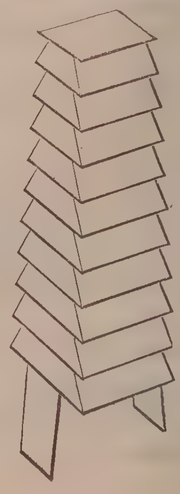
"La Escocesa." (*See* p. 97.)

1. Yes.

2. Once in ship "La Escocesa."

3. Fore and aft along keelson, and three vertical ventilators so constructed as to prevent admission of small coals, and at same time grant free exit to gas.

Vertical ventilators constructed as per sketch:—



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* This report of Captain Steinson's was read by Mr. Price before the Commission, and will be found in his evidence, Q. 1202.

4. About 45 per cent. over register tonnage.

5. Lancashire, North and South Wales coals.

“La Escocesa” was loaded in Cardiff with South Wales coals fresh from the pit, and raining heavily while she was loading.

6. Of late years only a very short time before shipment.

7. Hatches off during fine weather at all times. Fire-engine, hose, and buckets always on board.

8. Coal-laden ships should either be ventilated properly or not at all. If only partially ventilated, and heating should take place, the partial ventilation causes them to burn rapidly. If not ventilated at all, they may occasionally be kept smouldering for weeks, and thus give the ship an opportunity of reaching some port.

From Mr. E. Bates.

“Euxine,” “The Foundling.” (*See* pp. 93 and 98.)

1. “Euxine” and “The Foundling.”

3. Generally a ventilator run fore and aft through the coals, of wood connected with a perpendicular shaft in each hatchway.

4. Varies according to the vessels, “Euxine” 5 to 4. Usually about one third more than the register tonnage.

5. Almost all kinds exported at various times.

6. Unknown.

7. Have never had a case of explosion. Ventilation prevents this; and have never had a case of combustion until last year, 1874, when besides the above known cases we have lost two other coal-laden ships which have not been heard of, and are “missing.”

From Mr. D. Kennedy, Liverpool.

“City of Richmond.” (*See* p. 98.)

1. Yes.

3. Platformed.

4. 25 per cent. to 30 per cent. more than the register tonnage.

5. Different collieries “said” to be double-screened.

6. A few days if loaded in the Mersey, a shorter time if in the Tyne.

7. Ventilators up each hatchway, and orders to have hatches off in fine weather.

8. Quite at a loss what to suggest. For I say (20 at least) found platform of 3-inch deals from mizen mast foremast with ventilators up each hatch sufficient, but lately have had two ships on fire from coal cargo. Think the quality and character of coal changed.

From Messrs. Elliott, Lowrey, and Dunford, Newcastle-on-Tyne.

“Broomhaugh.” (*See* p. 99.)

1. Yes, in the case of the S.S. “Broomhaugh.”

3. Since the accident above steamer has been fitted with ventilators in the holds, but generally our masters are instructed to uncover the hatches whenever the weather will permit.

4. Our cargoes average ·87 over the N.R. (this of course includes fire coal).

5. Our steamers have carried nearly all kinds of Welsh and North country steam and gas coals.

6. Our impression generally is that coals are shipped within, say, 12 to 24 hours of being worked.

7. As explained above our captains are instructed to remove hatches in fine weather.

Our ships are furnished with rivetted leather hose of sufficient length to reach all parts, of the ship, besides being provided with a proper complement of water buckets.

8. Apart from explosion on board a coal-laden ship from extraneous causes, as in our case, we are inclined to the idea that too little attention is paid to the shipment of coals in vessels going long voyages.

With reference to ventilation, provided coals are shipped and kept in a dry state we would certainly look upon it as a preventive against spontaneous ignition; but on the other hand, should they be shipped after being exposed to rain, or from wet workings, we fancy ventilation would rather assist combustion than otherwise.

From Messrs. Livingston, Briggs, & Co., 31 Great St. Helen’s, London.

1. As owners none. As charterers for the Peninsular and Oriental Company, we believe several have been burnt, but whether by spontaneous combustion or otherwise we are unable to say.

2. These can be better answered by the owners or masters of the vessels.

3. Ventilation is adopted and stipulated for in our charters, but we are unable to state the exact means. This is left to the owners and the Peninsular and Oriental Company’s inspectors at the loading ports.

4. About 40 per cent. to 50 per cent. over.

5. All descriptions, namely, Powell’s Duffryn, Ocean Merthyr, Letty Shenks, North Wales Main Scotts Brymbo, West Hartley Main Carrs, Davidson’s, Howard’s, Bower’s and Bebside’s Hartley.

6. Sometimes the same day, but generally a few days before.

7. This is left entirely to the masters of vessels.

8. Nothing especial, we believe. It is more a question for chemists.

From Mr. C. Thompson Wigram, Blackwall Yard.

1. In one case.

2. The coal was taken direct from the pits into the ship’s hold in wet weather at Cardiff, and subsequent bad weather prevented the hatches being kept open at sea.

3. We do not take any special precautions to ventilate the coal bunkers of the steamships in which coal is stowed in bulk to the extent of three or four hundred tons. The coal is never taken direct from the collieries to the ship’s hold.

5. Nixon’s Navigation, and Hartley’s.

6. Ten to fourteen days.

7. No special precautions are taken beyond those adopted for the general protection of the ships from fire.

From Messers H. Fernie & Co., Liverpool.

1. No.

3. System of ventilation explained in accompanying letter.

4. From one fourth to nearly one half in excess of the N.R. tonnage according to the build of the different ships.

5. From Birkenhead and Cardiff the usual diversities of North and South Wales steam coal. From the Tyne, principally the West Hartley.

6. When shipped from Birkenhead we should say from two to three days. From the other parts we believe considerably less time.

7. Having taken such thorough precaution to prevent any fire by ventilation, we have hitherto left the masters to divise any special means of dealing with fire if it did after all arise.

8. We believe that if all coal could be exposed to the influence of the air for two days before shipment the risk of spontaneous combustion would be decreased.

Sir, Liverpool, 27th May 1875.

In reply to your letter of 24th instant we beg to state that in the interval from 1866 to end of 1874 we had despatched 49 large ships fully laden with coals to various ports of India and South America, and that every one of these ships carried their cargoes safely although some of them had the coal on board for fully six months.

During all that period we have followed out the one principle of ventilation, though a little varied, in the wooden and iron ships respectively; and, as our system has been so successful, we have thought it best to send you pen and ink sketches of same, marked No. 1 for iron ships and No. 2 for wooden ships, for your further information.

Besides this ventilation, however, we invariably lay a platform of deals secured by spike nails to bearers laid across the bottom of the ship, and extending from the keelson to the lower part of the bilge in breadth, and extending nearly the whole length of the ship. This platform stands about 16 inches above the ceiling beside the keelson, and runs off to nothing at the bilge, and the lower ends of the upright louvre or venetian ventilators rest on the keelson and platform, through which sufficient air holes are bored in the proper places to allow a continual current of air passing along the ship’s bottom all the way from the fore hatch to the after hatch.

We may mention three cases in which we think the value of our mode of ventilation was specially tested.

First, in the case of the ship “Iron Cross,” which sailed from Shields to Bombay on 30th November 1872, and having encountered one of the furious gales of that season off Scilly had her decks swept, and put back to Portland with 9 feet of water in her hold, which had come in through the decks and stanchions being injured, and which had passed through the body of coals, thereby rendering them more dangerous for fire. The ship being refitted reached Bombay on the 2th May, which was 5½ months after sailing.

Second, in the case of the “Royal Alexandra,” which sailed from Shields for Bombay on the 11th September 1874. This ship by inadvertance was laden 5 inches deeper than usual. Her cargo was West Hartley coal (and several ships whose cargoes consisted of that description of coal and sailing very nearly about the same date were burnt), and she made a long passage of 161 days, but her cargo remained perfectly cool and safe until discharged.

Third, in the case of the "Grampian," which sailed from Birkenhead with a cargo of North Wales steam coal on 1st October 1874, and also, as usual at that season of the year, made a very prolonged passage of 158 days, and her cargo was quite cool and safe till discharged.

We may mention that our orders to the captains are very strict, that all the hatches should be off in good weather, so that for a great portion of the voyage in most cases a further and large current of air is brought on the surface of the cargo.

Should it be thought necessary we could have a wooden model made at very little cost showing this plan of ventilation more effectually than by the sketches which accompany this letter.

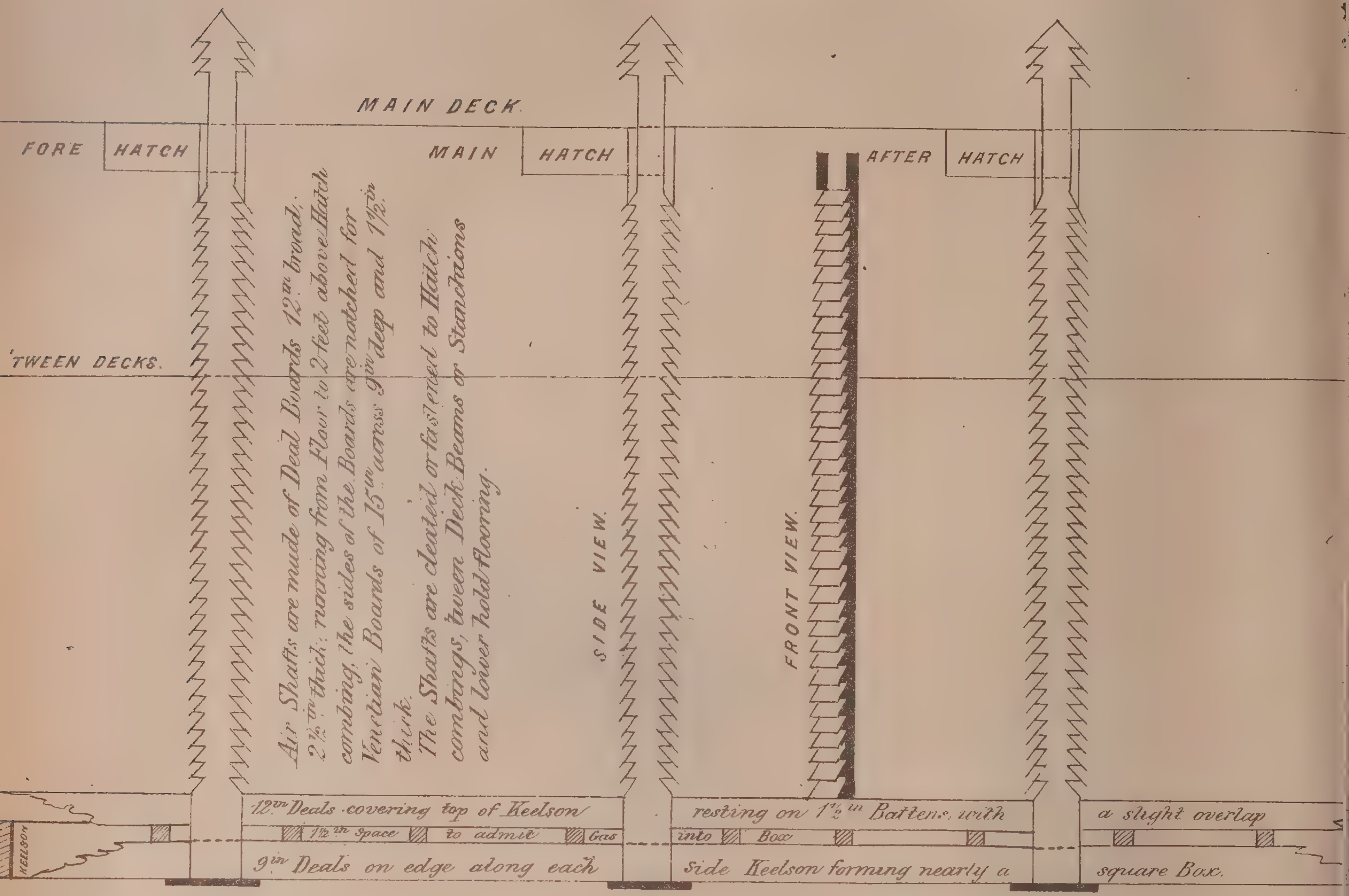
We are, &c.,
J. M. Carmichael, Esq. HENRY FERNIE & SON.

MEMORANDUM of COAL-LADEN VESSELS.

Sailing Date.	Ship.	Ship's. Registered Tonnage.	Coals. No. of Tons shipped.	From	To
1866.					
November 24	Lady Rowena	1,181	1,688	Cardiff	Rio de Janeiro.
1867.					
July 1	Juventa	1,324	1,744	Do.	Muscat.
1868.					
January 9	Adelaide	1,693	2,074	Birkenhead	Aden.
April 25	Chrysolite	1,097	1,466	Greenock	Port Blair.
September 8	Lancashire Witch	1,574	2,035	Birkenhead	Bombay.
„ 8	Juventa	1,324	1,630	Do.	Do.
1869.					
September 22	Do.	1,324	1,644	Do.	Do.
December 22	Queen of the Mersey	1,227	1,602	Cardiff	Rio de Janeiro.
1870.					
January 29	Adelaide	1,693	2,190	Birkenhead	Aden.
February 26	Lady Rowena	1,181	1,644	Newcastle, N.S.W.	Bombay.
March 18	Chrysolite	1,097	1,455	Cardiff	Singapore.
June 6	Feronia	1,250	1,649	Do.	Aden.
July 7	Vancouver	1,065	1,436	Do.	Singapore.
August 6	Flower of the Forest	937	1,302	Do.	Monte Video.
September 15	Queen of the Mersey	1,227	1,653	Do.	Do.
„ 23	Robert Kerr	1,191	1,640	Birkenhead	Bombay.
November 28	Royal Adelaide	1,385	2,033	Do.	Do.
„ 30	Juventa	1,324	1,600	Do.	Do.
1871.					
February 13	Chrysolite	1,097	1,413	North Shields	Aden.
April 6	Lady Rowena	1,181	1,728	Do.	Bombay.
May 26	Lancashire Witch	1,574	2,000	Do.	Mollendo.
September 16	Feronia	1,250	1,720	Birkenhead	Aden.
October 15	Iron Cross	1,508	1,844	North Shields	Signapore.
„ 25	Waterloo	1,222	1,700	Birkenhead	Bombay.
December 12	Robert Kerr	1,191	1,650	Do.	Signapore.
1872.					
March —	Melpomene	1,439	1,950	Sydney, N.S.W.	San Francisco.
April 27	Timour	1,330	1,600	Sunderland	Aden.
„ 27	Adelaide	1,693	2,029	Birkenhead	Do.
May 11	Chrysolite	1,097	1,415	North Shields	Singapore.
October 7	Lancashire Witch	1,574	2,000	Do.	Mollendo.
„ 7	Royal Alexandra	1,383	1,746	Do.	Bombay.
November 9	Magdala	797	1,030	Cardiff	Monte Video.
„ 30	Iron Cross	1,508	1,704	North Shields	Bombay.
December 11	Robert Kerr	1,191	1,616	Birkenhead	Do.
„ 14	Feronia	1,250	1,624	North Shields	Mollendo.
1873.					
May 15	Vancouver	1,065	1,474	Birkenhead	Point de Galle, Ceylon.
June 24	Lady Rowena	1,181	1,648	North Shields	Do. do.
August 1	Royal Alfred	1,239	1,653	Newcastle, N.S.W.	San Francisco.
„ 28	City of Manchester	1,791	2,496	North Shields	Calcutta.
September 6	Cashmere	1,245	1,866	Do.	Bombay.
October 13	Prospero	1,328	1,977	Birkenhead	Do.
December 21	Adelaide	1,693	2,057	Do.	Aden.
1874.					
May 1	Robert Kerr	1,191	1,652	Do.	Singapore.
„ 27	Melpomene	1,439	1,805	Newcastle, N.S.W.	San Francisco.
June 27	Idomene	1,390	1,824	Sunderland	Do.
July 14	Great Victoria	2,278	2,508	London	Calcutta.
„ 30	Chrysomene	1,777	2,585	Newcastle, N.S.W.	San Francisco.
September 11	Royal Alexandra	1,383	1,794	North Shields	Bombay.
October 1	Grampian	1,492	2,293	Birkenhead	Do.
	Total tons	65,871	86,886	Tons of coal.	

Nº 1.

VENTILATION FOR IRON SHIPS.

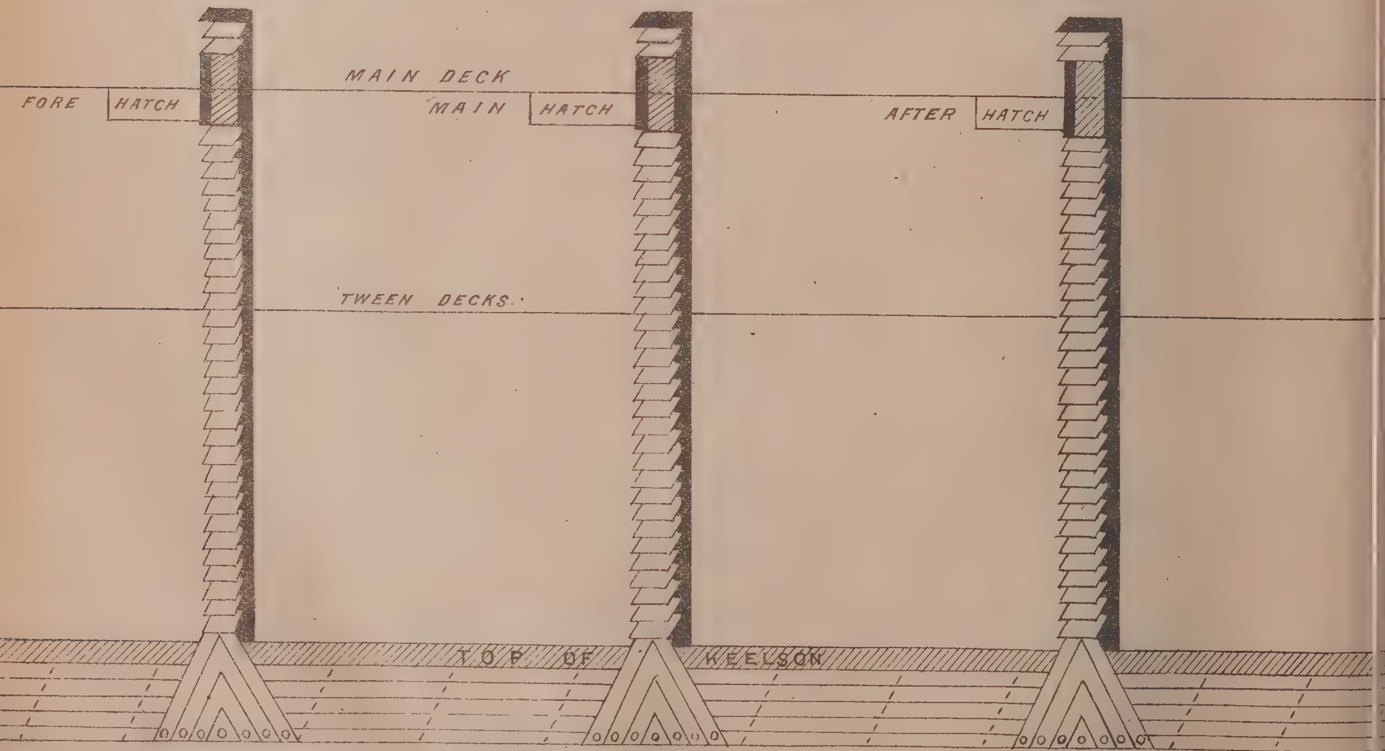

MAIN DECK
FORE HATCH
MAIN HATCH
AFTER HATCH
'TWEEN DECKS.
SIDE VIEW.
FRONT VIEW.
KEELSON
12in Deals covering top of Keelson resting on 1½ in Battens, with a slight overlap
1½ in Space to admit Gas into Box
9in Deals on edge along each side Keelson forming nearly a square Box.

Air Shafts are made of Deal Boards 12in broad; 2½ in thick; running from Floor to 2 feet above Hatch combing, the sides of the Boards are notched for Venetian Boards of 15in across 9in deep and 1½in thick.

The Shafts are cleated or fastened to Hatch combings, 'tween Deck Beams or Stanchions and lower hold flooring.

Nº 2.

VENTILATION FOR WOOD SHIPS, *WITH HIGH KEELSONS.*



Fore and aft Battens laid sloping, with small overlap from 2 feet off bottom of Keelson up edge of Keelson, on pieces Deals notched and forming venetian work, as in above Air Shafts, also large auger holes bored through Timbers & Platform, if any, for escapement of foul air into air shafts.

Dangerfield, 22 Bedford St. Covent Garden

2 ships	Cardiff to Rio de Janeiro.
1 ,,	do. Muscat.
2 ,,	do. Singapore.
1 ,,	do. Aden.
3 ,,	do. Monte Video.
1 ,,	North Shields to Aden.
5 ,,	do. Bombay.
3 ,,	do. Mollendo.
2 ,,	do. Singapore.
1 ,,	do. Point de Galle.
1 ,,	do. Calcutta.
5 ,,	Birkenhead to Aden.
10 ,,	do. Bombay.
2 ,,	do. Singapore.
1 ,,	do. Point de Galle.
1 ,,	Newcastle, N.S.W., to Bombay.
3 ,,	do. San Francisco.
1 ,,	Sunderland to Aden.
1 ,,	do. San Francisco.
1 ,,	Greenock to Port Blair.
1 ,,	Sydney, N.S.W., to San Francisco.
1 ,,	London to Calcutta.
49	Total number of coal-laden ships.

From Mr. E. T. Gourley.

1. None.

3. Wooden longitudinal spouts with upright shafts.

4. This varies in accordance with the voyage.

5. Every description.

6. This depends upon the coal demand. When the demand is slack colliery owners pile their heaps, and ship them off when trade is brisk.

7. Board of Trade fire engine and hose regulations.

8. That all coal-laden ships for long voyages be fitted with bildge fore and aft, and upright air shafts of wood or metal.

From Mr. H. E. P. Adamson, North Shields.

1. No.

3. The steamers are ventilated in each hold. The 'tween deck ventilators being venetians, with the usual cowls on the upper deck.

4. It depends on the quantity of bunker coals we take on board of our steamers.

5. All descriptions of Northumberland and Durham coals, and some descriptions of Welsh coals.

6. From 12 to 24 hours.

7. The sailing ships we have are engaged in the Mediterranean trades, and are ventilated daily from or through the hatches. The steamers go to India, America, the Mediterranean, and the Baltic.

8, No.

From Mr. John S. De Wolfe, Liverpool.

1 No.

3. Venetian wood ventilators in each hatch, let into fore-and-aft ventilators on each side of the keelson; also in lower hold ventilators from side of ship into up-and-down ventilators; iron masts, pump well, and chain lockers also constructed to act as ventilators,

4. Depends upon the model, &c. of the vessel. Refer to list attached of vessels loaded. Don't take register tonnage into account.

5. Names of collieries unknown. Believe coals generally double screened. Always so when purchased on our own account.

6. Unknown, but we believe generally shipped immediately.

7. Instruct captains to keep off their hatches in fine weather. Supply the vessels with fire engines.

From Mr. J. R. Hawley, Liverpool.

1. No.

3. Wooden passages with upright shoots.

4. Tonnage and third.

5. North Wales and Lancashire steam coals single screened, and cargoes from Newcastle, N. S. Wales, to India, and China.

6. A week to ten days.

7. I have arranged ventilators or passages made and composed of substantial timber planking, open at corners placed and running on each side of the keelsons, similar passages placed 6 to 8 feet up in the body of the coals or fuel along the bildges. All my ships have 'tween decks laid, and I cause similar passages to be carried on each side thereof in the body of the fuel.

The passages all communicate with upright shafts through each and every hatchway, and made of sufficient size to admit a big lad to go down these shafts for inspection. These are kept open in all weathers, but in case of its being very bad have venetian caps to lift on and off.

The cargo is trimmed well up to the decks in the centre of the vessel, keeping a clear space at both ends the which having grating communications also act as ventilators through lower forecastle and lazarettes. Masts when of iron are bored, and are air conductors of great merit. Should fire occur each vessel carries two very powerful force pumps. Fire engines kept ready for use at all times. I carry lightning conductors to each masthead; these have saved the vessels very frequently when struck.

8. No vessel should go to sea without sufficient lightning conductors.

My vessels being high-classed wooden and composite ships engaged in the Indian and China trades, and carrying large and very valuable cargoes, both on my own and general account, ventilation has at all times had my most particular attention, and is essential to safety both to ship and cargo, and is too little attended to.

I never allowed coals to be shipped in wet weather, and I think this is a point that should call for strict attention, particularly so in large and bulky cargoes.

Having run very many times uninsured on my ships and cargoes I was naturally very careful on all their equipment. I consider ships are too lightly manned, and the class of seamen are very deteriorated of late years. I should mention that in 1872 I lost a ship, the "Sea Pink," classed at Lloyd's, just out of graving dock after caulking and re-coppering. She took on board a cargo of coals at Shields for my own account and sailed for Calcutta. Ventilated as noted. She passed the Downs after bad weather, from where I had letters from Captain McKenzie, an old tried servant, stating everything satisfactory, cargo perfectly cool, and little or no difference in the atmosphere of the hold. I have never since heard of him or any of her crew.

I do not attribute her loss in my own mind to fire. My impression is that she was lost in very bad weather off the Cape of Good Hope, in which being afloat she would likely be caught, and Lloyd's list reported a vessel sighted after apparent collision or accident in great distress, the description answering to the paint and of the said vessel. I tried perforated iron ventilators on two or three occasions (Meacock's patent), found the weight of material too great for them to bear, and the iron pipes collapsing practically it was useless.

From Messrs. Adamson and Ronaldson, London.

1. No.

3. If platformed, shafts to same with venetian sides; otherwise dunnage laid each side keelson fore and aft, with shafts as above through each hatchway, besides the ventilation arrangements on the upper deck. Instructions given to officers to keep the hatches off at all times as long as possible, but not to allow the cargo to get wet.

4. About one third over, sometimes a little more, sometimes a little less, according to the dead weight capacity of the ships, which of course vary.

5. Steam coal, gas coal, and patent fuel. We can hardly specify the collieries, but should say from most of the leading ones of Tyne, Cardiff, and Birkenhead. Almost always double-screened, but from what "seam" never comes under our notice.

6. This we cannot say. Shippers only could give this information.

7. Replied to under No. 3. All our ships are supplied with good fire engines, hose, and buckets.

8. We consider if the above precautions are adopted, and the cargo not put into the ship for 24 hours after being in the trucks, there should be little danger. Also that small coal or dust should not be allowed to accumulate in large quantities in the hatchways under the cargo. It stops ventilation, and, if damp, is liable to heat by pressure of the coal above.

From Messrs. H. N. Hughes and Stephen, London.

1. None to our knowledge over a period of say 25 years, during this time having been very heavy shippers.

2. See above.

3. We ventilate our ships as recommended by the Salvage Association Surveyors in slip 221.

4. Large spruce ships carry from 40 to 50 per cent. in excess of registered tonnage on draughts approved of by surveyors. Other wooden ships about 30 to 40 per cent. more than their register tonnage. Iron ships vary very much in their carrying. The best carriers take nearly the same proportion as spruce ships.

5. We ship only North Wales main seam coal from collieries on the list of the Great Indian Peninsular Railway Company, which has been inspected and tested by the consulting engineer of that company. All the coal according to agreement is double-screened, and pyrites are carefully picked out at the collieries.

6. We believe 24 hours is the shortest interval between raising coal from the pits in North Wales and shipment of same at Birkenhead.

7. We consider careful ventilation, as explained in the Salvage Association slip 221, also care in picking out pyrites, both at the collieries and again alongside ships on shipment, and the loading of ships by handbarrows instead of tips, to be the best precautionary measures against spontaneous combustion.

From Mr. James de Wolf, Liverpool.

1. No.

3. A ventilator alongside of the keelson fore and aft each side, and a ventilator from the keelson to main deck in the fore, main, and after hatches.

4. About 50 per cent. above register tonnage; sometimes more, according to the build of the ship, voyage, and season.

5. All kinds of coal double-screened as far as I know.

6. I do not know.

7. I take no other measures than those mentioned herein. I have never had an accident during 30 years' experience.

8. I consider coal taken directly from the pit and put on board ship more likely to ignite than coal remaining on the bank some time before shipment. I disapprove of too much ventilation.

From Messrs. Leitch and Muir, Greenock.

1. No.

3. Wooden ventilators along both sides of keelson and below 'tween deck beams connecting with upright wooden sparred ventilators in each hatchway. Hatches kept open at all times when state of weather admits of this being done.

4. Very nearly tonnage and half.

5. Lancashire Welsh, and Scotch steam. Cannot give the names of collieries. Coal is usually double-screened for shipment to India. The shipments have been made at Birkenhead, Cardiff, and Clyde.

6. Cannot say, but believe in most of the cases it came direct from the pits to the ships.

7. Beyond keeping the hatches open and watching the temperature of the hold, as shown in the state of the air in the ventilators, do nothing else; fire engine and head pumps, with hose for throwing water on the cargo.

8. Suggest fitting shifting boards fore and aft along both sides of hold, stanchions carried up to close beneath the deck, or piling stowwood alongside of the hold stanchions at various parts of the ship.

From Mr. W. O. Taylor, Dundee.

1. Out of nearly 40 cargoes British, and some half-score Australian coal shipped for the East and West Indies, and East and West Coast, South America, I have not had one case of spontaneous combustion or explosion.

N.B. All vessels carrying the above have been under 1,000 tons register.

3. Simply keeping the hatches open when practicable, being of opinion that all other methods have a tendency the opposite of that desired.

4. In most cases say full deadweight capacity.

5. Chiefly steam coal from various collieries, shipped in Wales, the rivers Tyne, Mersey, Clyde, Forth, &c.

6. Different periods, according to the demand and convenience of the sellers, but prefer coals to have been worked some time previous to shipment.

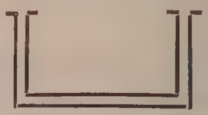
7. None beyond seeing that the coals are shipped in as dry a state as possible, and having proper attention given to the ventilation as above.

8. I am decidedly of opinion that ventilation by wooden ventilators tends to spontaneous combustion, and I believe the first coal-laden vessel that caught fire in this way (the "Humayoon") sailing from Dundee, was the first to adopt the new method of ventilation, say by introducing wooden ventilators amongst the coals.

From Messrs. Edward Shotton & Co., North Shields.

1. Nothing to speak of. In one case many years ago a vessel's keelson was partially burnt. I think she was loaded at Birkenhead. Water was pumped into the vessel and the fire extinguished. This occurred at Point-de-Galle. This is the only case happened to any of our vessels.

3. Ordinary ventilating shaft communicating with each hatch, thus—



4. About one half more; in summer time a little more.

5. Generally steam, but sometimes gas.

6. Generally about two days.

7. A current of air is passed from the fore to the aft hatch by means of shafts. Fire hoses.

8. No hatches should be battened down for some hours after the vessel is laden. Length of time depending on description of coals shipped. Fore-and-aft hatches we have built 2½ to 3 feet high, so that at almost all times excepting when the sea is very heavy they can be open,

This we have practised for from 20 to 30 years, and to this we mainly attribute our escape from the risks above alluded to, for the gas has no means of collecting in sufficient quantity to do damage.

From Messrs. Stuart and Douglas, Liverpool.

1. No.

3. A ventilating shaft, 24 in. by 16 in., constructed of boards with a sufficient space between the boards on each side of the shaft to allow of any gas that may be thrown off by the coal to pass through these gaps into the ventilating shaft, and so be conveyed at once to the outer air, also a chamber of boards enclosing the keelson of the ship from end to end. The shafts above described are fixed into this chamber; one shaft is placed in each hatchway, and the shafts project 4 feet above the top of hatch.

4. Generally about 1⅓. The average of 25 coal cargoes gives 1·225.

5. We have shipped Bower's West Hartley coal from the Tyne, Wigan coal under charter from Liverpool, and North Wales steam coal from Birkenhead. We cannot give the names of the collieries. We believe the coal has in all cases been well screened.

6. The last cargo from the Tyne (and which has arrived safely at its destination) was shipped in about an hour after leaving the pit. The coal shipped at Birkenhead has, we believe, been not less than two days out of the pit before being shipped.

7. Copious ventilation as above shown and having the hatches opened during the voyage as often as practicable, and regular attention to the ship's permanent moveable headed ventilators. For extinguishing fire the ship is provided with fire engine and hose, and set of fire buckets; but we rely mainly upon preventive measures.

8. We have to remark that we attach great importance to the way in which the ventilators are fitted in the ship. We always employ our own carpenters, and spare no expense to have the work done substantially. From the careless way we have seen this kind of work done, we are quite sure it would give way with the pressure of the cargo, thus effectually breaking the current of ventilation, and probably ending in the ignition of the pent-up gas.

From Messrs. Abram Lyle and Sons, Greenock.

1. None.

3. Vertical wooden ventilators in each hatch, and fore-and-aft ditto along keelson.

4. About tonnage and third, varying a little according to circumstances.

5. Principally Wishaw coal, say Watson's, Hartley, and Spencer's splint, double-screened; seams unknown to us.

6. Latterly we understand a very short time, perhaps about a week.

7. We rely chiefly on our strict injunctions to the captains to have the hatches off on every possible opportunity; but if fire should take place, the instructions are to flood the ship by means of a fire engine, and at the same time to keep throwing overboard the coal until the fire is overcome.

8. We think if large stringly-sparred ventilators (vertical), sufficient to let a man easily down, were put in each hatch, they should both give better ventilation than the small ones now in use, and would also, in the event of fire, let a man down to the very bottom of the coal with a hose. A more systematic style of stowing might also do good.

From Mr. G. A. Caws, Northumberland.

1. No.

3. No regular system, only hatchway and deck ventilators.

4. Nearly double.

5. Newcastle, Cardiff, and coals of all descriptions, both double and single screened, and unscreened.

6. A day or two.

7. For long voyages, ventilation through deck and hatches, taking care that the squares of the hatchway are kept clear of coal, and not filled up so as to prevent the escape of gases.

8. For long voyages the coal should be shipped dry and as clear of brasses as possible. Lower deck hatches should be kept off, and the upper deck hatchways kept clear of coal, with ventilators through the decks and hatches to allow a current of air to pass over the top only and prevent the accumulation of gas; cocks to admit water in case of need. Coal ships are seldom properly dunnaged, and the same care is not taken (as with perishable cargoes) to keep the vessel always pumped dry, the water being thus sometimes allowed to get above the ceiling; this would be dangerous with small coal, which would be apt to heat.

From Messrs. Smith, Bilbrough, & Co., London.

1. No.

2. We build a wooden platform rising from 12 to 18 inches above the skin of the ship at the keelson, and 18 to 24 inches above the skin at the bilges, opening at the ends with two small compartments free of cargo, and communicating with two ventilators through the deck. We also have ventilating ladder shafts through each hatch.

4. From one third to one half above, according to the ship's carrying powers.

5. Principally steam coal from South Wales, Newcastle, and Liverpool; also from Newcastle, N.S.W.

We cannot name the collieries as we do not know them; the coals were sometimes double and sometimes single screened.

6. We have no means of telling.

7. We keep the hatches open whenever possible, and we provide all our ships with two fire-engines, one fixed, the other portable. Some of our larger ships have steam engines with which the fire hose can be connected.

8. We consider the principal causes of fire in coal-laden ships are—

1. The coal is frequently shipped in a wet state. The small coal, which retains the moisture longest, accumulates at the loading hatch. In then gets heated in time, and finally ignites.
2. The same thing happens unless the coal is raised by a platform well out of the reach of the bilge water.

We find that our wooden ships have taken about 45,000 tons of coals in the last four years. We have not carried any in our iron ships.

From Mr. George Adam, Greenock.

1. None.

3. Along the keelson and in the three hatches; and some of the iron ships have six ventilating mooring bitts on each side,

4. About one third more than the registered tonnage of the ship.

5. Principally Brymbo Welsh, Hartley steam coal shipped at Birkenhead, and a few cargoes Glasgow steam coal, but I do not know the name of the collieries.

6. Cannot say as the coals are shipped by the charterers

7. Keep the hatches off in fine weather during the voyage, and in carrying to India upwards of 40 cargoes never had any case of fire on board of any of our ships.

8. No suggestion to make further than we have been fortunate with our system of ventilation, which some parties now repudiate; at the same time would recommend that coals should not be shipped without being exposed to the air for a reasonable time after being dug from the pit.

From Messrs. Eyre, Evans, & Co., Liverpool.

1. No.

3. Two shafts fore and aft communicating with shafts up each hatchway, and hatches taken off in fine weather.

4. Generally one third over registered tonnage.

5. All kinds, Glasgow, Dundee, Tyne, Cardiff, Newport, Birkenhead, and Leith, Wishaw, Powell Duffryn, &c.

6. Latterly, say for two or three years, coals have come direct from pit to port of shipment, and have been more liable to heat.

7. Positive orders to masters to examine ventilation closely, seeing there is no heating in the hold, and that temperature in hold is not greater than on deck. We have fire engines and hose always on board, but never required to use them.

8. We believe if masters were particular and coals shipped in a fair dry condition, there would be little risk of fire from spontaneous combustion.

From Mr. W. Turner, Greenock.

1. Never.

3. Trunks in the hatchway to the keelson.

4. About tonnage and third.

5. Shipped coals at Newcastle, Greenock, Birkenhead, and Cardiff.

6. Cannot tell.

7. See No. 3, with this exception, if the masts are iron they also act as ventilators.

8. The last cargo shipped here on board new iron ship "Rossdhu" for Calcutta *were very wet*, and yet no spontaneous combustion or explosion occurred.

From Mr. Joseph Robinson, North Shields.

1. No.

3. None, excepting keeping the fore-and-aft hatches open until the ship gets to sea, and in fine weather when at sea, which allows the gas to pass off.

4. Register tonnage and three fifths.

5. Both gas and steam (see general remarks).

6. I think two or three days.

7. None, excepting as referred to in question 3 (as above). No provision against fire made in the sailing ships, steamers have the usual fire hose.

8. Yes; I would strongly recommend that all coals intended for long voyages, should be covered with tarpaulins in transit from the collieries to the ship, and should only be shipped in dry weather, and as a further precaution ships might be dunnaged. My ship "Gladiolus" has been three voyages to the River Plate, and in each case the cargo was on board about four months. Two of the voyages she had Nixon's steam navigation coal, Cardiff double screened, and on the other she had Scotch coal from Glasgow (no particulars of colliery).

The "Eleanor Grace" has just made a voyage out to River Plate, and has had her cargo in four months, and nothing wrong. It was Nixon's steam navigation and double-screened. She had very bad weather going out Our steamers are principally employed in Mediterranean, and sometimes to India. All the ventilation they have is, from the top of the cargo, the ventilators being fitted on the upper deck only.

From Messrs. J. and A. Roxburgh, Glasgow.

1. No.

3. In North American trade, and from Newcastle, New South Wales, to California or India, have ventilated the cargo only from the hatches. From Britain on long voyages have used moderately-sized ventilating trunks placed longitudinally, and vertically constructed, so as to admit the vapour arising from the coal. Sometimes vertical trunks only,

4. From 20 per cent. under to 50 per cent. over the net register tonnage, according to the build of the ship, the voyage, the season, and the prospect of a profitable return for carrying the coal.

5. From Clydesdale pits; Davison's West Hartley steam coal.

6. Generally fresh from the pit.

7. Ventilation as above, and instructions to captain to keep the hatches off in fine weather.

8. Ventilation as far as it provides for the escape of foul air is good, but we think it has been overdone in some instances, and that the current of air introduced into the hold may have been the immediate cause of ignition.

From Messrs. William and Alfred Brown & Co., Glasgow.

1. No.

3. In addition to the ordinary deck ventilation we have from three to five shafts, each about 2¼ feet square, latticed on two sides. These shafts run from the keelson to about 3 feet above the deck. A fore-and-aft current is also obtained by laying stout slabs along the keelson. Windsails are set on the forward or after shaft according to direction of wind.

4. From 20 to 50 per cent. in excess of net register tonnage.

5. Chiefly steam coal, double-screened when shipped at English or Welsh ports. Cannot say as to screening in Clyde. Cannot name collieries.

6. Generally understood to be fresh wrought.

7. Thermometric observations daily to ascertain whether heat is being generated in the hold (a copy of results recently received is attached hereto). Means for dealing with fire are confined to force pumps, of which in large ships we have three. In the event of heat increasing greatly in the hold, very frequent observations are to be made with the thermometers, and the master is instructed personally to descend the shafts and endeavour to ascertain the condition of the cargo. Should it appear probable that combustion has commenced or is likely to break out, the force-pumps are to be rigged and the coals deluged with water, the steam-winch keeping the ship free by means of the main and bilge pumps. Should this not check combustion, cargo is to be jettisoned so as to give buoyancy for a more thorough flooding of the hold, and the ship is (if possible) to bear up for a port and there be scuttled.

From Messrs. T. Davies & Co., Swansea.

1. No.

3. Upright clinker-built ventilators in two hatchways, sometimes horizontal under the beams.

4. From one third to one half above the register tonnage.

5. Bituminous coal for smelting purposes.

6. Generally fresh wrought.

7. Nothing except ventilation, but I attach no importance to ventilation.

8. If the coal is clean, free from dirt, sulphur, and iron pyrites, it will carry even if shipped in wet weather; if not, even if shipped in dry weather, it will probably heat.

From Messrs. J. Morrison and Son, North Shields.

1. No.

3. Hatches kept open frequently.

5. Steam coals double screened.

6. Probably two or three weeks.

7. Only adequate ventilation.

8. Yes. The present system of ventilation is bad; vessels should have high comming and main hatches taken off whenever weather permits. Always order my captains to take off the hatches whenever the weather permits. Had vessels to India and other distant trades, but never had fire on board any of them.

From Messrs. Thomas Skinner & Co.

1. Never.

3. We have a considerable number of iron ventilators from the deck, and in some cases have carried a wooden trunk along the bottom, connecting it with upright ventilators to the deck in others. We had no ventilation through the coals at all, and all went safe.

4. Register and one third.

5. Both Cardiff and West Hartley.

6. So far as we know only a few days.

7. None except as described in third paragraph. Fire engine.

8. None, as ships and steamers have just gone as safely that had none but deck ventilators as those ventilated through the whole cargo.

B. MASTERS.

Upwards of 100 of the following Paper of Questions were issued to MASTERS of Vessels employed in the Coal Trade.

QUESTIONS TO MASTERS.

1. Has spontaneous combustion, or explosion, ever occurred in any coal-laden vessel under your command?

2. If so, state all the circumstances of each case, as fully as you can, on one of the inclosed forms.

3. Do the owners, or others interested in the safety of the ship or cargo, generally give you any special directions for providing against spontaneous combustion or explosion, or for dealing with fire from these causes should it occur? If so, state them, noting any distinctions in them for fine or stormy weather.

4. In the absence of any instructions, what system of ventilation, if any, do you adopt in the holds of coal-laden vessels under your command? State at the same time the materials of which the ships to which you refer are constructed.

5. Is the ventilation constant, or, if not, at what intervals, and under what circumstances?

6. Is it customary to fill the hold completely up to the square of the hatchways, or to allow a space to remain between the surface of the cargo and the deck beams?

7. Do you test the temperature of the hold during the voyage, and, if so, how, and how often?

8. What means or appliances are usually at your command on board for testing temperature?

9. Does your experience lead you to the opinion that coal has a tendency to "heat" in the tropics?

10. What precautionary rules against explosion do you lay down as to the use of lights?

11. What kinds of coal have you generally carried? State whether small or lump, the names of the collieries, which seams, and whether double or single screened.

12. Are there any special kinds of coal exported that you have found to be more liable to spontaneous combustion than others? If so, describe them.

13. What do you gather to be the general impression among shipmasters as to the provisions made on board coal-carrying ships for the prevention of spontaneous combustion or explosion?

14. Have you any remarks to make, or suggestions to offer, to the Commission on the subject of their inquiry?

Please write "*Confidential*" against any of your answers that you desire to be so treated.

The following are some of the Replies received to these Questions.

From Captain P. T. van Overklift.

"Anna of Rotterdam." (*See* p. 93.)

1. Yes; "Anna," of Rotterdam, 969 tons register. My ship was twice in danger from spontaneous combustion of a coal cargo.

3. Nothing further than that I must ventilate the cargo as ordered by the surveyor for French Lloyds. I received a certificate that the cargo was properly ventilated.

4. Sometimes I have not ventilated, but lately I put wooden pipes in the hatchways; last voyage I had in addition wooden pipes along the keelson.

5. Always acting.

6. Lower hold completely filled except forward and aft. The 'tween decks were only partly filled, a space of two feet being left all over the coals up to the deck.

7. Yes, frequently during the voyage, sometimes twice a day.

8. By thermometer, but unfortunately it would only indicate as high as 110° Fahrenheit.

9. No, or but very slightly.

10. I use a miner's safety lamp, also wind sails to remove the gas emitted by the coal.

11. Always steam coals from Newcastle or Cardiff, once patent fuel from Cardiff.

12. I do not know.

13. Captains are most afraid of explosions, they have usually great faith in the efficacy of pipes in the cargo.

14. All depends on the quality of the coal. I would suggest that coal liable to ignite spontaneously should not be exported for long voyages.

From Captain James Finlay.

"Alfred of Belfast." (*See* p. 85.)

1. Neither has happened in any vessel I have commanded. Spontaneous combustion occurred on board a ship in which I was chief officer.

3. Messrs. I. and T. Sinclair of Belfast and W. P. Sinclair & Co. Liverpool, owners of the ships I have sailed in as master from 1852 to 1872, take care to have their ships well ventilated with air shafts through the coals whilst loading, and instruct the masters to keep the hatches open at all times during the voyage when safe to do so. Their ships generally took coals to the East Indies on the outward voyage, and there never was a case of spontaneous combustion known.

4. Their ships constructed some of wood and some of iron.

5. Constant unless in bad weather, when the hatches had to be put on, then we generally managed to have some deck ventilation open.

6. A space is generally left under the beams, no ship could carry the hold quite full of coals.

7. No, unless by personal feeling; I generally visited the hold once every day. Officers were often in the hold when on duty.

8. Common thermometer, of which there are generally one or two on board every ship.

9. I have found steam arise from the coal in the tropics, I think from the increased temperature of the atmosphere.

10. I had the lamps closed and locked before being taken into the hold, as a preventative against fire.

11. Both large and small mixed. Most of the cargoes I have taken from Birkenhead, Brymbo and Welsh Hartley coals; one cargo shipped there was brought from Merthyr, South Wales. I have also taken three large cargoes from Sunderland to the East Indies, all Londonderry West Hartley coal.

12. The Merthyr South Wales coals were the most *suspicious-looking* cargo I ever had from Birkenhead to Singapore, four months passage, in 1869, but kept cool all the way.

13. All I have consulted agree with me that if the coals are shipped in a dry state and ventilated on board, and the hatches kept open at sea, there is little danger of spontaneous combustion.

14. Captain W. Pirie, of 12, North John Street, Liverpool, who has been overlooker for the ships I have sailed, and others belonging to the same owners, could give information on this subject and the names of the collieries from whence the ships were loaded.

From Captain F. G. Fry, Liverpool.

'Admiral Lyons," "Glenlyon," "Hannah Law.", (*See* p. 86.)

1. Yes.

3. The owners of the ships I have commanded always gave particular instructions to give free ventilation to the cargo by keeping off the hatches when practicable, and during stormy weather to keep the shafts up each hatchway open. The free use of windsails and a careful register of the temperature of the hold by using the thermometer each day at noon, lowering it down the shafting of each hatch to determine the temperature of each portion of the cargo.

4. By keeping off all hatches during fine weather and keeping open all ventilation possible during stormy. Constructed of wood of the pine species.

5. Yes, when a shaft is used at each hatch and sufficiently high above the deck to be kept open during stormy weather, and fitted with a cowl on the top to keep out rain.

6. It is customary to fill the ship up to the deck in the middle section in order to keep the centre of gravity high up to make the ship easy, but after a few days at sea the cargo settles sufficient to make a clear space between cargo and deck beams. Hatchways are mostly filled up to the square when a ship has finished, but usually trimmed down afterwards for purpose of ventilation.

7. Yes; each day at noon down each shaft in the hatches.

8. Copper-framed thermometers protected by wire.

9. Yes; more especially in calm weather when there is but little motion, or wet weather when the hatches have to be kept on.

10. By using the safety lantern.

11. Howard's West Hartley, Westminster, Brymbo steam, Laird's Welsh Hartley, Powell, Duffyrn steam, Aberdare steam, all double screened and moderately lumpy.

12. Howard's West Hartley steam and Westminster Brymbo steam more dangerous if shipped newly wrought or wet with rain.

13. That they would like to have the means to admit a large body of water into the hold in case of fire or be provided with an apparatus to inject a gas into the hold to extinguish fire.

14. Yes, to advocate the adoption of a sea valve to admit water in such a volume that it would speedily flood the hold in case of fire, also the loading of coals by hand in lieu of the tips. The providing of every ship with a fire extinguisher and means to inject an extinguishing gas into the hold in sufficient quantity to permeate the whole cargo.

From Captain William Buchanan.

"Royal Family." (*See* p. 87.)

1. No; in one instance the coals became very much heated.

3. The owners gave me no special instructions, as they approved of my method, but usually enjoined caution.

4. I usually laid a strong platform, planks nailed roughly close together, fore and aft the hold to about 10 feet from each end, where I placed ventilators. Between fore and main masts I placed a jelesie ventilator athwart ship leading into lower air ports, with an upright ventilator of same description amidship leading up to main deck. The ships to which I refer were two and three decked ones constructed of wood.

5. Ventilation constant.

6. Always filled full up amidships and left any space in the ends.

7. About once a week tested by driving down iron rods in each hatchway.

8. Carried bars of iron for the purpose.

9. Yes.

10. Always used closed-in lights under deck (with any cargo).

11. Welsh steam coal. Names of colleries unknown, shipped from Cardiff and Birkenhead, single screened.

13. That the open-ended ventilators in hatches are not only much crushed in loading, but are, in the event of ship encountering heavy weather, channels for the entrance of water, which in my opinion is the primary cause of all spontaneous combusion of coals.

14. In large vessels I would recommend the open-ended platform; in small vessels iron bell-mouthed ventilators fixed on deck near the hatchways to carry off the foul air and gas.

From Captain G. Symons, Swansea.

"Serena." (*See* p. 89.)

1. Yes.

3. No special directions are given, but masters are left to their own discretion.

4. The cargo is separated in three or four compartments by platform and double longitudinal deals on their edges fore and aft each side of stanchions the whole length of hold.

5. Constant.

6. According to the weight of coal, in some cases the hold is chock full.

7. Yes, as often as possible in fine weather.

8. Thermometer.

9. Yes.

10. Carrying lights in lanterns.

11. Smelting coal, generally small unscreened.

12. The largest proportion of coal sent to Chili is smelting coal; out of 24 voyages to Chili I have carried smelting coal 23 times.

13. Shipmasters differ very much with regard to this question.

14. In my opinion smelting coal should be stowed in as many separate compartments as possible.

From Captain J. Galloway.

"Malabar." (*See* p. 89.)

1. Yes.

2. Have done so.

3. Nothing further than to see the ventilators carefully put in and to keep hatches off whenever weather would permit.

4. Have replied to this in statement as to the "Malabar."

5. Constant.

6. Necessarily there is a space between top of cargo and the deck beams, as no ship can carry herself full to the square of the hatchways.

7. On some voyages not at all, and on others daily.
8. An ordinary thermometer.
9. No.
10. None.
11. Generally called steam coal, both Scotch and English, the former Watsons; Hartley and latter.
12. Cannot specify as to this question.
13. Have never heard any captains complain of insufficient means or provisions, so far as known, being on board.
14. Never to allow coals to be loaded in a wet condition.

From Captain Wm. Kennedy.

"L'Agouhanna." (*See* p. 89.)

1. Yes; once.
3. None.
4. Lay ventilators alongside keelson on both sides, also down each hatch, each one entering into fore and aft ventilators in ship's bottom, each hatch ventilator being 2 feet above coomings; then 6 feet above keelson ventilators place two ventilators (16 inches square) fore and aft, the ends in peak clear of coal; 18 inches under beams place two ventilators right fore and aft, ends clear in the peak; thwartship ventilators into each of the fore and aft ventilators at each hatch entering into upright ventilators.
5. Constant.
6. The coal from fore part of after hatch to about 13 feet before fore part of main hatch, 2 feet from lower part of deck beams, tapering in each end to about 5 feet from lower part of deck beam.
7. Once a week, by a thermometer being lowered down upright ventilator.
8. Thermometer.
9. Find more heat in wake of hatchways in from 30° to 40° W., or ditto south, than in tropics.
10. None allowed below decks.
11. North Wales and Cardiff coals.
12. Cannot say.
13. Consider ventilation best preventative, but should fire occur I believe ventilators assist the burning; in 1872 loaded 1,900 tons coal in Glasgow, "Loconada;" no ventilators and no signs of heating. Consider coal should not be shipped during wet weather.

From Captain C. S. Caird, Greenock.

"Arracan." (*See* p. 92.)

1. Yes.
3. I have been left to my own judgment from my owners; always advised not to ventilate by the coal shippers in the Tyne.
4. "Arabia," three upright ventilators. The fore hatch one was burned 3 feet at foot when discharged at Bombay; no other fire. Coals very hot in the hatches among the small in several vessels. I have found this small coal to be almost unbearable discharging with and without ventilators.
6. It is advisable to fill a ship amidships up to the deck beams, leave all open space in the ends for to ease the vessel.
7. Only by going below and feeling above blood heat, especially forward, three times a week.
8. Thermometer.
9 Yes.
10. Never seen nor never used any.
11. Steam coal:—Tyne, Sunderland, Swansea, Cardiff, and Birkenhead loaded?
12. Can't say as all small coals are liable if damp even in bunkers and smiths' heaps.
13. Careless in loading and coals getting wet, trimmers not cautious in protecting ventilation and dividing coal big and small?
14. Have never seen any care taken to keep coal dry while loading in the north or Cardiff. Inspectors are much wanted to look after ventilation and stowage.

From Captain John Frostick.

"John," (*See* p. 93.)

1. Yes.
5. Constant.
8. None.
9. No.
11. Abbw coal.
12. Some of the South Wales smelting coal are more iable than others.
14. In my opinion coal cargoes when bound on long voyages ought to be shipped dry, and laid upon a platform, say 6 inches high, with an up and down ventilator at each end, which would cause a strong draught under the cargo.

From Captain John Ramsey, South Shields.

"Columbia." (*See* p. 85.)

1. Yes, on one occasion.
3. None.
4. I never ventilated any ship except the one named in the accompanying form. I have had command of both wood and iron ships.
6. The hold was always filled quite up to the square of the main hatch, but with generally a large space left at both ends of the ship.
7. No.
8. I have always had a thermometer on board which I could have used.
9. No, I do not think so.
10. I never allowed naked lights in the hold.
11. Have carried all descriptions of coals from the north country, large and small steam coals and small smithy coals, also Welsh steam coals.
12. No.
13. I think the general opinion among north country captains is that ships are safer when not ventilated.
14. None.

From Captain Henry F. Scott.

1. Three instances of dangerous heating which was effectually subdued by the method described in letter.
3. Never had any directions from owners to provide against the danger, but allowed the expense of ventilators in all but one instance by them; was *once* told to take hatches off occasionally.
4. Horizontal, wooden, square tubes on each side running from end to end of lower hold; well perforated at sides and bottom, and connected to perpendicular tubes from the corners of each hatchway, the latter projecting a foot through hatches, to be able to attach windsails to them to drive air down and through the ventilators.
5. Constant in all except in very stormy weather, when the windsails cannot be used.
6. Few vessels will carry the coals right up to the deck for a long voyage, the stores and water required being heavy. I have always had an average vacancy of about 2 feet under the upper deck when laden entirely with coal. Smaller class of vessels having their cabin and forecastle below the deck, will load nearly full to the deck, but will then be deep, depending much on the kind of coal, light or heavy description.
7. Have always had thermometers kept lowered down the perpendicular tubes and noted the temperature daily in the ship's log book; on finding heat increasing used long probe rods, which are easily forced down to the bottom.
8. As above.
9. Of this I have no doubt from continued testing, especially during long calms; when the vessel has little motion heat increases rapidly. I have paid much attention to this particular effect.
10. Rarely any, when ventilated and the hatches kept off as much as possible. It is nearly impossible to prevent taking lights below or striking matches by seamen.
11. Both large and smithy coal, mostly the former, have few records of names of the collieries; have had both screened, double and single, and unscreened; the latter appear most liable to heat, particularly under the hatches where the dust forms a heap which cannot be ventilated.
12. In my cases "West Hartley" heated quickly, also "Cowpen"; have no record of others which I have carried as to name.
14. Respectfully beg to refer to letters on the subject.

12, Grove Street, Newcastle-on-Tyne,
May 1st, 1875.

Sir,

Actuated by a sincere wish to assist in lessening the chances of calamity by fire at sea, I respectfully address the following observations to you on spontaneous combustion in coal cargoes, and chiefly to submit to your consideration a simple and efficacious method of preventing ignition, and which has in three instances been perfectly successful in ships under my command, the result showing it to be a successful experiment reduced to practice and submitting to your approval whether it might be of sufficient importance to be communicated to the Royal Commission about to investigate the subject referred to. After a few remarks on cause I will describe the method referred to, which for simplicity of construction and ease in carrying out renders it available to all, if disseminated among ship masters and others who may be interested.

From a careful observation I am of opinion that there are four causes. First, from coals which give out gas without the aid of heat, even in a low temperature and which is *greatly assisted* by attrition in shooting them down the hatchways, and those in a vessel with closed hatches frequently explode before leaving port when a light is taken below.

Second.—From coal which does not emit gas at first, but after having been a month or two closed up in a ship's hold in tropical waters a gas is generated in them assisting an increase of heat and which unexpectedly culminates in an explosion (as in the case of the "Belcarreg" and other ships).

Third.—From small, dusty, or smithy coal, lying so compact that it is difficult to ventilate it.

Fourth.—From coals which are shipped in a wet state, say in rainy weather, and if unscreened and small are certain to heat in a short time. In this last description heat increases rapidly towards the last, of which I lately had an unmistakable proof.

In 1852 I took a cargo of coal from Sunderland to Madras without ventilators. After passing the Cape, not having been able to take off hatches for a considerable time owing to bad weather, were alarmed at finding smoke issuing from the cargo, with a strong smell of incipient gas. In this dilemma a panic came over my crew, and they insisted on getting the boats out and preparing to leave the ship. Now as danger and necessity often sharpens the wits, after looking at things in all its bearings I determined to try and save ship and life likewise. After a little trouble in getting my men to second me in my plans we went to work.

Happening to have on board some rod iron I roughly pointed a few of the bars and drove them down into the cargo in different directions, allowing them to remain awhile we drew them out, feeling them all their length with the hand, marking the hottest parts with chalk. By driving them down in all directions from the main hatchway I found that the heat was all in one place, forming a nucleus of about 10 feet in the lower hold and directly under the hatchway, the bars driven diagonally through the coal coming out quite cool. I then got four studding-sail booms roughly squared, and knocking down bulkheads and part of bulwarks to procure boards, we made a trunk about 16 feet long, 2½ feet square at the upper end and 3½ feet at the lower, in form of a square *cone*. We then dug down as far as we could, and placed the *large end* of the cone in the hole with a man inside, who dug out the coal from under it, driving the cone down from the top as the hot coals were sent up in buckets. After getting down about 7 feet we came upon the coal so hot that the man at the bottom could only remain a short time, and had to be replaced at short intervals owing to the great heat and smoke and putting his feet into wooden boxes which we made to save from burning them. We now found that as fast as the coal was cleared from the bottom, the trunk crept down *without forcing*, owing to its conical shape and the rolling of the vessel. In six hours we had it down to the keelson, and by removing each alternate board from the side of the trunk at the hottest part we grubbed out great part of the coal which was smoking, and would not have been long before taking fire. I then rigged a topmast studding sail as a wind-sail, tying the foot of it round the top of the cone, and thus forcing a strong current of air down to the bottom and through the sides where the boards were removed, and in about 24 hours, on going down our pit, we had the inexpressible pleasure of finding the temperature little more than milk warm, and by continuing the use of the windsail all heat and smoke disappeared, and ignition was prevented just in time.

On two subsequent occasions I have repeated the process, and always found it successful. I have invariably found the heating to commence right under the hatchways, but chiefly the main hatchway, the largest body lying there, and this I account for thus: The coals as they are poured down from the spout or bottoms of waggons perpendicularly, form an increasing heap of small and dust coal, as the large coal roll away down the sides of the pyramid thus formed, leaving a mass of small coal imbedded in dust in the centre of the hold, the large coal as they are trimmed to other parts ventilating themselves, while the heat is invariably generated in the undisturbed pyramid in the centre, and which will gradually extend to the surrounding parts if not prevented in time by the process above described. I may add that I have had to cut the cone into several parts, joining one on to the other below the deck, when the state of the weather and sea obliged us to keep hatches battened down, so that we found no difficulty in getting a cone of 17 feet down, even with closed hatches, and thus saved ship and cargo.

I will now state a case which may be said to come under cause second.

In 1858 I took a cargo of coal out to Melbourne for gasworks, properly ventilated on my own plans, frequently attaching wind-sails to the upright tubes and forcing air all through the cargo. The barque "Lizzie Scott" sailed with me at the same time, and with the same kind of coal for Melbourne also, but *without ventilators*. My cargo heated a little, but I kept it down with the help of the wind-sails, and discharged in good order. The "Lizzie Scott" arrived at Melbourne apparently all right, but on discharging they found to their surprise that the cargo got quite hot as they worked down, and that the two hold beams of the part nearest the main hatch were burnt away, all except the ends. On getting further down they found nothing but coke, and that the keelson to the extent of about 20 feet had entirely disappeared, leaving nothing but the bolts sticking up, and that a corresponding area of the bottom ceiling was burnt away and charred an inch or so into the floor timbers. All this I saw myself; but the strangest thing was that there was scarcely any smoke, and before breaking bulk on arrival, they had seen no sign of smoke in any part of the vessel and never suspected fire, so completely had it consumed its own smoke. I attribute this case to the want of ventilators.

By the use of wind-sails attached to the upright tubes of ventilators I have frequently reduced the heat 15 and 30 degrees in as many hours in a stiff breeze, always keeping a couple of small thermometers lowered down the tubes, registering the increase or decrease of heat daily in the log. After heat has attained to a certain point the large coal crumble to fragments, accelerating the escape of gas as it is evolved by the increasing heat, thus augmenting the danger of explosion, and when it is discovered that the coal in the lower part of the cargo has become heated to a dangerous point it is useless to deluge it with water, the effect being only to form an impenetrable crust over the heated part, through which the water cannot descend, but finds its way down the sides where it is useless for the purpose.

In conclusion I may say the most important points are to use the probe rods frequently, and then if danger threatens not to put off too long, but use the means to subdue the heat before it gets beyond the power of control, and this I affirm can be done in most cases by adopting the foregoing method. The materials required cost but little, and to condense the whole into a few words, let a ship about to proceed on a long voyage with coal be provided with a few ¾-inch rods of iron, a moderate quantity of rough inch boards, some extra nails and (to save studding-sail booms) a few poles with those, and a knowledge of the construction of the cone, which is very simple, the watchful commander is prepared to checkmate a merciless enemy, insidious at first, but terrible at last.

I am, &c.
(Signed) Henry F. Scott.

R. B. Peverley, Esq., Secretary,
Chamber of Commerce, South Shields.

12, Grove Street, Newcastle-on-Tyne,
28th May 1875.

Sir,

I beg to return the circular of questions relating to spontaneous combustion in coal-laden ships which I have had the honour to receive.

As the questions therein appear chiefly to apply to cases of *actual fire* I am not in a position to attach the answers in direct form, owing to never having had a vessel under my command actually on fire, and having in my cases of dangerous heating always succeeded in preventing ignition in time.

My exertions have been directed chiefly to the means of prevention and subduing of heating before it got beyond control, for if allowed to heat to a certain degree explosion or combustion is certain to ensue if means are not used to stop or check it in time, and this I have proved can be effected with comparative ease by the method which I have in three instances adopted and which in each case succeeded to my utmost wish. I lately addressed a letter to the Secretary of the South Shields Chamber of Commerce describing the method, also describing how effectual it proved, and I am informed by that gentleman that he had kindly forwarded the letter to you, and to which I beg respectfully to refer you for description.

Taking a deep interest in the subject, I beg with deference to submit to the consideration of the Royal Commission the method referred to.

In the letter above-mentioned I described my first case, and now will as briefly as possible give the other two.

In June 1862 I sailed from Sunderland in command of the ship "Areta," wooden ship, of 678 tons register, laden with 700 tons of "Hartley's" large coal, single screened, and about 200 tons glass on the top of the coal, bound to Shanghai, the coal having wooden ventilators. After being three months at sea, having previously found by the thermometers in the tubes that the coal was heating, I found by the probe rods that the heat under the hatchways in the lower hold (where it invariably generates) that fire was imminent before I could reach port (I was then entering the Pacific). Being prepared with materials I lost no time in making the cones, as described in the letter referred to, and sunk them down to the bottom of the hold, bringing up the coal dangerously hot, the thermometer buried in them showing 180°, and the heat had been increasing rapidly for the few days previous. I put a cone down all three hatchways where I found the greatest heat, and by attaching large windsails to the mouths of them the heat was reduced so quickly that in a few days it subsided to 100°. On arriving at Shanghai it had receded to 90°, but in the bottom round the cones I found on discharging the coal had the appearance of semi-melted bitumen, showing it had been subjected to great heat. Those coals were partly shipped in a wet state, and although they were pierced fore and aft with ventilating tubes I am convinced that actual ignition would have taken place in a short time if the means described had not been resorted to. * * *

"Touching explosions, I am convinced that if the hatches of a coal-laden ship are kept off whenever the weather permits, and if the ship be provided with properly arranged ventilators on a principle I would be glad to explain (and they are anything but costly), such disasters would rarely occur. Hatches can generally be kept off night and day in the trade winds, and the ventilation be made *continuous* the whole way through the longest stage of the voyage and where the heating is most to be apprehended, for undoubtedly it is between Cancer and Capricorn that the greatest danger of heating and emission of inflammable gas exists, but coals shipped wet are the greatest danger of all.

"In all cases of heating under my notice I have found that *with the exception of the coal being wet all through* the first indication was found immediately under the hatchways, particularly the main one where the greatest body is, and from experience have the greatest confidence in the efficacy of the cone method in preventing actual combustion from taking place. Moreover, the extreme simplicity of the method, and the ease with which it can be carried out, renders it available to those who may be in peril of conflagration in a part of the ocean where there is no escape except the doubtful one of boats.

In conclusion, I have some misgivings that addressing you to such length may be deemed informal, and not in accordance with the intention of the Royal Commission to elicit information on the subject, but as the questions in the form appear to apply only to cases of actual fire I have ventured to submit the foregoing experiences in the present form, confining my remarks to *prevention* of actual ignition, and actuated by a sincere desire to contribute towards lessening the number of terrible casualties of which the writer has for many years been in dread.

I am, &c.

J. M. Carmichael, Esq., Secretary. HENRY F. SCOTT.

From Captain A. PEARE, Swansea.

'Henry Bath,' "Alpha." (*See* p. 90.)

1. Coals have heated and cooled again on the voyage.

3. Instructed to give all possible ventilation by keeping the booby hatch off at all times the weather will permit.

4. By running planks fore and aft on each side of the stanchions from keelson to the deck. Ships all of wood.

5. When hatches are closed, the iron ventilators through the decks are sufficient.

6. The hold is generally filled up at the main hatchway, but settle down after being a short time at sea.

7. By lowering a thermometer occasionally down to the keelson, and by running a long rod of iron as far as possible into the cargo.

8. Thermometer.

9. Yes.

10. None required with smelting coals.

11. Smelting coal, which is oftener small than otherwise. Broadoak, Techan, Mynydd y Bach.

13. Masters appear quite satisfied with our system of ventilation or separation.

14. That shippers should be particular about the class of coal they ship for long voyages, and colliery proprietors should ship clean coal. From the year 1860 to 1867 I was in command of several ships carrying coal from Swansea to West Coast of South America. I adopted the system of separation by running planks fore and aft the ship upon each side of the stanchions, from the keelson to the upper deck beams, and never had any case of spontaneous combustion or explosion, and from 1867 up to the present time I have superintended the loading of at least 150 ships with coal, principally smelting coal, and have always adopted the same system of separation, and have only had the two cases of spontaneous combustion mentioned in the paper. The "Henry Bath" put into Port Stanley and discharged part of her cargo. The coals were much heated, but no fire. They turned the coals over in the hold, and took the remainder they had discharged in again and proceeded on their voyage. The "Alpha" put back to Port Stanley and discharged part, and found the inside planking and several of the floors charred and burnt; she was partially repaired and took in her coals again and proceeded on her voyage. I attribute the heating of the coal to its being very small and very wet when shipped. I adopt the same system of separation with steam coal, and find it answer well, and vessels having iron ventilators through their decks will always be clear of gas and very little fear of explosion. I think the great number of casualties to ships loaded with coal in the last two years may be attributed to the high price of coal, and a great many of the coal proprietors have not been particular about the quality of the coal shipped; and if shippers and colliery proprietors only act conscientiously in regard to the coal they ship, and ordinary care used in the loading and ventilation, coal ought to be a safe cargo. I have always found the under-mentioned coal quite safe with my system of ventilation or separation. At Swansea the Broadoak, Techan, Mynydd y Bach,—the "Alpha" and "Henry Bath" excepted,—Rhyding; Nixons and Powell Duffryns (steam); at Shields the Tanfield Moor; at Liverpool the Ornell. I consider the system of putting a ventilator at each hatchway when loaded with smelting coal of much more harm than use, with steam coal it may be of some use, but as a rule they are either broken or filled with coal before the ship is loaded; the separation with boards or planks is expensive, but safest.

From CAPTAIN of the "NORTHBROOK," forwarded by Messrs. Adamson & Ronaldson.

'Northbrook." (*See* p. 96.)

1. No, except as stated in enclosure.

3. It is not usual for me to receive any special instructions except from Captain Muscatt (ships husband).

4. Establish a free current of air through the hold. Iron wood.

5. Constant, except in cases of very bad weather.

6. No, spaces are always left for a free current of air.

7. Occasionally.

8. Thermometers.

9. No.

10. No naked lights below, and all lights extinguished at 9 p.m. except those absolutely necessary.

11. Not known.

12. I cannot tell.

13. I don't know.

14. Nothing more than a complete system of ventilation.

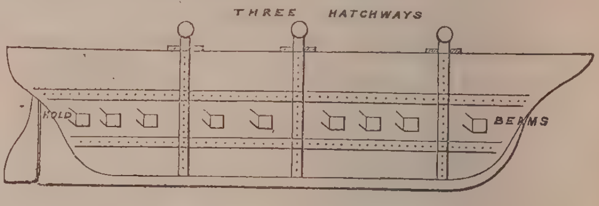
From Captain WM. MURCH, Newcastle-on-Tyne.

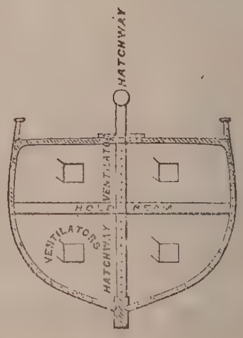
1. Spontaneous combustion has taken place on board of a steamer I was chief officer in, by the overheating of the coal in bunker (Midridge coal), shipped at Hartlepool; it was caused by the coal laying against the heated bulkhead of engine room.

2. There was no damage done in this instance as it was in deck bunker, and with a copius supply of water was soon extinguished,

* * * * *

4. The following diagram will show the system I would adopt either in wood or iron ships:—





This shows four trunkways fore and aft the hold, say 8 inches square, perforated with auger holes at every 18 inches, connected with a trunkway in each hatchway, the same protruding 6 feet above the hatch to keep the water out in bad weather. This was adopted on the last voyage I was out to India and found it answer well.

5. The ventilation would be constant because the tops of ventilators in hatchways could be fitted with their heads to slew to leeward, by that means preventing any seas breaking into them, and by that means keeping up a constant circulation of air through the coal. The present system generally used is to take off the hatches in fine weather, which only allows the surface heat and gas to escape, and I have been a fortnight rounding the Cape of Good Hope in winter when it would not have been prudent to have taken a hatch off.

6. It is customary to fill up square with the hatchway, but there is generally a space below the deck, the height of the deck beams.

7. I never tested the heat of a ship's hold, although in the tropics when the hatches have been taken off it was found like a moderately-heated oven, more especially in iron ships with rice cargoes, as such sweats tremendous, therefore the same system of ventilation applys to them.

8. I have seldom seen a thermometer on board of ships, except those connected with barometer.

9. I have great confidence in saying that coals has a great tendency to heat in the tropics. Some years ago I was in Havana, where I saw some hundreds of tons of coal laying in a heap 20 feet deep take fire by getting wet and being exposed to the sun's rays, although exposed to the open air.

10. Where there is any suspicion of gas don't use any lights whatever below.

11. Both steam and gas, mostly steam.

12. Most kinds of coal are liable to spontaneous combustion if shipped in a wet state and destined for a long voyage.

13. I have spoken to many shipmasters upon the subject of spontaneous combustion, and found many to agree with me that coal ships on long passages are like coal mines, and ought to be treated the same in respect to ventilation, and I may say the same applys to all ships loaded with inflammable material.

14. Remarks. 1. All sailing ships bound south of the equator or continent of America or West Indies to be ventilated through the coal as proposed. 2. All coal coming from the collieries to be brought in covered waggons to ships. 3. When not teaming coal on board ship, hatches to be kept on, preventing any water getting into coal. I think if those simple, yet efficient, means were used, and have a proper authority to see it was done, we would seldom hear of such lamentable cases as the ships "Euxine" and "Arracan," and I have no hesitation in saying many more that have never been again heard of, besides saving much capital.

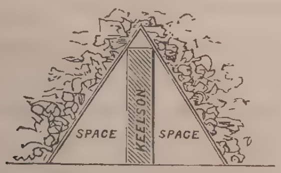
From Captain James Muir, Liverpool.

1. No.

2. Never occurred with me.

3. Yes; by ventilation as a preventative. No; as regards steps to be taken in case of fire.

4. Usual method of ventilation adopted by me, and approved by owners, as follows:—Fore and aft the keelson on each side by means of wood laid from top of keelson to the floor, angled so as to give good air space and view. In addition to this an upright shaft in each hatch from floor to upper part of hatch combings, with sufficient span at bottom to cover ventilation of keelson. Fore and after hatches kept off whenever weather admits. Wooden ships, colonial build.



5. Constant but sluggish in calm weather and smooth sea.

6. Fore and after hatches generally clear. Main hatch often filled up to top of combings. There ought to be a free air space fore and aft under main deck.

7. Yes, by thermometer lowered down the ventilation shaft. Twice a day.

8. Every ship coal laden should have thermometers on board and the captain be instructed to use them.

9. No, I found in very hot weather, that the temperature of the hold at foot of shaft was lower than the temperature in the shade on deck.

10. None. Except for the first few days after the ship was laden. Hatches should not be put on immediately.

11. Steam and gas, generally lump, cannot name colliery or seam, steam coal usually double screened, have carried in all 14 cargoes. Cardiff, Tyne, and Liverpool.

12. I have been informed by steathmen in Cardiff and the Tyne that some classes of coal are more dangerous than others, but cannot say from own experience.

13. The majority are in favour of ample ventilation, and many think with myself that sufficient care is not taken in ventilating the ships or in keeping off the hatches. Cases have been mentioned to me where the hatches have not been removed for the entire passage.

From Captain Cloake, Cardiff.

1. On two occasions an explosion of gas occurred, caused on both occasions by engineers taking naked lights in the after compartment.

3. Many owners give instructions to attend to ventilation of hold by opening hatchways when weather allows such to be done, also by putting ventilators down in holds. So far as my own instructions went I had no special orders, and my experience was greater than my owner's, and he would not have objected to any means being taken to ensure safety.

4. I had 25 years' experience in command, 12 years of that time in steam, and I on all occasions when opportunity occured opened the hatchways, two if possible, one for down and other for upcast. I have heard of instances of air being forced by pumps into the hold, thus causing circulation. Above explosions were in steamers built of iron.

5. In steamers I commanded, being for the fruit trade, we had proper ventilators fitted; in this case the ventilation was pretty constant, but where only hatches were available it depended on the weather and also to attention of the officers in causing the hatches to be opened.

6. With Welsh coal the holds are very rarely full, being often much space left, especially at ends of the ship; the bodies of the hold or in the neighbourhood of the hatchways are often filled up to deck, but the large Welsh coal generally leaves vacancies near the deck. One great stoppage of ventilation is caused by trimmers to save labour having at finish of loading to cram the hatchways very full, filling up to top of combings, packing same preventing gases escaping freely.

9. Coal in my opinion has a greater tendency to heat in the tropics than in the cooler latitudes.

10. For going into holds I have always carried safety lamps, also for going into funnel of steamers and watertight compartment; at same time open lights are mostly used in the cabins and forecastles.

11. I have loaded all kinds of coal, mostly Welsh coal, at times the coal has been double sometimes single screened, and also carried small coal as cargo; both explosions occurred with large Welsh coal.

12. We have some coals, both in the north and Wales containing much more gas than others; but regarding spontaneous combustion, in my opinion this depends on the state the coal is shipped in, and also whether it is comprised of much small coal in the cargo; again, some Welsh coal after being wet and then shipped falls all to dust, and this would in my opinion foster heat.

From Captain Beldrum, Greenock.

1. No.

3. None but to see the ships ventilated in the usual manner.

4. An air shaft running fore and aft each side of the keelson; second, another running same way half way up in the lower hold communicating with the air shafts down each hatch. Both wood and iron.

5. Constant at all times, but freer in stormy weather.

6. Coals should not touch the upper deck and a passage left round the wings 'twixt decks and lower hold.

7. Have never done so.

8. None but the common thermometer.

9. I am of opinion that it does.

10. Never permitting any below.

11. Cardiff steam coal, coal shipped from Birkenhead to Bombay and from Clyde to Bombay.

12. Am not prepared to answer this question, but fancy those from the Tyne the least safe.

13. Insufficient ventilation and putting on the hatches too soon after being loaded.

14. I am also of opinion that were the precautions generally adopted that I have stated in answer to paragraph 4 and the cargo lowered into the hold by hand instead of from "tips" or "large steam cranes" as is usually the case, fewer losses would occur from fire. I would also add that no ship intended to go round either Cape of Good Hope or the Horn ought to be allowed to take in coals otherwise than by basket or wheelbarrow lowered into the hold; when from tips or steam cranes they lay in solid mass under the main hatch, where fire usually first breaks out, the ventilation either partially or altogether destroyed, just sufficient to fan the flame but not enough to allow the foul gases to escape. In my opinion ships would be safer without any ventilation than it in an imperfect state.

From Captain JOHN GIBBS, Birkenhead.

1. No.

3. Only to see carefully to the proper ventilating of the cargo during the course of loading, and this is effected by laying ventilators fore and aft along each side of the keelson, and the same on each side of the lower hold, 5 feet under the beams, all carried out to the ends of the ship; these are all connected with large up and down ventilators in all the hatchways. I have commanded colonial-built vessels of birch, herekmatack, pitch pine.

5. The ventilation is constant.

6. The main hatch is frequently so filled, but should not be.

7. Occasionally by means of a thermometer suspended in hatch ventilator.

8. Thermometers.

9. Becalmed, and more particularly during rains. I am of that opinion it does.

10. That none are used, except in safety lanterns.

11. The North Wales steam coal, as it is produced from the collieries, mostly Bryn Mally and Tron, Brymbo, Broughton.

12. In my experience none more liable than others, but undoubtedly there are some.

13. That the foregoing amount of ventilation generally introduced on board coal-laden ships, if fairly distributed about the cargo, may be looked upon as the greatest safeguard against spontaneous combustion or explosion, but this must be followed up by the constant attention of those on board to the keeping open of the hatches on every available occasion, at the same time keeping the coal dry. These are precautions I have had rigidly carried out during my 13 voyages with coal, and one with patent fuel to India, and in not one instance has the cargo been overheated.

From Captain W. RANKIN, Greenock.

No.

4. Do nothing further than what was provided before the ship sailed, and the ships I commanded were principally wood.

5. Ventilating by the trunk was regular and found satisfactory enough.

6. Surrounding the main and after hatchways the coals have generally been packed close, but there have always been blank spaces in the hold forward and aft.

7. Not usually done by me, but think it highly desirable that it should be so, seeing that thermometers could easily be lowered down the ventilators.

8. Thermometers are generally provided for ships use.

9. It never occurred to me that the tropics have any such effect.

10. Not to admit any light into the hold unless well screened by a lantern.

11. I have no further particulars to give as to the character of the coals carried further than stated in the replies by me as an owner.

12. I believe there are coals more subject to explosion than others, but not being sufficiently acquainted with their characteristics I am unable to give further information.

13. I have never heard any better suggestion made than ventilating by trunks.

14. I may mention that in my early days of sailing in coal-laden ships spontaneous combustion was not thought of, and no provision whatever was made to avoid it.

From Captain H. VELRUNT.

1. Spontaneous combustion took place in the bunkers of a steam transport I was in during the Crimean war.

2. I think it was patent fuel from Swansea, and it ignited on two occasions, steaming for Malta and after arrival.

4. Keep the hatches off where possible. Wood and iron.

6. There should be spaces left at each corner of the hatch to allow the air and gases to escape or a man to get in.

8. Common thermometers.

9. Yes.

12. Salved cargoes of coal or fuel having been under water should not be shipped for long voyages.

13. Not good.

14. Coals should be covered in the waggons if intended for long voyages as a protection from heavy rain. Candles should not be allowed for trimming as they are allowed to lie about and form liquid grease when heated.

C.—STEATHMEN, COAL SHIPPERS, AND OTHERS.

ABOUT 100 OF THE FOLLOWING PAPERS OF QUESTIONS WERE CIRCULATED AMONG STEATHMEN, COAL SHIPPERS, AND OTHERS.

QUESTIONS TO STEATHMEN, COAL SHIPPERS, AND OTHERS IN CHARGE OF COAL BEFORE SHIPMENT.

1. What time usually elapses before coal newly raised from the mine is shipped for exportation? And has there been any change in this respect of late years?

2. Are any precautions taken to shelter coal during wet weather while it is waiting shipment? Does it often occur that it is left in open railway trucks for days together?

3. Is it, or is it not, the custom to ship coal in all weathers?

4. Can you suggest any measure by which coal could be better protected from the weather before shipment without serious inconvenience or expense?

5. State the heights at which the coal usually falls from the wagon into the ship's hold?

Please write "Confidential" against any of your answers which you desire to be so treated.

The following are some of the REPLIES received to these QUESTIONS.

From Messrs. A. FINNIE AND SON, Kilmarnock.

1. Generally shipped same day as raised or following morning.

2. None—seldom for days together.

3. Yes, in all weathers unless prevented by storms.

4. No.

5. From 2 or 3 feet when coal is up to hatchway, to 18 or 20 feet when hold is empty at commencement of loading.

From Messrs. HUGHES AND NEPHEW, Liverpool.

1. Almost all coal from North Wales comes from some distance. Supposing 100 tons are raised from the pit during the day (as at our Cœd Talon Colliery), they are screened, loaded up, and forwarded before the evening, and arrive at Birkenhead in the course of the following day, and are sometimes shipped at once, sometimes not for days.

2. Coal is never conveyed in covered waggons or sheltered during wet weather. It often occurs that coal is left for days together, sometimes for three or four weeeks, in open trucks at Birkenhead before it can be shipped.

3. Coal is shipped in all weathers.

4. Coal might be protected from the weather by tarpaulin coverings, but this would cause inconvenience and expense. To provide other shelter at Birkenhead, say by erecting sheds, would be almost impracticable owing to the immense traffic.

5. As we never load our vessels under the tips, but only by hand barrows, we cannot say from what height coal usually falls from the waggon into the ship's hold.

From Mr. E. C. DOWNING, Cardiff.

1. This varies exceedingly. When the demand is continuously brisk, 12 or 16 hours would be the average time from the mine to the ship's hold; sometimes a delay of many days occurs. In consequence of the greater regularity

of business by increase of steamers, the tendency of late years has been to reduce the interval between the time of working the coal and the shipment.

2. None whatever, except in the case of gas coal, which by some firms is protected by tarpaulins placed over the waggons. The coal is often left in open railway trucks for many days together.

3. It is the custom to ship coal in all weathers by night as well as by day.

4. I think that coal liable to ignite spontaneously, when intended for shipment to distant parts (say where the voyage exceeds three weeks or a month) should be protected from the wet by tarpaulins over the waggons. This precaution should be enforced by some legal enactment, the expense would be trifling. Cardiff steam coal proper will not ignite spontaneously.

5. The mode adopted for shipping coal at Cardiff is this. The coal is emptied from the waggon into a long shoot extending from the tip to the hatchway of the ship. At first the coal is lowered into the hold in a box (arranged with a counterpoise) until a large heap is formed. When the box cannot longer be used the coal falls from the point of the shoot into the hold on the coal, a distance of five or six feet, until the hatchway is filled after which there should be no distance for the coal to fall.

From Messrs. John Pirie & Co., London.

1. This depends on the state of trade, when demand is good the coal is put on board with the least possible delay, often in a few hours. When trade is bad, it has sometimes to wait a market on the bank or in trucks. There has been no change of late years in this respect.

2. No.

Yes, in wet weather this must be so, as there are no sheds at collieries for loaded waggons, and they are exposed during the journey from colliery to place of shipment and there till shipped.

3. Often in wet weather.

4. Only by having trucks covered which is scarcely practicable.

5. Depends where and how shipped, by shoot or drop; a heap is generally allowed to acumulate under hatch so that coal slides down sides of pyramid instead of falling into hold.

From Messrs. Merry and Cuninghame (Limited), Glasgow.

1. From two to ten days; average four to five days. It very seldom occurs that coal has to stand ten days in trucks previous to shipment. The time it has to stand depends very much on the state of trade; when there is a good demand for coal it is not kept standing in trucks very long. Matters in this respect have been much the same for a number of years.

2. No precautions are taken, and it often happens that coal is left open in trucks for several days waiting shipment.

3. It is the custom to ship coal in all weathers.

4. Coal waiting shipment could not be protected from the weather without incurring great expense for erecting sheds, as it has to stand at the pits longer than at the shipping ports because the railway company will not take coal forward until the vessel can get into a crane berth.

5. From three to six feet to deck of vessel, according to size of vessel and state of the tide at time of shipment.

From Messrs. W. S. Partridge & Co., London.

1. We believe districts differ in their mode of acting on this point. At present and for some years very little in North Wales has been put on the bank for shipment. The rule is to put newly-raised coal direct into waggons, and send the waggons by first train to the ship.

2. Not as a rule; they may occasionally get sheltered. Sometimes.

3. Where tips are used it is seldom that they are allowed to stand idle at Birkenhead, where we load a large quantity by hand-barrow; the men won't work in very wet weather.

4. We do not think it possible, as the expense would be too heavy.

5. The height various from two to 25 feet, depending on depth of vessel's hold; but if captain or owner will lower carefully about 100 or 150 tons, a mound will be formed under main hatch, and the fall of the coal will be five to seven or eight feet. The longer drop would not be for more than the first 40 tons, as the mound would be rising. Captains do not like the extra expense of lowering.

From Mr. John Traill, Cardiff.

1. When trade is regular, and the supply of shipping sufficient, much South Wales coal is shipped in 24 to 36 hours after being raised. No work is done during the 24 hours of Sunday. The increase in the number of steamships which require quick despatch, improved shipping facilities, and the extension of railway transport, have much tended to hasten shipment of late years.

2. None, except rarely in the case of small consignments of gas coal. It does often occur; sometimes it is left for weeks. All our trucks are open.

3. It is.

4. I think I ought here to premise that many kinds of South Wales coal and all our good steam coals are not liable to spontaneous combustion. With regard to the descriptions so liable, I believe that only a small proportion is sent to distant ports, most of it going for home consumption. Though it would be very desirable that such coal should be protected from rain by tarpaulins, when intended to remain say a month in a vessel's hold. This measure would entail considerable if not serious inconvenience and expense.

5. The mode of shipment usually adopted here prevents the coal falling any great distance; the trucks are brought to the upper extremity of a "shoot," set at an angle of about 40° with the horizon, and of which the lower end reaches to the hatchway. Into this the coal is "tipped," and may if necessary be "screened," or sifted while passing down the shoot, which is furnished with bars for the purpose. The coals then run into a large box with a hinged bottom, by means of which they are lowered into the hold until a heap reaching to within a few feet of the hatchway is formed, when loading proceeds without the box.

From Mr. Joseph Fothergill, Agent to the Cowpen Hartley and Strakers West Hartley Coal Company, Newcastle-on-Tyne.

1. Five to six hours there has been no change.

2. None. No.

3. It is the custom to ship coals in all weathers to keep the mines in regular work.

4. No.

5. The coals fall from the waggons into spouts and thence run into the ship's hold, the end of the spout generally resting on the combings, therefore the fall is to the bottom of the ship until she is run up with cargo.

From Messrs. James Smith & Son, Glasgow.

1. Usually not more than 10 days, no change of lat years.

2. No precautions are taken. It does.

3. It is the custom to ship coal regardless of the weather.

4. We know of no means except that railway companies might provide covered sheds for coal intended to be shipped long voyages, and remove such coal only during dry weather.

5. About five feet above combing of ship's hold. We may add, during some 15 years our firm have shipped a great many cargoes of coal, in no case have we ever heard that combustion has taken place except in one case a ship named the "Workington" loaded by us in June 1874, with 1,640 tons of splint coal was we understand burnt off the River Plate. Her cargo was shipped in very wet weather, and to this cause we ascribe the burning.

From Mr. Rd. Bittaby, Cardiff.

1. Coal is generally sent as fast as it is raised, and is usually tipped within 24 hours; there has been no change in this respect within my knowledge.

2. None. Not as a rule.

3. Yes.

4. As nearly all the coal sent down for shipment rarely stands for any length of time, I do not see that any protection is at all necessary.

5. The drop from the point of the shoot into the hold after the box has been used would be about four feet.

From Mr. John Macdinan, Glasgow.

1. About 48 hours. No change.

2. None. The coals frequently get wet in transit from colliery to port of shipment, and I have known instances of trucks standing loaded for days by ship not being ready

or vessels being in time to load at crane before the ship for which the coals are intended.

3. Customary to ship in all weathers.

4. Only by tarpaulins, but this would be expensive, and tarpaulins would require to be provided by railway companies, as nearly all coals for shipment here are carried in railway trucks, and the trucks after being discharged are probably sent to a different colliery from that at which they previously loaded.

5. Trucks with about 6½ tons of coals are lifted from wharf by steam crane, and lowered on board the vessel to within two to three feet of hatchway, the fall therefore depends on depth of ship's hold and as she loads is decreased.

From Mr. McCullough.

1. Coal is put into waggons and will average about 24 hours ere it is shipped. Has been no material change the last few years. From 5 to 20 years ago, if a colliery was short of waggons, coals would be teemed by, *e.g.*, put in a heap, but this is very expensive.

2. No precautions are taken to shelter coals in waggons during wet weather, and sometimes from various circumstances coals will be left in trucks for several days.

3. Yes, ship in all weathers as a rule.

4. Coals could not be protected from the weather without incurring serious expenses.

5. This varies very much. Some colliers use large iron tubes, which are filled at spout, then lowered into hold; bottom opens by means of a clutch, and the coals are left at bottom of hold, where swing drops are used. Waggon is lowered close to deck; bottom opens; the coals fall into hold of ship; generally in Wales spout reaches within 6 or 8 feet of deck, and from there falls into hold.

From Mr. John Evans, Foreman, to James Thick and Sons.

1. Generally from a week to a fortnight, but in some cases as little as 24 hours. No change since the railway was completed.

2. No precautions are taken to shelter coal, and it is frequently left open for days.

3. Coal is shipped in all weathers.

4. No.

5. From 15 to 20 feet, according to the mode of shipment.

From Messrs. Barnes, Guthrie, & Co., Cardiff.

1. Coal generally shipped within 24 hours after coming out of the mine, seldom, if ever, stacked in this country, excepting in cases of cargoes being relanded; often stored in foreign depôts for many months.

2. No precaution taken. Seldom left in trucks for any length of time, and then only in extreme cases a few days.

3. Coal shipped in all weathers,

4. No.

5. The waggon tips the coal down a shoot about 15 feet long, and from thence the first portion of the cargo runs into an iron box, which is lowered into the hold. The coal falls at an average about 6 feet all throughout an entire cargo.

From Mr. F. E. Stamford, Cardiff.

1. Coal is frequently shipped within 24 hours after it is worked from the mine. Owing to the great pressure for coal during the last three years, the waggons have been emptied sooner than in former years.

2. No.

Yes.

3. It is.

4. No.

5. The coal is tipped out of the waggons down a shoot (of about 15 feet long) to the hatchway of the vessel; the distance from the hatchway to the bottom of the hold is dependent upon the size of the vessel.

From Mr. D. Turner, Peninsular and Oriental Steam Navigation Company, Liverpool.

1. Since the commencement of the late strikes in the coal trades and the consequent short delivery, I find the time elapsed between the dates of leaving the mines and that of shipment is from 24 to 48 hours. I cannot answer the latter question, as my experience does not go back so far.

2. None. Yes.

3. Yes; as required.

4. Certainly not without covered trucks or covered sheds, which would involve expense.

5. Eight to nine feet, the way the sailing vessels are loaded for the Peninsular and Oriental Company.

From Mr. Alexander Dalziel, Cardiff.

1. From 12 hours to three or four days, according to the demand?—No.

2. No, excepting in the cases of certain consumers, who pay a little extra price for having the coals in the best condition?—Yes.

3. It is the custom to ship the coal in all weathers.

4. Coal is too bulky to be covered without great expense and inconvenience.

5. From one to five feet. The coal is lowered by a mechanical contrivance until a cone is formed in the hold.

If this precaution be neglected, then the fall of the coal is great, from 12 to 30 feet; the rule is, however, very fairly adhered to.

From Messrs. John Carr and Son, Newcastle-on-Tyne.

1. From three to 24 hours. No change.

2. None. No.

3. Yes.

4. No. We have been the shippers of several cargoes of steam coals, the vessels carrying which have been reported as lost with all hands, or burnt, but our evidence would only be hearsay. We believe many cargoes of coal are ignited, especially on board of British vessels, through the unfortunate system of keeping the ship's stores above the cargo in the hold, necessitating the use of candles at the exact point where the escaped gas must accumulate.

5. Coal falls from waggon to bunker at top of spout, in general about five feet, and the spout rests upon the combings assisting the fall to the depth of ship's hold, and the quantity of coal shipped.

From Messrs. T. Beynon & Co., Newport, Monmouthshire.

1. Generally speaking, it is raised from the mine, sent down to port, and shipped without delay, if the vessels are ready to receive it.

2. None that I know of. It often occurs that it stands in trucks completely exposed to the weather for some days previous to shipment.

3. Yes.

4. No.

5. In this port (Newport, Monmouthshire), the truck or coal is raised by hydraulic power, and emptied into a shute, the end of which rests or nearly so on the ship's coomings, and the coal when the shute is full is eased into the ship's hold by means of a "wing;" it follows that it is only about the depth of the ship's hold the coal has to fall, and in ships of great depth measures are taken to break this fall.

From H. Cloake.

1. Very often coal when in demand is shipped within a few hours after being worked, or filled into trucks, and I am not aware that any change has been made lately.

2. Coal being filled into trucks open at the top at collieries where worked, and in this manner remains in the open air until shipped, and often for days, sometimes weeks, if the coal does not meet with ready sale or is a coal of inferior class and not much in demand.

3. Coal is shipped in all weathers, and rain must be *very* heavy to prevent or delay shipping.

4. I can suggest no other method than by having covered trucks; in most cases this would not impede or cost more shipping coal, but would delay the loading of trucks at collieries and entail expense there.

5. Coal in Wales is mostly shipped from a shute, which shute goes into the hatchway, therefore the height of the fall of coal would be at commencement loading depth of ship's hold. After coal comes up in centre of ship the fall of coal is almost nil. Some coals are lowered into the hold from boxes until there is a heap formed. This is done to prevent breakage.

D.—PAPER OF QUESTIONS ENCLOSED IN FOREGOING CIRCULARS A. AND B.

THE FOLLOWING IS THE PAPER OF QUESTIONS ENCLOSED TO SHIPOWNERS AND MASTERS, TO BE FILLED UP BY THEM IN THE EVENT OF THEIR HAVING HAD EXPERIENCE OF AN ACTUAL CASE OF SPONTANEOUS COMBUSTION OR EXPLOSION IN COAL-LADEN VESSELS.

1. Name of ship, her class and age, and whether steam or sailing.
2. Date, and port of loading.
3. Destination.
4. Date, and place of combustion or explosion.
5. Of what material the vessel was contructed.
6. Did she make much water, or otherwise?
7. Her (net and gross register) tonnage.
8. No of statute tons of coal shipped.
9. Denomination of coal carried, the name of the colliery, which seam, and whether double or single screened.
10. Name of exporter.
11. Whether the ship's hold was ventilated, and if so, how? And state the material used.
12. In what part of the ship the combustion or explosion took place.
13. How long the coal had been on board.
14. How long the coal had been out of the pits before being put on board.
15. How was the ship loaded, and, if from waggon, from what height did the coal fall into the hold?
16. State of weather at time of loading, and generally during her voyage.
17. Condition, as to moisture, of coal when put on board, and also during the voyage.
18. Was any indication given of the cargo being heated before the fire broke out; if so, how did it show itself, and how long before the fire?
19. Was the temperature of the hold taken during the voyage, and, if so, how, and how often?
20. Was the hold completely filled with coal up to the hatches?
21. During what proportion of the voyage were the hatches open, and did water ever get down, and to what extent?
22. What measures were adopted when there were indications of the cargo heating, and also when the fire broke out?
23. What special appliances, if any, had you on board for the extinction of fire?
24. To what causes do you attribute the spontaneous combustion or explosion in this case?

Please write "confidential" against any of your answers that you desire to be so treated.

PARTICULARS OF CASES OF SPONTANEOUS COMBUSTION SENT IN REPLY TO THE FOREGOING QUESTIONS.

(These cases will also be found in the tabular statements of vessels bound from Great Britain to foreign ports, which suffered from spontaneous combustion. See page .)

"COLUMBIA."

From Captain JOHN RAMSEY (Master) by Mr. THOMAS HENDERSON, Shipowner.

1. "Columbia," 4 years A 1, two years old, sailing ship.
2. Liverpool, January or February 1850.
3. Aden.
4. First off the Cape of Good Hope. When about 90 days out.
5. Wood.
6. No, she was a very tight ship.
7. 517 tons, 633 tons.
8. About 750 tons.
9. I think it was North Welsh coal; cannot give any further information.
10. Coals were for Peninsular and Oriental Company.
11. Ship's hold was ventilated with wood ventilators laid fore and aft, and up and down into the fore main and after hatches.
12. Was first discovered just abaft the fore hatch.
13. About 100 days.
14. Unknown.
15. Entirely from craft.
16. Wet weather whilst loading. Fine-weather passage from leaving Liverpool until fire discovered.
17. Coals wet when taken on board.
18. Yes, for 10 or 15 days before fire broke. Could find a great heat coming up the after hatch.
19. No.
20. Filled up to the main deck in the way of the main hatch, but a space vacant at both ends.
21. Fore hatch was often off, and the after hatch having a booby hatch over it was never closed.
22. So soon as the fire was discovered the carpenter bored a hole through the side, and water was allowed to run in until there was 10 feet in the hold at which height the fire was supposed to be, and when the fire was supposed to be extinguished was pumped out. About a fortnight after the fire again burst out, and the same expedient was used, when 12 feet was run in.
23. None, except the small deck pump.
24. I attribute it to ventilation; up till last year I was constantly employed carrying coals of all descriptions to the East Indies, and on no occasion were the cargoes ventilated, and I had no mishaps.

"ALFRED."

From Captain JAMES FINLAY.

1. "Alfred" of Belfast, sailing ship, classed at Lloyd's five years old.
2. September 1850, London.
3. Valparaiso.
4. January 9th, 1851, off the port of Talcahuano, Chili.
5. Wood spruce, built at St. John's, N.B.
6. A moderate quantity.
7. 1,073 tons register.
8. 1,050 tons, not a full cargo.
9. Transhipped from several collier brigs which brought the coals from the Tyne. Do not know from what colliery, or if screened.
10. Do not know; was bought in the London market by the owner of the ship.
11. Not ventilated.
12. A little before the main hatch, starboard side, about the centre of the body of the coals.
13. Nearly four months.
14. Cannot say, suppose at least eight or ten days. The coal was all weighed into the ship in drafts of two hundredweight each; so any gas among the coals would have ample time to escape.
15. Dropped from the deck into the hold, a height of about 20 feet at first, gradually decreasing.
16. Fine whilst loading, and generally fine during the voyage.
17. Seemed dry all the time; were a small sized coal.
18. First indication was a smell of gas in the hold about 48 hours before the fire broke out, next smoke gradually increasing.
19. No.
20. The hold was only filled to about midway between the 'tween deck beams and the upper deck.
21. The hatches were generally opened during the day in fine weather and closed at night. No water of any consequence got down.
22. A large pit was dug in the coals where the smoke was first seen oozing out, a hole cut in the upper deck above the pit, and water poured down till the pit overflowed; we

found water would not penetrate the coals that way, but flowed over and ran down by the ship's side.

23. None.

24. I attribute this combustion to the small coal being all in one body; the ship not having any 'tween decks to separate the cargo and want of air shafts to carry off the gas generated in the body of the coals.

"MARY SHEPHERD."

From Messrs. Gillespy and Scott, London.

1. "Mary Shepherd," A1 at Lloyd's. 10 years. 4 years old; sailing ship.
2. Autumn, 1852. In London.
3. Shanghai.
4. Spring, 1853. In China Sea.
5. Wood.
6. Believed to be tight.
7. $\frac{550}{629}$ Builder's Register.
8. Record destroyed.
9. Resolven, from Resolven Colliery—handpicked.
10. Thomas Gillespy (now deceased).
11. By wooden trunk alongside keelson, and two or three shafts to hatchways.
12. Believed to have been in the forehold.
13. Four or five months.
14. It was brought by Great Western Railway from Swansea to Bull's Bridge, near Brentford, and conveyed thence by lighters to the West India Dock, London. It was many days on the road much longer than would be the case now.
15. From lighters brought alongside. The coal was lowered into hold by baskets.
16. Wet during loading; do not know the weather during voyage.
17. Exposed to weather in transit from Swansea.
18. Hot decks, &c., for sometime before the fire.
20. Most probably not.
24. It has generally been attributed to the coal having been shipped wet, no other cause has we believe been suggested.

"ADMIRAL LYONS," "GLENLYON," "HANNAH LAW."

From Captain F. G. Fry, Liverpool.

1. "Admiral Lyons," 7 A1; about five years old, sailing ship; I was mate in this ship; "Glenlyon" under my command; "Hannah Law," I was on survey. "Glenlyon," ditto.
2. "Admiral Lyons," Sunderland, June 1861. "Glenlyon," Birkenhead, 1865. "Hannah Law," Birkenhead, 1868. "Glenlyon," Birkenhead, 1868.
3. All. Bombay.
4. "Admiral Lyons," July 1861, about 5° S. 25° W. "Glenlyon," January 1866, Aden. "Hannah Law," January 1869, Bombay Harbour; "Glenlyon" January 1869, Bombay Harbour.
5. All of pine woods.
6. None of them to any extent.
7. "Admiral Lyons," 1133. "Glenlyon," 1440.
8. "Admiral Lyons," about 1,000 tons coal, 400 tons coke; "Glenlyon," 2,050 tons, all coal.
9. "Admiral Lyons," I believe Howard's, West Hartley double screened; "Glenlyon," Westminster, Brymbo double screened; "Hannah Law," I do not know the colliery but shipped from Birkenhead.
10. "Admiral Lyons," do not remember; "Glenlyon," Messrs. Battersly.
11. "Admiral Lyons," two shafts, one each side, 4 feet from the bottom of the ship connected with shafts up each hatch; "Glenlyon," planks on the beams 4 inches apart, ventilation along each side of the keelson connected with a shaft up each hatch.
12. "Admiral Lyons" right under the main hatch about 5 feet from the keelson and close to the ventilator. "Glenlyon," right under the fore hatch, about 5 feet from the keelson and close to the ventilator.

When smoke was first seen issuing from the hold we endeavoured to find the locality of the fire, finding that the temperature in the main hold was the greatest, commenced pouring water down by means of buckets and fire engine, but finding the heat and smoke increasing rapidly, held a council, and decided upon scuttling the ship and admit sufficient water to reach the fire if possible; did so; in the mean time attached large canvas hoses to the main pumps, and kept them going down the main hatch; when the water in the hold had reached 8 feet found steam issuing up the hatchways, plugged the holes up that were made for scuttling. The heat gradually decreased when we dug down the cargo in main hatch and found the fire was entirely extinguished, it having originated about the foot of the ventilation shaft, having burnt it; also a part of the main beam. At times during the rest of the voyage, when we found the temperature of the hold increasing, we poured large quantities of water over the coals which seemed to cool them down; arrived safely in Bombay, in October 1861. "Hannah Law" and "Glenlyon" in each case when called on survey I advised the immediate flooding of the hold which was in each case successful.

13. "Admiral Lyons," 7 weeks; "Glenlyon," 22 weeks; "Hannah Law," 5 months; "Glenlyon," 6 months.
14. I do not know exactly, but I think about two days.
15. "Admiral Lyons" from a short fall about 12 feet. "Glenlyon" tips fell from 15 to 25 feet. "Glenlyon" ditto. "Hannah Law," I think tips, but not certain.
16. "Admiral Lyons," wet. Voyage mostly fine; "Glenlyon," fine loading; fine during voyage; but much calm weather.
17. "Admiral Lyons," damp; steamed a little during voyage; "Glenlyon," dry when shipped; same during voyage; but I noticed much pyrites among the coal.
18. "Admiral Lyons," noticed a little smoke coming up the ventilators a few hours only before the fire; but rapidly increased. "Glenlyon," noticed an increase in the temperature of the hold for several days before the fire.
19. "Admiral Lyons," no; "Glenlyon," every day at noon.
20. Not quite; a free passage was left between the coals and deck fore and aft in each vessel.
21. "Admiral Lyons," fore and aft hatches were kept off always during fine weather, but not the main hatch, as the long boat was stowed on the top of it. "Glenlyon," the hatches were all kept off during fine weather about two thirds of the passage; in each no water got down.
23. In each case a good fire engine, plenty of hose, and two other pumps.

"USK."

From Messrs. T. Beynon & Co., Newport (Mon.).

1. Barque "Usk," lost 1861; built at Newport Mon., age 9 years; sailing vessel, 338 tons register.
2. Swansea, in 1861.
3. Valparaiso and a port.
4. About 200 miles south of Valparaiso.
5. Wood.
6. No, she did not make water.
7. 338.
8. About 450 tons of coal.
9. Smelting coal which was not screened.
10. I think it was Messrs. Henry Bath and Sons, Swansea.
11. In the usual manner, with wooden shafts from each hatchway.
12. It was supposed under the fore hatchway.
13. About 100 days.
14. I think it was shipped almost directly it came from the pit.
15. The ship was loaded from waggons tipped into a shute and allowed to run from thence into the ship's hold.
16. The weather was raining during the loading of the vessel.
17. The coal appeared to be damp when shipped.
18. Only a few hours previously a great heat was felt in the forecastle, and on the fore-hatches being taken off the flames burst up as high as the tops, and the ship was immediately abandoned.
19. I do not think it was.
20. The main hold was completely filled up to the hatches, but the after and fore holds were not.
21. I cannot tell.
22. There was no time to take any measures.
23. None.
24. To the cargo being shipped in a damp state, principally.

"JOANNA," "WANDERER," "ANNIE LAURIE," "MAY QUEEN," "PALMYRA."

From Mr. William Thompson, Uphall.

1. 1 "Joanna". No class. Sailing.
2 "Wanderer." Do. Do.
3 "Annie Laurie." Do. Do.
4 "May Queen." A 1 Do.
5 "Palmyra." A 1 Do.
Nos. 1, 2, 3. August, 1864. Leith.
,, 4. September, 1874. Newcastle.
,, 5. June, 1874. Liverpool.
3. Nos. 1, 2, 3. Quebec.
,, 4. Rangoon.
,, 5. Madras.
4. Nos. 1, 2, 3. October 1864. St. Lawrence.
,, 4. February 1875. Off Madras.
,, 5. October 1874. Do. But Extinguished.
5. All timber.
6. No.
7. No. 1, 555 tons. No. 2, 639 tons. No. 3, 645 tons. No. 4, 1,054 tons. No. 5, 888 tons.
8. No. 1, 442 tons. No. 2, Account lost but under 500 tons. No. 3, 434 tons. No. 4, 1,340 tons. No. 5, 1,206 tons.
9. Nos. 1, 2, 3. Duke of Buccleuch. Unscreened.
,, 4. Cowpen Hartley. Screened.
,, 5. Lancashire Do.
10. Nos. 1, 2, 3, 4. On ship's account.
,, 5. Madras Railway Company.
11. Nos. 1, 2, 3. Not ventilated.
,, 4 and 5. With wood up and down each hatch.
12. Unknown.
13. Nos. 1, 2, 3. About two months.
,, 4 and 5. About four months.
14. Nos. 1, 2, 3. I believe had been stacked at the pit head for some time.
Nos. 4 and 5 fresh drawn.
15. All from waggon by steam crane.
About 20 feet.
16. Unknown.
17. Unknown.
18. By smoke and strong smell of gas.
19. No.
20. In none of them.
21. Orders given to masters to open hatches only in fine weather, and believe this was attended to.
22. Unknown.
24. In cases Nos. 1, 2, 3, the quality of the coal, they were all shipped at the same time. We had carried them for two or three years previously quite safely. We have never shipped any of them since.
Nos. 4 and 5 we cannot explain.

Captain William Riddock, of London, also sent an enclosure relative to these cases.

"LOCHIEL."

From Captain Newton.

1. "Lochiel," of Dundee. Sailing. About nine years old.
2. Off Tayport, 17th September 1864.
3. Calcutta.
4. 4th January 1865. Bay of Bengal.
5. Wood.
6. Tight.
7. 1,501 tons.
8. 1,500 tons.
9. Townhill colliery.
11. Partially ventilated; wood.
12. Amidships; main hatchway.
13. 3½ months.
15. Loaded by tubs from lighters.
16. Very bad while loading; coals thoroughly wet with sea and rain. Moderate during voyage.
17. Very wet.
18. Only 24 hours before. Smoke.
19. No.
20. No.
21. Off every day except a few days before the explosion when crossing Equator.
22. Battened down hatches and made for Galle; escaped in the boats.
23. Ship's pumps.
24. Wetness of the coal.

"HER ROYAL HIGHNESS."

From Messrs. Charles Hill and Sons, Bristol.

1. "Her Royal Highness." A 1 eight years. New ship
2. November 1865; reshipped January 1866. Cardiff.
3. Valparaiso.
4. April 5th, 1866. Latitude 39° S., longitude 54° W. Cargo found to be in a heated state; bore up for Monte Video.
5. Wood.
6. Made some water during bad weather.
7. Gross about 1,280; net register 1,244 tons.
8. 1,804 tons.
9. "Rhonda," No. 3, through and through.
10. Henry Bath and Sons.
11. Ventilated as described in sheet No. 1.
12. The captain, on examining the cargo, found it very much heated under the main hatch, and smoke or steam issuing from it; he forced bars of iron into it as far as possible, and on withdrawing them they were found to be very hot; he feared combustion, and ran the ship back to Monte Video, which was reached in a few days, and where he sold the coals, as the expense of landing and reshipping would have been heavy.
13. Three months.
14. No means of knowing.
15. Loaded in first place from tips, and on reshipping from barrows wheeled over the hatch with a fall of about 24 feet at commencement.
16. The coals were exposed to rain after being discharged; some heavy gales were experienced during the passage.
17. See above.
18. Answered.
19. Temperature frequently taken, and by that means the tendency to combustion was discovered.
20. The cargo was pretty close up in main hatch at starting, but the hold was not full.
21. The hatches were taken off in fine weather, but no water got down.
22. Answered.
23. Answered.
24. This was a new ship and as she was very flat in the floor we placed the platform as high as we thought she would bear it to make her "sea kindly," but we put it too high, and in getting down channel she would not stand up to her canvas, and had to be put back; we discharged a large portion of the coals and took up the platform for a length of 50 or 60 feet, and let the coals run down on to the skin and then reshipped the portion that had been landed; I attribute the accident to the coals having got damp before reshipping, and the platform having been removed from the bottom of the ship so that there was no air or ventilation under the midship portion of the cargo.

"ROYAL FAMILY."

From Captain William Buchanan.

1. "Royal Family," sailing vessel, run five years without being classed, and was then classed five years. Was about six years old at time referred to.
2. Birkenhead, February 1868.
3. Aden.
4. Latitude 35° S., longitude 5° W., 72 days out from Liverpool.
5. Wood.
6. Tight but had encountered very severe weather.
7. Net tonnage register 1,750.
8. About 2,500 tons.
9. Welsh steam, shipped at Birkenhead, single screened.
10. P. and O. Steamship Co.
11. Platform, open ended with jelise ventilator in main hatch of wood, (this ventilator which was all broken below I considered as the cause of heating.)
12. About 14 feet from deck in main hatch.
13. About 80 days.
14. Unknown.
15. Wheeled on board.
16. Rainy weather in loading variable.
17. Damp when put on board, ship tight during voyage.
18. Hatches taken off every opportunily.
19. Once a week driving down iron rods in each hatchway.

20. Main hatch, yes. Ends, no.

21. Water got into the hold only through ventilator in main hatchway.

22. Found that after leaving iron rods in hatches four hours drew them up when they were so hot that they singed the hands. At once dug down to a depth of 14 feet and allowed the air to get at the heated coal.

23. Head and portable force pumps with hose.

24. Ventilator in main hatch being broken below (when loading) while end above remained open, allowing any water which might find an entrance to spread (or lodge) in the coals.

"PERSIA."

From Mr. J. H. Watt, Glasgow.

1. "Persia." Sailing ship, 1,290 tons. Built at Quebec, 1864. Seven years A 1.
2. Sailed from Greenock, 1st September 1868.
3. Bombay.
4. Off Cape of Good Hope, 31st October 1868.
5. Wood.
6. None.
7. Net register 1,290 tons; gross 1,331 tons.
8. 1,830 tons coal.
9. Wishaw coals. Lanarkshire. Single screened.
11. Ventilated with wood fore and aft.
12. Under the poop.
13. About 60 days.
14. Brought direct from the pits to the ship.
15. Loaded with six tons waggon, put on board by engine power, and might fall about 12 feet.
16. Weather was showery.
17. Coal was dry.
18. Heat was experienced in the poop, then in a few days smoke was seen, which increased, until decks were burning; this was followed by a violent explosion, the ship was wrapped in flame and then abandoned.
19. Not that I am aware of.
20. No.
21. I have no knowledge. Ship was tight and no water came on board.
22 Hatches were secured to cut off air, but the final destruction was very rapid.
23. Hand engine with long hose.
24. I cannot give any reason.

"JANET COWAN."

From Mr. James Cuthbert, Greenock.

1. "Janet Cowan," Â, three years sailing.
2. August 1869, Greenock.
3. Bombay.
4. 1st. November 7th, latitude 34° 52′ S., longitude 59° 53′ E.; 2nd. November 21st, latitude 29° 26′ S., longitude 69° 5′ E.; 3rd. December 11th, latitude 12° 58′ N., longitude 70° 16′ E.
5. Iron.
6. She made no water.
7. 1,277 $\frac{70}{100}$.
8. 1,756.
9. Cannot say.

10. J. and R. Young, Buchanan Street, Glasgow charterers.

11. Three ventilators, one in main hatchway, one in quarter hatch, one in ventilator on top of house on deck, of 9-inch deals, with 2½-inch space.

12. In the hatchways.

13. From August till above dates.

14. Cannot say.

15. From Crane by waggon, about 25 feet at the commencement, or about three feet from hatch coomings.

16. Wet for the first 300 tons or so, dry for remainder; fine till within a week of the fire.

17. Seemed to be dry.

18. November 7th, 1869, latitude at noon, 34° 52′ S., longitude 59° 53′ E. Moderate breeze from S.W. at 4 a.m. Saw smoke coming up the quarter hatch, went below and found the hole full of smoke; took off the main hatches and saw the yellow smoke issuing up from among the coals; commenced to get the coals on deck, when up to the rail both sides put them overboard by 11 a.m., was at the 'tween deck beams; the coals were very hot, the smoke yellow and stifling; got on the force pump and played water on the coals in the hatchway, and kept taking them up at the same time; 1 p.m. opened three of the side ports and let the water in as the ship rolled; supposed the fire to be at the bottom. 4 p.m., closed them at 6.30 p.m., no smoke to be seen; kept the pump at work till 7 p.m.; 2 feet 5 inches of water in the pump well; put on all hatches and stopped all ventilation, also main mast-head; covered it as the smoke was coming up it like a funnel during the day, but none at the foremast.

November 8th, lat. 32° 20′ S., long. 61° 50′ E., wind westerly, moderate breeze and cloudy weather, heavy swell from the westward; at daybreak took off the hatches, a great quantity of steam in it and very hot, the coals smoking; put on the force-pump, put planks against the stanchions to keep the coals from falling from between deck, got more coals up. 2 p.m. Got to the coals that were on fire, which had been watered out. 5 p.m. No smoke or steam, kept the pump going till 7 p.m.; cloudy and gloomy looking to the westward; put the coals that were on deck overboard. 8 p.m. Commence pumping the ship out, kept off all hatches, and open all ventilators.

November 9th, lat. 31° 2′ S., long. 62° 31′ E., wind N.N.W., light breeze and cloudy, no smoke to be seen in the hole; dug down in the coals 9 feet from the 'tween-decks, the coals were hot, but no appearance of fire.

November 10th, lat. 29° 26′ S., long. 65° 58′ E. No fire or smoke to be seen in the hole; dug down into the coals, all felt cool, put in another ventilator in the after part of the hatch, no appearance of smoke or steam until the 21st.

November 21st, lat. 8° 32′ S., long. 69° 5′ E. Light variable winds and calms; 2 a.m. smoke was seen coming up the quarter hatch ventilator; commence to get the coals up, put them in the main hatch and on deck; 5 a.m., down as far as between decks, coals hot, smoke suffocating, which caused some of the crew and myself to vomit, and made them unable to proceed with work for a time; was then only able to fill three beef casks and be relieved by others, the force pump at work, taking the coals up at the same time; by noon down 7 feet below 'tweendecks, no smoke to be seen; kept all hatches and ventilators open.

November 22nd, lat. 7° 13′ S., long. 69° E. Light variable winds, no smoke, coals hot, got some more on deck; dug down 10 feet from between decks, saw steam but no smoke, put another ventilator in the fore part of the hatchway.

November 23rd, lat. 6° 32′ S., long. 69° 07′ E. Light breeze from the westward, took some more on deck as they felt hot, kept the pump at work for about an hour when all felt cool.

December 11th, lat. 12° 58′ N., long 70° 16′ E. Fine weather 7 a.m.; smoke was seen coming up the forehatch; trimmed the coals in between decks aft, got them up from lower hold and put them in between decks; by noon got to the coals that was burning kept the force pump at work for about an hour when all felt cool.

December 12th. No appearance of fire.

19. Not until after the first fire, but was tried after by thermometers.

20. In the main it was, but could crawl fore and aft the ship in the wings.

21. Always open only in bad or wet weather. No water got down.

22. Perseverance to get to the place that was on fire, and would do the same again if ever it was my lot to be in the same predicament, but will be glad to hear of some improvement.

23. Force pump with hose.

24. To the dust left in the wake of the hatches and a current of air could not get through amongst them. I would propose to have large ventilators that a man could get down and clear the dust or small coal with a rod of iron and take the dust and coal out of it to the bottom of the ship, also a small ventilator fore and aft in the body of the coals to be led to the main ones.

Mr. William Buchanan, of Greenock, also sent an enclosure relative to this case, and states in reply to questions No.

9. Splint coal from Messrs. Scott and Gilmours' Collieries Wishaw district, screened at pit but don't know whether double or single.

24. Some mineral defect in the coal as many vessels loaded from same pits took fire.

"SERENA." *

From Captain George Symons, Swansea.

1. "Serena," 16 A1; three years sailing.
2. Swansea, October 1869.
3. Valparaiso, but put into Rio de Janeiro.
4. Indication of fire off Madeira. Put into Rio December 5. Condemned and sold the cargo. A crew kept employed throwing water on it for the purpose of working it out.
5. Wood and iron.
6. No.
7. 493 tons.
8. About 720.
11. Yes, as previously mentioned; yellow pine planks.
12. An intense heat all over the hold, and stunk of sulphur. Am certain, another week and it would have been all fire.
13. From October 20 to December 6.
14. Cant say.
15. From wagons, 40 feet fall.
16. Very fine.
17. Dry throughout.
18. Actual fire did not break out; a few days more was only required. Indication, top of cargo quite green, similar to metals calcined, and smell exactly same (suffocating); had to live on deck for some days previous to entering Rio.—Much slate in this coal; when discharging it came out white with heat, and smelt strong of sulphur. I think this slate in cargoes highly dangerous.
19. Frequently with them.
20. No. Could walk and crawl over it.
21. Hatches daily open. Water was thrown down, the day I bore up for Rio, for the purpose to cool it.
22. Turning it over and over, and ultimately jettisoned a portion.
23. None. Pump.
24. Veins of sulphur in more or less of the coal, and slate as above. A coal owner once told me there are veins in mines or pits that should not be mixed with coal for long voyages; but the colliers will do it if not watched. No doubt there is something in this. *Memo.*—Last February I was at F. Isles—(went out about the "Alpha"),—and in Stanley there were five ships with different kind of coals on board. All put in with coals on fire. Cargoes ventilated in the usual way. Two of my voyages exceeded four months. A full cargo of coals, and not ventilated, turned out cool and in good order. Consequently I think it's a spontaneous vein in and about some pits that will ignite when in a heap of some tons, whether ventilated or any other precaution used. (I allude to the Swansea copper ore fitted ships in ventilation, which is evidently the best plan.)

"GLARAMARA."

From Messrs. John Brymner & Co., Greenock.

1. "Glaramara," about 13 years A 1, sailing.
2. Greenock, May, 1870.
3. Matanzas.
4. Off Cuban Coast, July 1870.
5. Wood.
6. Tight ship.
7. 475 $\frac{45}{100}$, and 449 $\frac{28}{100}$.
8. 660 tons.
9. Wishaw Main coal.
10. John Brymner & Co.
11. Partially with wood.
12. Apparently in forehold.
13. About 60 days.
14. Believed to be fresh dug.
15. Partly by hand, partly by drop of from 8 to 10 feet.
16. Fine weather during loading, apparently the same during voyage.
17. Dry.
18. Slight smoke shown for two days before fire broke out.
19. Cannot tell.
20. No.
21. Have no information.
22. Water poured down the hatches.
23. None.
24. Captain could not suggest any reason; informed us he had taken every precaution; except day after fire was seen when there was a violent squall, with rain, &c.

"VANDA."

From Messrs. Stoddart Brothers, Liverpool.

1. "Vanda," A1 at Lloyd's; age five years. Sailing ship.
2. Shields; River Tyne, May 1870.
3. Calcutta.
4. Near Cape of Good Hope. Was abandoned on fire 28th July 1870.
5. Wood.
6. None.
7. 1,081 under deck. 1,141 gross.
8. 930 tons; remainder of cargo was 560 tons coke, and 11 tons bricks.
9. Do not know.
10. Alfred Turner, esquire, Liverpool.
11. Do not think there was any special arrangement for ventilation. Captain instructed to keep hatches off when opportunity offered,
12. Cannot say.
13. A little over two months.
14. Do not know; perhaps 12 to 18 hours.
15. Loaded from waggons by spouts; cannot say the height.
16. Do not know.
17. Do not know.
18. Cannot give particulars. Captain at sea; letters put away.
19. Cannot say.
20. Coal and bricks in lower hold and coke in 'tween decks.
21. Cannot say.
22. Cannot say.
23. Fire engine.
24. Cannot name any particular cause.

"MALABAR."

From Captain J. Galloway.

1. "Malabar," A 1, two years; sailing.
2. June 1870, London.
3. Batavia, Java.
4. July 25th, 1870; 29° S. latitude; 30° W. longitude.
5. Iron frame and iron-plated.
6. Quite tight.
7. 1,200 register.
8. 1,274 tons 14 cwt.
9. Langley Mill.
10. On ship's account.
11. Yes; horizontally on each side of keelson, and perpendicularly at each hatchway.
12. Main hatch.
13. Thirty-six days.
15. By hand-basket from coal barges.
16. Dry weather and fine weather till discovering fire.
17. A portion taken in wet from leaking in barges, but no water afterwards got near them.
18. No indication until the smoke was discovered.
19. Not taken.
20. No, only two thirds cargo on board.
21. Hatches constantly open, and no water got down.
22. When we observed smoke threw about 50 tons overboard, and worked force pumps for two days until compelled to close hatches.
23. Only the two force pumps.
24. Bad quality of coal and damp condition.

"L'AGOUHANNA."

From Captain William Kennedy.

1. "L'Agouhanna" 8 A1 five years old, sailing.
2. About 10th October 1870, Birkenhead.
3. Bombay.
4. About 25th January 1870, about 500 miles S.S.W. from Bombay.
5. Wood.
6. Ship tight.
7. Net 1,103, gross 1,130.
8. 1,600 tons.
9. This information to be had from Messrs. W. H. Ross & Co., Liverpool.
10. Refer to Messrs. W. H. Ross & Co.
11. Yes, explained in clause 4 other sheet, wood.
12. About mainmast.
13. About three and a half months.

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* A subsequent case of combustion appears to have occurred in this vessel in 1870 under very similar circumstances.

15. By waggon, coal fell about 20 feet to hatch coomings.

16. Showery.

17. Damp when shipped.

18. Yes, by quantity of steam rising from hold about three days before fire occurred.

19. Once a week by thermometer; found very little difference in temperature till fire appeared.

20. No.

21. Hatches open at all convenient seasons, no water ever got down.

22. Applied force pumps.

23. Force pumps.

24. To the height coals fell into the hold.

"CEREAL."

From Messrs. **Hine Brothers**, Maryport.

1. "Cereal." A1. 13 years old. Sailing ship.

2. August 7th, 1872, Swansea.

3. Santos.

4. September 28th, 1872. 4–30 S. 27–30 W.

5. Wood.

6. Made no water.

7. 299 gross. No nett.

8. 488.

9. Half, Merthyr Dare Colliery, and the other half, Gravesend and Gellia Cadoxton Collieries. Double screened. Cannot tell which seams.

10. J. S. de Vasconcellos & Co., London and Liverpool.

11. Not ventilated.

12. In the centre.

13. 60 days.

14. Fresh wrought.

15. Loaded from spout, the outer end of which to the hatchway was about five feet.

16. Rained all the time of loading. Fine weather until she took fire.

17. Wet.

18. Felt heat about week before fire broke out. No smoke until the day before flames broke out.

19. No.

20. Not nearly so; three feet from main hatch, five feet from fore and after hatches, and both ends of the ship empty.

21. Open all the time and no water ever got down.

22. Captain never expected ship was on fire until day before flames broke out. Time occupied in preparing and provisioning boats which was just accomplished in time Bore away from Pernambuco in hopes of scuttling ship there.

23. No special appliances.

24. Probably on account of being fresh wrought and shipped in a wet condition, combined with perhaps the fiery nature of the coals. This latter is only a surmise.

"BIVOUAC."

From Mr. **William Laing**.

1. "Bivouac," 20 years in Red, in Liverpool book, built in 1871, steam.

2. Sailed 23rd October 1872 from Newcastle-on-Tyne.

3. Odessa.

4. Between Gibraltar and Malta beginning of November 1872.

5. Iron.

6. None.

7. At that time $\frac{1122}{721}$ gross nett, now $\frac{1126}{723}$ gross nett.

8. 1,229 tons cargo.

9. Not known.

10. George Pyman & Co.

11. Yes, with galvanized iron cowled ventilators, two to each hold or compartment.

12. Main hold.

13. About ten days.

14. Not known.

15. By a staith or spout in the Northumberland Dock. Height not known.

16. Wet at time of loading.

17. Wet when put on board.

18. A light steam was seen a day before the fire broke out, but was supposed to be caused by the moisture evaporating.

19. No.

20. Not known, but very improbable, she being a three-decked ship.

21. As far as I know, hatches were never open, as weather was bad.

22. The cargo was carefully watched, and when it was noticed to be burning, holes were cut in the deck, and the ship's pumps brought to bear with the fire-hose inserted, which very shortly extinguished it.

23. Nothing special beyond the ordinary fire-hose attached to the engine pumps.

24. The wetness of the coals through being shipped at a time when heavy rains prevailed, and possibly aggravated by proximity to boiler bulkhead, which, however, was properly sheathed.

"HENRY BATH," "ALPHA."

From Captain **A. Peare**, Swansea.

1. "Henry Bath" and "Alpha," both sailing ships, A1 13 years, "Alpha" built 1863, "Henry Bath" 1864.

2. "Henry Bath," December 1872, at Swansea; "Alpha," July 1874, at Swansea.

3. Chili, West Coast of South America.

4. Off Cape Horn in both cases, about 70 days after sailing.

5. Wood.

6. Very little.

7. "Henry Bath," 486 register, 680 gross; "Alpha," 390 register, 560 gross.

8. 650 and 650.

9. Smelting coal, Mynyddy Bach.

10. Henry Bath and Son.

11. Planks on each side of stanchions from the keelson to the upper deck beams.

12. "Alpha," under fore hatchway; "Henry Bath," between fore and main hatchway.

13. About 70 days.

14. A short time.

15. "Henry Bath" loaded with boxes, lowered down to main hatchway before tipping; "Alpha" from spout or shoot that came down to the hatch.

16. Wet weather in both cases.

17. Wet when shipped.

18. Found the cargo getting heated, threw some overboard, and turned over as much as possible of the remainder.

19. Occasionally.

20. Filled up in the main hatch, not in the wake of fore and after ones.

21. Booby hatch aft nearly always open.

22. As mentioned above, and in the "Alpha" case pumped water into hold when the heat increased.

23. Pump.

24. The coal being small and wet.

"COUNTY OF NAIRN," "COUNTY OF PERTH."

From Messrs. **R. & J. Craig**.

1. Sailing ships, "County of Nairn," 16 years A1, three years old; "County of Perth," new 100A.

2. "County of Nairn" finished loading 3rd May 1873; "County of Perth" finished loading 4th July 1874.

3. "County of Nairn," Java; "County of Perth," Calcutta.

4. "County of Nairn," near Sourabaja; "County of Perth," near the entrance to the Hoogly.

5. "County of Nairn," composite; "County of Perth," iron.

6. Both ships were perfectly tight and had not at any time made water.

7. "County of Nairn," 1,047 gross, 999 nett; "County of Perth," 1,696 gross, 1,626 nett.

8. "County of Nairn," 600 tons; "County of Perth" 1,600 tons.

9. "County of Nairn," Hamilton Coal Company; "County of Perth," Roughrigg coal; both double screened.

10. "County of Nairn," James Finlay & Co.; "County of Perth," The Queen Steam Ship Company.

11. "County of Nairn," eight 12-inch ventilators and one 21-inch, of iron, all through main deck, and three iron mast ventilators; "County of Perth," one 28-inch wood from main deck to bottom of hold, with iron bell-mouth, and ten 12-inch of iron through main deck, and three iron mast ventilators.

12. "County of Nairn;" combustion took place in main hatch, lower hold; "County of Perth," combustion took place near the main hatch, in lower hold.
13. "County of Nairn," 110 days; "County of Perth," 114 days.
14. Direct out of the pits in the case of both ships.
15. Coals carted alongside of the ships and wheeled on board. The fall from planks to bottom of hold would be for "County of Nairn" 24 feet, and "County of Perth" 27 feet.
16. Fine dry weather during both loading and voyage.
17. Apparently dry when shipped and during voyage.
18. No indication of heating observed in either case, but a smell of gas was observed the evening before the fire broke out in the "County of Perth."
19. We are not aware whether the temperature of the hold was noticed during the voyage.
20. "County of Nairn," four-fifths of lower hold to a height of three feet; "County of Perth," about four-fifths of lower hold, from ceiling to a height of 7 to 8 feet, and about one-third of 'tween decks to a height of 3 feet.
21. "County of Nairn," hatches off nearly the whole voyage, and while discharging in Batavia; "County of Perth," the hatches were off nearly all the voyage till the evening before the fire, when they were put on in consequence of heavy rain; no water got down.
22. Fire hose from both pumps playing on the coal.
23. No other appliances but the fire hose and pumps.
24. Cannot attribute any cause for the spontaneous combustion.

"PERSIA."

From Mr. George Duncan.

1. "Persia," A1 9 years, built in 1853 (almost rebuilt in London, 1865).
2. Cardiff, 15th May 1873.
3. Aden.
4. Aden, 4th September 1873.
5. Wood.
6. None.
7. 1,704 net, 1,806 gross.
8. 2,087 tons.
9. Insoles Merthyr Steam for the Peninsular and Oriental Company.
10. Insole and Sons.
11. Same as stated in the "Indian Empire's" case.
12. In the way of the fore hatch near the bottom.
13. 112 days.
14. Cannot say.
15. Loaded from waggon which came very near the hatchway.
16. Good weather and favourable voyage.
17. Cannot say.
18. No; found on fire when discharging.
19. No.
20. No.
21. It is a standing order for hatches to be off during good weather. Believe no water went down.
22. Fire extinguished by aid of ships' fire engine hose. Ceiling was little charred.
23. Fire engine and head pump.
24. Cannot say.

"PRINCE ALFRED."

From Mr. Gates, Shoreham.

1. "Prince Alfred" (barque). Totally lost; burnt through combustion. Sailing vessel.
2. June 1873. Hull.
3. Galle.
4. Indian Ocean. Will obtain date, but think in August 1873.
5. Wood.
6. No, quite tight.
8. About 600.
9. I am unable to give the name at present.
10. I will find out, but cannot say now.
11. I believe it was not ventilated.
12. I believe the fore part.
13. I think between two and three months.
14. I cannot say, but understood they were shipped immediately.
15. I cannot say how great a fall.
16. I believe dry and warm.
17. I believe dry.
18. Yes, for some days before.
19. I cannot say, but believe not.
20. No.
21. I am not able to say, but believe water was thrown into the hold.
22. Hatches were battened down, but fire burst through deck.
23. No other than buckets.
24. Ignition of timber through combustion.

I shall be glad if I can in any way give information, but think the captain would be able to give more valuable information.

"CHANNEL LIGHT."

From Mr. Charles Beades, Lancaster.

1. "Channel Light," sailing, 6 years, and reclassed in August 1873 for six years.
2. Hull.
3. Rio.
4. Rio, October 1873.
5. Hackmatack, &c.
6. No.
7. 553 tons.
8. About 750 tons.
9. Do not know the name of the colliery.
10. Messrs. Wilson & Sons, 57, Great Church Street, London.
11. Yes, iron and wood.
12. Hold.
13. From 15th August to October 1873.
14. Short time only.
15. Tips trunk resting upon the deck.
16. Fine.
17. Dry.
18. No indication until after arrival at Rio.
19. Do not know.
20. No.
21. Do not know.
22. Ship moved into deep water and steam pumps used. Ship scuttled.
23. Force pump and hose.
24. Coal not laying long enough before shipment to let the gases escape.

"GEORGE DOUTHWAITE."

From Mr. Ralph Curry, Sunderland.

1. "George Douthwaite," built in 1860 at Blyth, Northumberland, 9 years A1, and continued.
2. 12th July 1873, Hull.
3. Constantinople.
4. First indications of combustion 6th September.
5. English oak frame, Stettin oak floors, planking inside and out, Danzig and Stettin oak, and American elm decks, Quebec yellow pine.
6. Ship quite tight.
7. 362 nett.
8. 626.
9. Reported as from South and West Yorkshire pits, double screened.
10. Cory Bros., of Cardiff.
12. Evidently amidships as main hatches blew off.
13. About two months.
14. Seven to 10 days out of pits before shipment.
16. Raining incessantly.
17. Wet decidedly.
18. By stilt and smoke first.
20. Main hatch, but not fore and after hatches.
21. After hatch would be open several times during voyage, as certain stores were in it on the cargo, lives, &c. No water would get down to the best of my knowledge.
22. According to captain's letter it seems to have been his intention to put the ship where she could be scuttled, and I fancy was afraid to take off main hatches for fear of giving free vent to fire.

The following is an extract from captain's letter:—

"I could do nothing else but put in here, she got worse every day; all hands were nearly suffocated with the smoke and dreadful stith; I bore up on Sunday night at 8 o'clock for this place, 11 o'clock at night she blew the hatches off, and sent sheets of flame up to the height of the main top. The wind came down the bay in the morning, so I had to beat up fresh breeze that set her going fast; the yellow smoke came rolling out like out of a steamer's funnel; as soon as we got in the entrance the men-of-war sent their boats with force pumps and played on the coals. Harbour master sent a steamer to tow her in. Put her into 18 feet water and scuttled her directly. The Greek men-of-war men and officers stopped on board all night to fill her up. An Italian frigate, a Russian gun-boat, and a Greek iron-clad are here, they all sent their assistance, about 50 hands on board us every day; commenced to pump her out yesterday afternoon; I shall hold survey to-morrow morning; cargo will have to be discharged, stores and spare sails all condemned in the Lazarette; could not get them up when we wanted, being full of smoke. Lloyd's agent intends to come to-morrow from Patras. The acting consul here told me to ask you to communicate with the Ionian Bank, London, about the money required here, &c."

23. No.
24. Think wet state of cargo when shipped.

"ST. MUNGO."

From Messrs. Gilmour & Co., London.

1. Sailing ship, "St. Mungo," built 1865, *A1.
2. Dundee, June 1873.
3. Point de Galle.
4. 13th September 1873, latitude 34° 57′ South, longitude 20° 18′ West.
5. Iron.
6. Perfectly tight.
7. 1,375 and 1,299 tons.
8. About 1,700 tons.
9. Townhill Colliery, steam coal, double screened.
10. Duncan Mitchell & Co.
11. Ship had 9 ventilators, fixtures, besides plank ventilators to all the hatches.
12. Aft, under poop.
13. Nearly three months.
14. Taken straight from the pit to ship.
15. From waggon, 10 to 20 feet.
16. Fine dry weather; very fair at sea.
17. Very dry.
18. Smoke seen on the 13th September, and she was abandoned 14th September.
19. Frequently.
20. There was a very large vacant space.
21. Nearly all the time.
22. Battened down to stop all draft.
23. Fire engine, &c.
24. Coal coming fresh out of pit.

"WALTER BAINE."

From Mr. J. S. Grieve.

1. "Walter Baine," AA1, 5 years, old sailing vessel.
2. Dundee, 8th July 1873.
3. Calcutta.
4. 27th September 1873, 39° 22′ S., 73° 19′ E.
5. Iron.
6. Never made water.
7. 898 tons nett, about 940 gross.
8. 1,170.
9. Spence's Wishaw Ell coal (Larkhalls district), single screened.
10. Baine & Johnston, Greenock.
11. Through ventilated; white pine was down the hatches and connected through along keelson.
12. Main hatch.
13. 81 days.
14. A few days.
15. From waggon; cannot say the height.
16. Very wet at time of loading.
17. Wet.
18. Refer to book by Rundell on Spontaneous Combustion on Coal Cargoes, page 46.
19. Not that I am aware of. Captain not in my employ now.
20. The bulk of coal was in the body of the ship, but not nearly loaded.
21. Cannot tell; believe no water ever got into the hold.
22. See Rundell's book, page 46.
23. Do.
24. I have a strong opinion the wet condition of the coal had a great deal to do with this case, and I do not approve of ventilators. I sent a new ship some months ago from Greenock, with 1,800 tons coal, and took care to ship only in dry weather; she arrived all right, after 120 days' passage. Another sister ship soon after was loaded with 500 tons coal and general cargo, for San Francisco, shipped in dry condition. She had 139 days' passage; arrived all right.

"ISABELLA KERR."

From Messrs. John Kerr & Co., Greenock.

1. "Isabella Kerr," 14 years A1, 9 years old, sailing.
2. August, at Dundee.
3. Calcutta.
4. 25th September, 32° 58′ S., 28° 28′ W.
5. Iron and wood.
6. Some water got in by side light, to which cause we attribute the fire.
7. 1,416 tons nett.
8. 1,955 tons.
9. Ell coal from Lanarkshire pits.
10. Ourselves.
11. Yes; wooden ventilator up each hatch and one along keelson.
12. After hatch, fore and aft in 'tween decks.
13. About seven weeks.
14. Cannot say.
15. From waggon at Dundee crane.
16. Wet during loading.
18. Captain found great heat coming up after-hatch ventilator five days before smoke came up.
19. No.
20. No.
21. Hatches kept open during fine weather; no water got down.
22. Heated coals were thrown overboard, and ship ran for St. Helena.
23. Force pump.
24. Water getting in through side light.

"ARRACAN."

From Captain C. S. Caird, Greenock.

1. "Arracan," sailing, 12 years, A1, A.M., 11 years old.
2. Cowpen coals; Howden dock, September 10th, 1873; North Shields.
3. Bombay.
4. February 16th, 1874; latitude 3° 40′ N., longitude 67° E.
5. Oak, Hackmatack, and pitch pine, all copper fastened.
6. Half an inch per hour.
7. 1,010 and 1,040 tons.
8. 1,441.
9. Cowpen coals.
10. Joseph Fothergill.
11. Six openings of two planks from beam to beam in between decks for the lower hold, two built ventilators in the house each end for the deck, and two hatches, one each end.
12. Supposed to be about fore rigging, starboard side, below the betwixt deck.
13. Five months and seven days.
14. Can't tell.
15. Waggon, 27 feet run, 26 feet fall (60 to 70 feet).
16. Fair; fair and rain.
17. Dry; dry during voyage.
18. Smell rising and hot in the fore hold on 14th February 1874; battened down. 16th; she fired two days.
19. Felt heat forward four months out.
20. Ship very open in the hold; could crawl fore and aft.
21. Half of the voyage; no water known to go down.
22. Battened down; three times burst open; drown out by water.
23. Two pumps for deck cleaning.
24. Damp in the small coal in the hatchway.

"INDIAN EMPIRE.

From Mr. George Duncan.

1. "Indian Empire," *A1, built 1865, sailing ship.
2. Shields, 27th September 1873.
3. Bombay, calling at Galle.
4. Off Cochin, 11th January 1874.
5. Iron.
6. None.
7. 1,515 nett.
8. 1,977.

9. Howard's West Hartley, best quality, steam, double screened.

10. J. R. Liddell.

11. Longitudinal shaft on each side keelson, and upshafts connected therewith coming up out of each hatchway.

12. About 8 feet aft the quarter hatch, in lower hold.

13. 102 days.

14. Cannot say.

15. Loaded at a spout in Northumberland dock, Tyne; ran down spout about 20 feet long.

16. Nothing particular as to wet while loading; fine during voyage.

17. Cannot say; believed in ordinary state.

18. Warmth felt in ventilators on deck three days previously, and subsequently a yellow smoke came up shafts.

19. No.

20. No; a considerable space in 'tween decks and at the ends of ship.

21. It is a standing order for hatches to be off during good weather; believe no water went down.

22. Hatches battened down till fire burst out; then ship was beached and scuttled.

23. Fire engine and head pump.

24. Coals shipped too soon after being dug, and not sufficiently screened.

"MOORESFOOT."

From Messrs. W. & R. Wright.

1. "Mooresfoot," A1, 10 years, sailing.

2. December 1873, Shields.

3. Bombay.

4. 10th March 1874; latitude 27° 30′ S., longitude 73° 30′ E.

5. Iron.

6. Perfectly tight.

7. 1,183.

8. 1,664 tons 4 cwt.

9. Howard's West Hartley.

10. Livingston, Briggs, & Co.

11. Ventilated by venetianed shafts up each hatch and along each side of her large box keelson; wood.

12. Supposed to be aft.

13. Three months.

14. Unknown.

15. By tip; height of fall unknown.

16. Unknown.

17. Unknown.

18. "Something was discovered coming up the fore hatch "at times having the appearance of smoke and accom-"panied by a strong smell of gas." As quoted.

19. Unknown.

20. No.

21. The captain has left no record.

22. Dug into the cargo to find out the seat of fire, battened down hatches, and closed ventilators.

23. Fire engine, hose, and buckets.

24. Have no idea.

"JOHN."

From Captain John Frostick.

1. Barque "John" sailing vessel A1, age 13 years.

2. September 12th, 1873, Porthcawl.

3. Valparaiso.

4. Seventy miles south from Cape Horn.

5. Wood.

6. No.

7. 367.

8. 564.

9. Abbw Smelting Coal unscreened.

10. Henry Bath & Sons of Swansea.

11. Hold ventilated with two half-inch boards on each side of stanchions from main keelson to deck beams.

12. In after hold.

13. 102 days.

14. About 24 hours.

15. Waggon. I should think the coals had to fall 30 feet.

16. Very wet, fine until off Cape Horn.

17. In after hold very wet, in fore hold moderate.

18. By gas and smoke in forecastle 60 hours before the flames burst through all.

20. No quite.

21. Every day when the weather was fine enough. No water got down.

22. All combustible matter taken on deck; all hatches battened down; four feet of water let into hold and ship put before the wind for the nearest port.

23. None.

24. The cargo being put on board in a wet state.

"ANNA."

From Captain P. F. Van Overklift.

1. "Anna" of Rotterdam first-class 12 years old sailing ship.

2. Cardiff, May 2nd, 1874.

3. Singapore.

4. 1st July 1874. 37° 14′ lat. S., 21° 25′ long. E.

5. Wood.

6. None.

7. 969 register tons.

8. 1,420 tons.

9. Insole's Merthyr steam coals, double screened.

10. Hypolite Worms, Esq., Cardiff.

11. Yes, with wooden pipes along the keelson and vertical ones in each hatchway (3). Two others from main deck to 'tween decks. Hatchway kept constantly open except during rainy or bad weather.

12. First about 9 feet down from surface of coals in the main hatchway. Afterwards about 15 feet from the deck or three feet from the bottom.

13. About 60 days.

14. Cannot say.

15. From waggons; there was but little fall for the coals.

16. Very fine and dry.

17. Coal appeared dry and was very dusty.

18. There was no fire but the coal became gradually very hot; temperature on deck 55° Farenheit, below 110° Farenheit.

19. Yes, twice daily every time the temperature was higher. An iron bar was forced down among the coal, this became so hot as to blister the hand when withdrawn.

20. No; forward and aft there was much space, in the middle of the ship the coal reached nearly to the decks.

21. Hatches open nearly all the voyage, only closed for rain or bad weather, hatchway in cabin always open.

22. I threw overboard about 300 tons of coal 38° S. lat. 1 East long. twenty days after (20) in 33° S. long. 78° E. long.; bad weather caused the closing of the hatchways for everal days during the time; the coal heated again but radually cooled down.

23. A force pump complete with hose always ready for use but not required.

24. To the peculiar character of the coal; I have carried coals several tons before but never met with a similar case.

"EUXINE."

From Mr. E. Bates.

1. "Euxine," iron sailing vessel, A1 20 years, built 1847, rebuilt by Laird 1869.

2. Loaded Newcastle, 12 June 1874.

3. Aden.

4. Abandoned on fire, 8th August 1874, about 1,500 miles south of St. Helena.

5. Iron.

6. None.

7. 1,594 tons.

8. 2,061¾ tons.

9. Howard's West Hartley.

10. Livingston, Briggs & Co.

11. Wooden additional ventilation. Iron permanent ventilation.

12. Supposed main-hold under main-hatch.

13. A few days over two months.

14. Unknown; believed a short time, waggons coming direct from pit.

15. Tipped along a shoot into the hold.

16. Fine.

17. Unknown.

18. In gale, 1st August, coals had shifted; while crew were engaged trimming, found great heat about main hatch; 4th Aug. ship trimmed upright; 5th Aug. coals smelling strong of gas, and smoke coming up main hatch.

19. Not known.

20. No, a space under each of three decks for ventilation, and on upper deck but little coal stowed.

22. Water pumped into hold continuously after fire discovered; 50 tons hot coals thrown overboard; coals afterwards so hot, hatches battened down; 6th Aug. hatches blew off; gas and smoke coming up ventilators.

"OLIVER CROMWELL," "CALCUTTA."

From Mr. Wm. R. Price, London.

1. "Oliver Cromwell" and "Calcutta," both colonial-built sailing ships, 1,112 tons and 1,372 tons nett register respectively, aged respectively eight and 10 years, both A1 at Lloyd's.

2. "Oliver Cromwell" sailed 18th June 1874. "Calcutta" sailed 2nd July 1874, both loaded at South Shields.

3. Of both, Aden.

4. "Oliver Cromwell," 6th September 1874, lat. 37° S., long. 14° E. "Calcutta," 17th September 1874, lat. 35° S., long. 7° E.

5. Both ships constructed of wood.

6. Both vessels were perfectly watertight, and made no water.

7. "Calcutta" (under deck), 1,261 tons, gross 1,372 tons. "Oliver Cromwell" (under deck), 1,013 tons, gross, 1,112 tons.

8. "Calcutta," 1,802 tons, "Oliver Cromwell," 1,616 tons.

9. B/L stated coal to be Howard's West Hartley. Cannot give any information as to the seam or whether double or single screen.

10. Charterers of ships were Mesrs. Livingstone, Briggs, & Co. on account of the Peninsular and Oriental Company.

11. Mode of ventilation described in clause 3. Material wood.

12. Fire in each case found to be in lower hold under main hatch.

13. About 85 and 82 days respectively.

14. Cannot say positively, but believe it was fresh from the pit.

15. By tips in Northumberland Dock, South Shields. Cannot state exact height, but it was considerable.

16. Fine.

17. What I saw on board the "Oliver Cromwell" was quite dry.

18. Please refer to clause 2.

19. The temperature was not, I believe, taken critically, but one of the crew of the "Oliver Cromwell" informs me that during the voyage he was frequently down in the fore and aft peak, and did not detect any unusual heat or vapour.

20. The hold was not completely filled in either ship, but the cargo was stowed high up to the main hatch, the ends being more open, but there was an unobstructed space from top of coals to deck the entire length of the ship.

21. The captain of the "Oliver Cromwell" stated to me that the first thing each morning, when he went on deck, he had the hatches taken off when the weather permitted, which it generally did. I did not learn that any water ever got down into the hold.

22. Water was poured down the hold, wet sails were put across the hatches and everything done to stop ventilation as much as possible.

23. Good fire-engine and plenty of hose in each case.

24. Speaking without any technical knowledge on the subject, I attribute these losses entirely to the nature of the coal. I may add that during an experience of about 25 years as an insurance broker I have largely insured coal cargoes to all parts of the world; with the exception of these two, I only had one case of burning, viz., the "Sir George Seymour," in 1867, from Birkenhead to Bombay, but in 1871, I effected insurances on the ships "Baringa" and "Sea Pink," the former an iron ship, the latter a wooden; both loaded from the Tyne for Bombay, neither of which were ever heard of, and as both were well found and well owned vessels, I have always considered that their loss was attributable to combustion rather than any other cause.

My experience, both as a ship owner and as shipping coal on my own account, is confined entirely to steam coal.

"EETA."

From Messrs. W. & J. Smith, London.

1. "Eeta," A1, 13 years, built 1874, sailing.

2. London, sailed 8th June 1874.

3. New Amsterdam, Berbice.

4. About 20th July, only a day or two before arrival in port, which she reached 22nd July.

5. Wood.

6. Not any.

7. 332 register.

8. 253 tons 7 cwt.

9.

Swadlincote	106	3
Baddersley	50	14
Babbington	96	10
	253	7

11. By a metal cowl ventilator forward and aft and screw ventilator in the bollards.

12. We think about midships, in the centre of the coals.

13. About 38 days.

14. Six to ten days.

15. From basket out of craft; they could only fall the depth of the hold.

20. No.

21. We had a very careful master and feel confident no water could have got down, and no general cargo was damaged, which must have been the case as the whole of the upper part was general.

22. General cargo was removed to give access to coals and water thrown down; this not proving effectual, we understand a hole or holes were bored in her to flood the floor and preserve the keelson, and everything done to smother it; the water washing in the hold greatly reduced it, and on arrival labour was obtained to discharge the upper cargo to put water on and pump, but the fire increasing she was taken on a mud bank and scuttled sufficiently to extinguish it. In this case only the coals were damaged, the ship and general cargo being saved.

"ALEXANDRE."

From Messrs. Agacio & Co., Liverpool.

1. French sailing ship "Alexandre," built at Glasgow, in 1869.

2. 23rd June 1874, Liverpool.

3. Valparaiso.

4. Took fire at sea, near Montevideo, and was scuttled to extinguish the fire.

5. Iron.

6. No.

8. 1,135.

9. Lancashire best orrell steam coal, double screened, bought from Messrs. W. H. Brancker & Co., Oldhall Street, Liverpool.

10. Ourselves.

11. The ships' hold was ventilated by means of a wooden case parallel to the keelson from fore to aft, 15-inch square, bored with holes and connected with three perpendicular similar cases projecting 4 feet over the hatches when shut.

12. Do not know.

13. She began to load in June 1874.

14. The coal was fresh wrought, but cannot state time before being put on board.

15. Loaded from waggons at the High Level Bramley Moore Dock.

16. Do not know.

17. Do not know.

18. Do not know.

19. In another ship, the "Coquimbo," the cargo was so heated that on her arrival at Callao, in March of this year, the captain found 40°/₀ of heat in the ship's hold.

20. Not completely.

21. Do not know.

22. Do not know.

23. Do not know.

24. Do not know.

"BRENHILDA."

From Captain Scurr.

1. "Brenhilda," A1, first voyage. Sailing.

2. 23rd June 1874. Glasgow.

3. Calcutta.

4. I cannot state to date exactly, on two different occasions I found the coals heated, smoking and smelling strong of gas. I turned them over and removed about 20 tons until all heat and gas subsided.

5. Iron.

6. No.

7. 1,321 tons.
8. 1,776 tons.
10. Watson.
11. Yes, one ventilator each hatch from upper deck to keelson, four large ventilators from upper deck to 'tween decks.
12. Fore hatch in lower hold.
13. About seven weeks.
14. I believe the coal came direct from the pit.
15. Waggon, by steam crane, about 5 feet from deck.
16. Very fine.
17. Dry.
18. Yes, refer to 4.
19. No, I examined the fore and after hatch daily, the main hatch never being unbattened.
20. No; from fore part of the fore hatch clear, and the after part of the after hatch 'tween deck hatches, all off; about 600 tons in 'tween decks.
21. Fore hatch open in fine weather, after hatch flap always open, and lee side of ventilators.
22. When heated turned them over, when fire broke out pumped water down.
23. Steam donkey pump, head pump, and fire engine, or force pump.
24. By coals coming direct from the pits.

The following letter was received by the owners from Captain Scurr:—

Gentlemen,

I beg to state that fire broke out 6th November in Calcutta in the fore hatch where I had turned the coals over on my passage out. I applied for the steam floating engine, and the port conservator sent it alongside at once, and put six jets on board, they putting 3 feet 6 inches water in the ship; in half hour all smoke and gas ceasing, I stopped pumping. At the time I should say I had discharged 500 tons of coal out of the main and after hatch. On getting down to the heated coals I found about 15 cwt. to a ton like a cinder or rather resembling coke. I have carried one cargo of coals out to Bombay from Sunderland, year 1866; 1,800 tons in the ship "Thomdean" with no ventilators, and never opened the main or after hatch, the fore hatch seldom; making a long passage of 130 days.

In 1853 I was seaman on board the ship "Hannah," from London to San Francisco with coals, no ventilators, the coals being six months on board and never heated.

John Scurr.

"ADMIRAL FITZROY."

From Mr. J. E. Burgess, Swansea.

1. "Admiral Fitzroy," 13 years, A1 10 years, sailing.
2. July 1874, Cardiff.
3. Valparaiso.
4. September 22nd, 1874, in lat. 53° 20′ S., long. 63° 8′ W.
5. Wood.
6. Very little.
7. 377 nett.
8. 525.
9. Insole's No. 3, Rhondda, worked from Cymmer Colliery, and from No. 3 Rhondda Seam, shipped through and through as worked.
10. Hy. Bath and Sons.
11. Main body of the cargo, say from abaft side mainmast to foremast, was divided by running deals up on either side of hold, stanchions from keelson to main deck, and left open at top.
12. Blew up all the main decks from fore part of main hatches to the poop; this took place at noon, and at 1 p.m. same day flames were coming up through the hatch.
13. Ten weeks.
14. A few hours.
15. By waggon; the first 120 tons would fall the depth of the ship, the balance would have only a few feet to fall.
16. Wet.
17. Slightly wet.
18. At 2 p.m. of the 21st September, a quantity of smoke was observed coming up the fore hatch, and on examination coals were found to be very hot and impossible to stop below. At 4 p.m. fore hatch was battened down, the cargo being evidently on fire; at noon of the 22nd inst. an explosion took place, which blew the main decks out from fore part of main hatch to the poop, and at 1 p.m. flames were coming up through the hatch.
20. Yes.
23. Not any.
24. I am unable to suggest any cause for this catastrophe, for the coals were shipped as No. 3 Rhondda, which is reported to be the purest coal in South Wales, and is entirely free from sulphur, whereas spontaneous combustion in coal is, I believe, usually attributed to water and iron pyrites, the unison of which gives rise to intense heat, something on the principle of slaked lime; if this was the cause of combustion in this instance, the coal must have been mixed, which may probably have been done (but now difficult to ascertain), as coals were very scarce at the time, and the vessel was on demurrage.

"DUMBARTONSHIRE."

From Mr. Thomas Law.

1. "Dumbartonshire," 100 A1., new, sailing ship.
2. June 1874, Glasgow.
3. Buenos Ayres.
4. 6th September 1874, at Buenos Ayres.
5. Iron.
6. Made no water.
7. 915 and 972.
8. 540 tons.
9. Splint. Russell's Colliery.
10. Owner.
11. Was only ventilated near main mast, but the mooring pipes acted as ventilators also.
12. Fore part.
13. About two months.
15. Was loaded by wheelbarrows.
16. Good weather both loading and during the voyage.
18. About 24 hours, by great heat.
19. No.
20. No. Ship had a general cargo.
21. Hatches were not opened often.
22. First poured water on coal, and then scuttled the ship.
23. Fire engine.
24. Cannot say.

"INDUSTRY."

From Mr. R. Robbins, Kenilworth.

1. "Industry," 5/6 L. 1. 1. in veritas, built in 1858; sailing.
2. July 1874, North Shields.
3. Bombay.
4. 19th October 1874, lat. 33° 47′ S., long. 52° 47′ E.
5. Wood.
6. None.
7. 1,138 tons.
8. 1,475 tons.
9. Davison's West Hartley Steam Coal, wrought out of Bidlington's collieries, most probably all from Low Main seam; ordinary single screened, as customary in the district.
10. Hickie, Borman, & Co. London.
11. Ventilator fore and aft, the ship's keelson and shaft through each hatch leading into it; the fore and aft hatches were never on, and ventilation well looked after.
12. After hatch.
13. Between three and four months.
14. They were drawn out of the earth and put at once into the ship.
15. From waggon by spout with a fall of about 24 feet when commencing to load.
16. Fine and dry, nothing of note occurred, no heavy seas got down the hatches.
17. The coals when shipped looked exceedingly well; were large, good, and dry.

Gas in cabin, and smoke from aft hatch two days before vessel was abandoned.

20. The ship was not full excepting in the main hatch, room at each end for about 90 tons.
21. See answer to questions Nos. 11 and 16.
22. All hatches opened.
23. None.
24. To the too rapid transition of the coal from the bowels of the earth to the ship's hold.

"MINDORA."

From Messrs. Stoddart Bros., Liverpool.

1. "Mindora," A1 at Lloyd's, built 1865, sailing ship.
2. Birkenhead, July 1874.
3. Bombay.

4. Took fire first on 16th October 1874, then on 29th November, and again 2nd December.
5. Wood.
6. None.
7. 971 tons register.
8. 1,416 tons.
9. Coal purchased from Robert James & Co., coal merchants, 3, George's Dock Gates, Liverpool, to whom refer.
10. Messrs. Edward Lawrence & Co., Liverpool.
11. Yes; a trunk on each side of the keelson the length of the hold and connected with it, a trunk up to each hatch and reaching above the deck; open at top; all of wood.
12. Main and after hatch.
13. About two months and a half.
14. Refer to Robert James & Co., George Dock Gate, Liverpool.
15. By the hydraulic tip at Birkenhead. The waggons are lifted by the tip and brought over ship's hatch. The greatest fall for coal would be about 10 to 12 feet. Ballast coal (350 tons) previously lowered into vessel's lower hold.
16. Weather fine when loading, and fine generally on the voyage.
17. Cannot say.
18. Early in October. Coals were hot.
19. Cannot say; captain is still abroad in the ship.
20. In the centre she was, and gradually tapered off to both ends.
21. Cannot tell; captain is still in India with the ship.
22. Captain does not state what he did at sea. On arrival at Bombay, on 2nd December, the water boat was sent to the ship and pumped water on the coal.
23. Fire engine.
24. Cannot determine any particular cause.

"STAFFORDSHIRE," "MONMOUTHSHIRE."

From Mr. D. Stuart.

1. "Staffordshire" and "Monmouthshire," A1, 11 years, sailing.
2. 1874, Liverpool.
3. San Francisco.
4. In both cases about 70 days out.
5. Iron.
6. None.
7. 1,172 and 1,167 tons respectively.
8. "Staffordshire," 814 tons; "Monmouthshire," 1,500 tons.
9. Lancashire steam coal, White Moss coal; single screened.
10. J. G. & W. Barnes.
11. Yes; the ordinary mode; wood.
12. Lower hold.
13. About 100 days.
14. A very short time, to the best of my knowledge.
15. From the high level. The fall was small.
16. Very fine.
17. Quite dry.
18. Only a few days.
19. I am unable to answer.
20. In the second case, yes; in the first, no.
21. I do not know.
22. In the first case the ship was taken to the nearest port and scuttled. In the second case the fire was promptly extinguished and a small jettison effected.
23. Ordinary fire-engines.
24. The newness of the coal solely.

"GLEN TILT."

From Messrs. Hughes & Co., Menai Bridge.

1. Sailing ship, "Glen Tilt," A1, built 1865.
2. July 30th 1874, Cardiff.
3. Coquimbo (Chili).
4. October 28th, 1874, off Cape Horn.
5. Wood.
6. Little or none.
7. 991 tons net.
8. 610 tons coal, 591 tons coke.
9. Non-screened smelting coal, Pen-y-graig colliery.
10. Pen-y-graig Coal Company.
11. Ventilated with platform under cargo.
12. Between the after part of fore hatch and after part of main hatch.
13. From date of leaving Cardiff, 30th July, to arrival at Port Stanley, Falkland Islands (to which place ship put back to), 11th November.
14. Cannot say.
15. Depth of hold 21 feet; it slid down the shoot to combings of hatches.
16. Very fine dry weather.
17. Very moist.
18. None that we are aware of.
19. Cannot tell.
20. Not quite; large space forward, coal being in *the bottom* and coke on the top.
21. We believe hatches were not opened, nor did any water get down.
22. Cargo was got up to get at the fire, but it became too hot to work, and then the hatches were battened and well caulked. Ultimately the decks got very much heated, and holes were then bored in the decks and waterways to pour down water.
23. Pumps.
24. To gas, inferior quality, and damp state of the coals at time of shipment.

"ONTARIO."

From Messrs. John Vaughan & Co., Liverpool.

1. "Ontario," 3/3 to 1.1., Veritas; built 1861. Sailing vessel.
2. 1 to 7 of August 1874, at Tyne.
3. Bombay.
4. 22 January 1875, at sea.
5. Wood.
6. Perfectly tight.
7. Her net register tonnage is 1,062 tons.
8. About 1,500 tons coals, and 100 tons coke.
9. West Hartley Main coals.
10. Livingstone, Briggs & Co., London.
11. Was well ventilated, a ventilator in each hatch, and along each side of the keelson under the coals.
12. Main hatch.
13. About 5½ months.
14. Unknown to us.
15. The vessel loaded from tips, not from waggons.
16. Fine, with occasional showers; weather during voyage unknown to us.
17. Unknown.
18. No indication given, until volumes of smoke issued out of the ventilators.
19. No.
20. No.
21. Unknown to us.
22. Water was poured down, and the vessel run ashore and scuttled.
23. Fire-engine.

"NORTHBROOK."

From Messrs. Adamson and Ronaldson, London.

1. "Northbrook." When new, 100 A 1. Sailing ship.
2. August 1874. The Tyne.
3. Madras.
4. December 1874, Madras Roads.
5. Iron.
6. None.
7. 1,820 tons net, 1,876 gross.
8. 1,656 tons coal and 866 coke.
9. Not known.
10. James Burgess and Sons.
11. By iron ventilators, and a free current of air through the hold.
12. After hold, immediately under hatch.
13. 120 days.
14. Not known.
15. From waggons; about 30 feet.
16. Changeable.
17. Slightly damp.
18. None.
19. Occasionally.
20. No.
21. Closed only during very bad weather or much rain; a small portion of water may have got below.
22. The hatches were immediately closed, and a hose supplied by a steam pump, pointed to the seat of fire, and a constant supply of water kept up.
23. Steam pump.
24. Damp.

"NEW LAMPEDO."

From Messrs. Seed, Armstrong, & Co. Liverpool.

1. "New Lampedo" 3·3, 11 Veritas 5 years, age about 10 years, sailing ship.
2. 10th August, 1874. Liverpool.
3. Panama.
4. Abandoned 16th November off Cape Horn.
5. Spruce and hackmatack with birch floors.
6. Made no water.
7. Nett 1,099, gross about 1,127.
8. 1,499 tons.
9. Don't know.
10. J. G. & W. Barnes.
11. Her San Francisco grain lining was left in up to her 'tween decks which was Oregon pine; deck ventilators fore and aft.
12. It was supposed the fire originated in the main hatch low down.
13. Three months.
14. Don't know.
15. From the height of her decks.
16. Very wet weather at the time of loading.
17. Coal very moist when taken on board.
19. No.
20. No.
21. Hatches open at all times when possible; no water got into hold.
22. Battened down when there was indication of fire.
23. None.
24. Cannot tell.

"ENTHUSIAST."

From Messrs. W. & J. Smith, London.

1. "Enthusiast" A 1, built 1854, sailing ship.
2. Sailed from London, about 11th August 1874.
3. Demerara.
4. Abandoned at sea, 14th September, on fire.
5. Wood.
7. 283 register.
8. 114 tons.
9. Swadlincote, single screened.
13. She was abandoned on fire, 14th September, shipped about 21st or 22nd July, say 53 days.
14. 6 to 10 days.
15. Out of basket from craft.
20. No, only a part of her general cargo.

"LA ESCOCESA."

From Messrs. Balfour, Williams, & Co. Liverpool.

1. Sailing ship "La Escocesa"
2. Cardiff, September 1874.
3. Valparaiso and Payta.
4. Five days previous to her arrival at Payta.
5. Iron.
6. No.
7. 946 net, 988 gross.
8. 1400 tons.
9. South Wales shipped at Cardiff; colliery, not known.
11. Yes, with fore and aft along keelson and three vertical ventilators as per sketch attached.
12. Amidships.
13. 130 days.
14. Very short time.
15. From coal tips at Cardiff.
16. Raining heavily.
18. Smouldering five days previous to the arrival; after the arrival the hatches were opened, 400 hundred tons discharged previous to actual fire.
19. Not known, captain not yet arrived home.
20. No.
21. Same reply as to No. 19.
22. Closed all ventilation when fire actually broke out, ship was scuttled by crew of ship with assistance of one of Her Majesty's frigates.
23. Fire-engine, hose and buckets.
24. Coals green and weather wet while loading.

"TRY."

From Messrs. W. and J. Smith, London.

1. "Try," A1, built 1866. Sailing.
2. London, sailed 4th September 1874.
3. Georgetown, Demerara.
4. 13th October, at sea; smoke found coming up fore hatch.
5. Wood.
6. There is no report of it in the protest.
7. 428.
8. 107 tons 15 cwt.
9. 57 13 Swadlincote.
   50 2 Hockley Hall.
   107 15
11. Being a sugar trader we think so, but do not know of our own knowledge.
12. Fore hold.
13. About 60 days.
14. Six to 10 days.
15. By basket from craft.
18. Not any.
20. No, only a small part of the cargo.
22. The cargo was got on deck and water thrown down until the fire was subdued.

"CRYSTAL."

From Messrs. W. and J. Smith, London.

1. "Crystal," A1 10 years, built 1872. Sailing ship.
2. 5th September 1874, from London.
3. Berbice River (New Amsterdam).
4. In the Berbice River; she arrived there 23rd October, and after discharging nearly all general cargo the coals were found on fire, we think it was about 31st October.
5. Wood.
6. Not any.
7. 263.
8. 239 tons 5 cwt.
9. 102 tons 11 cwt. Hockley Hall, 136 tons 14 cwt. Swadlincote; as to seam, &c.
11. By two cowl metal ventilators, one forward and one aft, and the after hatch was frequently off we believe in fine weather.
12. Near the foremast and within a few feet of the ventilator.
13. Under 80 days.
14. Six to 10 days.
15. By baskets from lighters. They fall only from deck to bottom for first few tons, and afterwards on the pile and run down.
18. Not any indication.
19. It was not taken as far as we know, but nothing excited attention during the voyage or discharge of the cargo stowed over the coals.
20. No.
21. We understand the weather was fine and we are quite confident no water would be allowed to enter; all the top was general cargo and she has an old, most experienced, and careful captain.
22. Immediately it was discovered it was subdued by water being thrown on the spot on fire, this operation being of course a simple matter in consequence of all the other cargo being out, and ship in port.

"KHERSONESE."

From Mr. George Duncan.

1. "Khersonese," 100 A1, built 1855, sailing ship.
2. Shields, 19th September 1874.
3. Bombay, calling at Galle.
4. Bombay, 20th February 1875.
5. Iron.
6. None.
7. 1,811 gross, 1,711 nett.
8. 2,022 tons.
9. Bebside West Hartley steam, best quality, double screened.
10. James Walton.
11. Same as stated in the "Indian Empire" case.
12. In the way of the main hatch, about half way down.
13. 154 days.

14. Cannot say; think some of it a very short time, say not more than 12 hours.
15. Loaded from waggon, coal falling 15 to 20 feet.
16. Weather showery during shipment, not seriously wet; rough passage out of the Channel, afterwards fine weather.
17. Cannot say; believed in ordinary state.
18. Fire broke out three days after commencing to discharge cargo; no previous indication.
19. No.
20. No; a considerable space in the three holds and at ends of ship.
21. It is a standing order for hatches to be off during good weather; believe no water went down.
22. Floating fire-engine brought alongside, and fire extinguished without damage to ship.
23. Fire-engine and head pump.
24. Coals shipped too soon after being wrought, and not sufficiently screened.

"THE FOUNDLING."

From Mr. E. Bates.

1. "The Foundling," A1, 20 years, and AA1, built 1866 iron sailing vessel.
2. Birkenhead, 24th September 1874.
3. Bombay.
4. 21st November 1874.
5. Iron.
6. None.
7. 1,186 tons.
8. Mixed cargo, 100 tons iron and 100 tons bricks, 200 tons coke; 795 tons Westminster Brymbo coals, 306 tons Oaks Pits coals.
9. North Wales Main steam coals; 795 tons from Westminster Brymbo Coal and Iron Company's Colliery, and 306 tons from Oaks Pits Colliery, Mold.
10. Edward Bates & Sons.
11. Yes; in addition to permanent iron ventilators a wooden shaft through the coals and down each hatchway.
12. Supposed in main hold under main hatch.
13. About two months.
14. Unknown; probably a week.
15. By tips; coals run down a shoot into the hold, falling the height from deck to coals.
16. Wet.
18. Smoke observed issuing from main ventilator on 19th November; hold full of smoke, 20th November; flames bursting out, 21st November.
19. Believe not.
20. No; ship was not full; large space over coals in lower hold.
21. No water got down; unknown time hatches were off.
22. 19th November, took hatches off, pumped water into hold for 2½ hours, then battened hatches down.
23. None.
24. Unknown.

"LEVANT."

From Mr. John Jones, Milford.

1. "Levant" class A1, 12 years, 9 years old last April, sailing.
2. September 1874, Swansea.
3. Valparaiso for orders.
4. 8th of January 1875. Lat. 60° South, long. 73° West.
5. Oak teak and green heart.
6. Did not make water.
7. 314 tons register.
8. 470 tons of coal, 10 tons of powder, 1 ton of fusees.
9. Smelting coal for copper works, do not know the name of the colliery, not screened.
10. Messrs. Elford, Williams, & Co., Swansea.
11. With three boxes one in each hatch, made of wood.
12. Seven feet before the foreside of the main hatch, and at the bottom of the vessel.
13. About 100 days.
14. Do not know.
15. From waggons, about 20 feet fall.
16. Partly wet, latter part dry; generally strong weather.
17. Did not observe.
18. Captain has left the ship, do not know.
19. Do not know.
20. Within four feet of the deck.
2., Do not know.
22. Threw the 10 tons of powder and 1 ton of fusees overboard and battened all down.
23. None, except buckets to pour water on the coal.
24. Do not know.

"CITY OF RICHMOND."

From Mr. Donald Kennedy, Liverpool.

1. "City of Richmond," 3/3 Veritas; 10 years old. Sailing ship.
2. Birkenhead, October 1874.
3. Bombay.
4. 9th March 1875, Bombay; combustion.
5. Wood.
6. None.
7. 1,284 total, 1,241 nett register.
8. 1,638.
9. Laird's West Hartley seam, to be double screened.
10. Messrs. Busaby and Wichelhaus, from Liverpool.
11. 3 in. deal (spruce) platform, with wooden ventilators up each hatch.
12. Believe about main hatch.
13. 4½ months.
14. A few days.
15. Tip.
16. Ordinary weather for season of year.
17. Ordinary state.
18. Found to be very much heated during discharge.
19. Don't know.
20. No.
21. Hatches open all fair weather; no water got down.
22. Water poured down.
23. Usual (ship) fire-engine.
24. Don't know.

"FEDALMA."

From Mr. J. Hamlyn (Master), by Messrs. Scrutton & Co., Shipowners.

1. "Fedalma," A1, 4½ years, sailing ship, barque.
2. 12th October 1874, Porthcawl, Wales.
3. "Valparaiso" for orders, ordered to Chanaral.
4. 6th February 1875, at Chanaral.
5. Iron.
6. Made no water.
7. Nett $478\frac{48}{100}$, gross $507\frac{41}{100}$.
8. 654 tons.
9. Smelting, from the "Aber Coal Company," not screened.
10. Messrs. W. Nicholson & Sons, Sunderland.
11. The ship was ventilated, fore and aft, by stantions from deck to keelson and 1½ boards nailed to them, thus leaving a space of 6 in. broad from top to bottom, and dividing the whole cargo in the middle.
12. On the starboard wing, from the after part of main hatch, and extended 20 feet aft, also from ceiling 4 feet up, the coals on fire being about 4 feet deep.
13. 117 days.
14. I do not know for certain, but was told they came direct from the pit.
15. The coals were shot from the wagon into a shute of about 20 ft. in length, and thus conducted to the combings of the hatchway; the depth of the ship's hold 16 feet 4 inches.
16. Fine and dry during the time of loading, not much rain during the passage.
17. Very dry when shipped and quite dry during the voyage, that is, having only the usual moisture in the dryed coals, viz., about 3 per cent.
18. The coals were warm just about 2 feet under the surface when we commenced to discharge, and when we got down about 4 ft. it became quite cool in all parts except the starboard wing, which got hotter the farther we got down, and the first indication was a jet of white steam, with the smell of paraffin oil which came from under the stringer.
19. Only by going into the hold and feeling with the hand, and this was done every day the hatches were taken off.
20. No, only about two-thirds full, falling from 2 feet 6 inches below the main hatch to nothing fore and aft.
21. Some part of almost every day; and I do not believe a bucket full of water went into the hold.
22. To discharge the coals as quick as possible, working all hatches to cut the fire off from going further aft.
23. Plenty of fire buckets.

CASES OF EXPLOSION.

THE FOLLOWING PARTICULARS OF CASES OF EXPLOSION OF GAS ON BOARD COAL-LADEN SHIPS HAVE BEEN SENT IN ANSWER TO THE FOREGOING QUESTIONS :—

"MEUSE."

From Messrs. N. PAQUET & Co., Marseilles.

1. "Meuse," built in 1872, steamer.
2. The 10th or 14th August 1874.
3. Port Said.
4. 23rd August, about 10 miles north-west of Cape Bengut Algiers.
5. Iron.
6. Did not make water.
7. $\frac{1234}{496}$.
8. 1,100 tons.
9. Invoice, stated, Wood's, Merthyr, large.
10. H. Worms & Co.
11. Not ventilated.
12. In the fore hold.
13. About eight days.
14. We do not know, probably not long, as the gas still existed.
15. By the ordinary method in use at Cardiff, from wagons which are lowered to the hatches.
16. Bad weather till we entered the Mediterranean, with fine weather from then till the time of the explosion.
17. Not known.
18. None.
19. Temperature was not taken.
20. There was a considerable space below the spar deck.
21. The hatches had not been off until such time that the explosion took place. It was said that the hatches were taken off the evening before but this was not well confirmed.
22. The heating of the coals was not detected until the explosion occurred, which caused us to return to Algiers, where the coals were discharged without further damage.
23. Nothing special beyond the pumps with which all steamers are supplied.
24. We believe that this explosion was caused by a man decending the hatchway with a naked light, and this man was the first victim of the explosion so caused. This was not a case of spontaneous combustion.

"HOLYROOD."

From Mr. E. C. DOWNING, Cardiff.

1. "Holyrood" steamship, of Limerick, A1, built in 1857.
2. Cardiff September 23rd, 1868.
3. Limerick.
4. September 23rd 1868 in main hold. Cardiff.
5. Iron.
6. None.
7. 296 net, 358 gross.
8. 585 tons.
9. Cardiff Merthyr steam coal, 414 tons from Ocean Collieries, 171 tons from D. Davis and Sons, Merthyr Collieries (through and through coal).
10. E. C. Downing.
11. Not ventilated at all.
12. Mainhold.
13. About 24 hours.
14. Probably not more than 24 hours, perhaps not so long.
15. Ex-waggons; the fall was very little, perhaps 2 or 3 feet.
16. Fine.
17. Dry when shipped.

19. No.

20. The hold was completely filled aft under the cabin, but there was room forward for 10 or 12 tons coal; the hatchway was quite filled up with coal. The cargo was shipped and then 71 tons coal for bunkers were tipped on the hatchway and wheeled to the bunkers; this occupied several hours; when done the men were about to clear the hatchway and one man went below to trim down the coal, another man handed down a *lighted* candle, when an explosion occurred damaging the steamer slightly, but seriously burning the two men.

24. To the want of ventilation and the exposure of a naked light.

"BATTALION."

From Mr. WM. LAING, Leith.

1. "Battalion." A1 two years. Steam.
2. 6th March 1863, Cardiff.
3. London.
4. 8th March 1863; channel; explosion.
5. Iron.
6. None.
7. 574–694.
8. 715 tons.
9. "Navigation" steam coal (late "Nixon's Duffryn").
10. Nicon, Taylor, and Cory.
11. Not ventilated.
12. Fore hold.
13. About four days.
14. Unknown.
15. Unknown.
16. Unknown.
17. Unknown.
18. No.
19. No.
20. No.
21. Unknown.
22. Unknown.
23. Nothing beyond engine pumps.
24. To the accumulation and concentration of gas in the fore hold, which was not half full, and contact with a naked light which a sailor carried down, in order to get the square-sail out of the hold.

"BROOMHAUGH."

From Messrs. ELLIOTT, LOWREY, and DUNFORD, Newcastle-on-Tyne.

1. "Broomhaugh;" 90 A1, 1872; steam.
2. September 1873 (sailed 14th), Cardiff.
3. Port Said.
4. 20th September 1873. Near Gibraltar.
5. Iron.
6. Ship made no water.
7. 1,352 tons; 865 tons.
8. 1,576.
9. Powell Duffryn steam coal; double screened.

11. Not ventilated.

12. Fore peak and fore hold.

(The explosion first occurred in the peak; the fire then penetrated to the fore hold, where there was also a vacant space, resulting in a second explosion there.)

13. Seven days.

14. A few hours.

15. From tips. Coals would run through a shoot for about 50 feet.

16. Very wet during loading; do. passage to Gibraltar.

17. Coals, considering the weather, would be much wet in transit from pit to ship.

18. None.

19. No.

20. None of the holds were full.

21. All the hatches off from loading till midnight of 14th. State of the weather would not permit of their being removed again until the explosion.

22. No indication until explosion.

23. Fire-hose and buckets.

24. September 1873.—All went well until the 20th of same month, when one of the crew having occasion to sound the water tank in the fore-peak, proceeded (having first provided himself with a lamp) to remove the hatch and descend into the peak.

Whilst standing over the hatchway with the lamp in his hand it was noticed by those in the vicinity that a blue light seemed to encircle the lamp, caused, no doubt, by the accumulated gas from the cargo in this unoccupied space; the gas becoming ignited burnt the man's hand, causing him to drop the lamp down the hatch. The effect was instantaneous. There was a loud explosion; the decks were blown up from the foremast to the forecastle; bulkheads blown down, and generally the fore part of the ship was made a complete wreck.

"THAMES."

From Mr. H. Cloake, Cardiff.

1. "Thames," steamship.

2. First explosion occurred May 1861 after loading at Swansea, second explosion was July same year from same port.

3. First voyage, Alicante; second voyage, Valencia.

4. First explosion occurred off or near Lisbon after being out about five days, severely burning chief engineer rendering him incapable of duty all voyage; second explosion occurred on leaving piers of Swansea and burning second engineer who had been ordered to take no light down in the tunnel or compartments.

5. Iron.

6. No.

7. 191 net, 302 gross.

8. 360 cargo and 60 bunkers.

9. First cargo, I believe, was Wayne's Merthyr single screened; second was a mixed cargo.

10. Cannot remember.

11. We had ventilators fitted in each hold, of the same description as those used for the stoke hole.

12. Both explosions occurred in the compartment aft, entrance to which was from the cabin down a trelliced hatch, but the gas had accumulated further aft. Engineer, who had been in boat some time, took a match down and struck light, but had been cautioned to use safety lamps.

13. First case about 7 days, second case not 24 hours.

14. I am under impression that coal was shipped soon after worked.

15. From trucks containing 8 to 10 tons each; the shute down which coal came from the trucks being on level with combings.

20. We had much space vacant in both holds.

21. On first occasion the hatches were occasionally off; on second occasion the hatches had not been on since loading. It is my opinion that often when hatches are off that much harm is done by salt water going down; being cargo coals it is supposed often that they cannot damage, but the wet during long voyages is the cause of most of the fires.

24. To accumulation of gas in the after end and a naked light being, used although against express orders and proper lamps being available.

"WILLIAM BATTERS."

From William Batters.

1. "William Batters." A. 90. New ship. Steam.

2. 16th and 17th September 1873; Cardiff.

3. St. Nazaire.

4. 18th September 1873. Penarth Roads just outside Cardiff.

5. Iron.

6. None.

7. 530 nett, 829 gross registered tonnage.

8. About 1,040.

9. Small coal collected from several collieries.

10. C. Goddard of Nantes and Cardiff.

11. No special ventilation, but the hatches were off at the time of the explosion.

12. In the fore part of the fore hold.

13. About 48 hours.

14. I could never ascertain, although I tried to do so at the request of underwriters.

15. From tips in the usual manner at Cardiff.

16. Fine.

17. Usual condition of small coal; did not present any difference as regards appearance.

18 and 19. The voyage had barely commenced.

20. Not at the part where the explosion originated.

21. See reply to 18th and 19th queries.

22. No such indications were perceptible.

23. A powerful donkey engine and fire hose.

24. There were two scuttles, alike in appearance and close together, one on the fore side of the forward bulkhead leading to the chain locker, and one on the after side leading to a store-room, taken out of the fore hold and constructed of wood; there is little doubt that the gas had passed through the joints and connexions of the wooden structure and accumulated in the store-room. The ship was weighing anchor at daybreak, a man went with a naked light to stow the cable in the chain locker, and instead of going down through the forward scuttle made a mistake, lifted the after scuttle, and the explosion was the result.

"J. H. LORENTZEN."

From H. Lorentzen.

1. "J. H. Lorentzen," steamship. 16 years at Liverpool. Age, built in 1872.

2. October 30, 1874, at Sunderland.

3. Hamburg.

4. South Dock, Sunderland, October 30th, 1874.

5. Iron.

6. No.

7. 567 net, 884 gross.

8. 1,056$\frac{4}{20}$ tons.

9. 611 tons Ryhope Hartley steam (best screened) and 445$\frac{4}{20}$ tons of Ryhope treble-screened nuts; out of Maudlin seam, Ryhope Colliery.

10. J. S. Barwick.

11. No.

12. In the fore hold where the steam coals were.

13. Just shipped.

14. A few hours.

15. From waggon into a spout that was level with the ship's hold.

16. Fine weather. Accident took place an hour after ship was loaded.

17. Dry.

18. No.

19. Ship had just commenced the voyage.

20. Very nearly.

21. See answer to No. 19.

22. None, because none were necessary as it was just an explosion that blew off the hatches and singed the paint a little.

23. Donkey pump.

24. Through the hatches being battened down too soon for coals that had just come out of the pit.

APPENDIX V.

STATISTICS prepared by LLOYDS especially for the Commission.

SHIPMENTS OF COAL, 1874.

TABLE showing the Number of COAL SHIPMENTS and the QUANTITIES SHIPPED from the UNITED KINGDOM to FOREIGN PORTS in 1874, and the NUMBER of CASUALTIES which occurred from SPONTANEOUS COMBUSTION.

(This Table was compiled by Lloyd's from Returns furnished by the Custom House Authorities at the various Ports of the United Kingdom.)

—	NUMBER OF SHIPMENTS.												QUANTITIES SHIPPED.					
	Cargoes under 500 Tons.	Casualties from Fire or Heat.	Cargoes 500 and under 1,000 Tons.	Casualties from Fire or Heat.	Cargoes 1,000 and under 1,500 Tons.	Casualties from Fire or Heat.	Cargoes 1,500 and under 2,000 Tons.	Casualties from Fire or Heat.	Cargoes 2,000 and upwards.	Casualties from Fire or Heat.	Total Number of Vessels.	Total Casualties from Fire or Heat.	Cargoes under 500 Tons.	Cargoes 500 and under 1,000 Tons.	Cargoes 1,000 and under 1,500 Tons.	Cargoes 1,500 and under 2,000 Tons.	Cargoes 2,000 Tons and upwards.	Total Quantity of shipped Tons.
Baltic - - -	4,993	1	903	1	304	—	40	—	—	—	6,240	2	1,310,392	602,871	352,152	64,663	—	2,330,078
Mediterranean - -	1,390	1	1,614	2	583	—	105	—	19	—	3,711	3	419,104	1,085,373	684,937	175,243	39,359	2,404,016
Black Sea - -	201	1	298	4	123	—	30	—	4	—	656	5	69,394	212,508	147,994	49,654	8,109	487,659
Other European ports -	13,113	—	2,241	—	666	—	4	—	—	—	16,024	—	2,776,766	1,790,323	717,921	6,418	—	5,291,428
Total, Europe -	19,697	3	5,056	7	1,676	—	179	—	23	—	26,631	10	4,575,656	3,691,075	1,903,004	295,978	47,468	10,513,181
North America - -	220	—	198	—	18	—	2	—	—	—	438	—	54,969	139,383	19,792	3,193	—	217,337
West Indies and Central America.	677	4	341	1	31	—	3	—	—	—	1,052	5	175,788	221,721	35,601	5,066	—	438,176
																		624,357
East Coast, South America	525	—	316	3	91	—	61	—	23	—	1,016	3	138,357	217,847	109,183	105,509	53,461	344,156
West Coast, South America	129	1	256	9	75	6	20	—	5	2	485	18	28,912	182,885	87,099	34,211	11,049	73,142
San Francisco - -	13	—	16	1	19	3	14	3	5	2	67	9	3,215	11,167	23,863	23,682	11,215	
Total, America -	1,564	5	1,127	14	234	9	100	3	33	4	3,058	35	401,241	773,003	275,538	171,661	75,725	1,697,168
Africa, outside Gibraltar -	321	—	95	—	11	—	5	—	—	—	432	—	51,132	69,446	13,111	7,554	—	141,243
Asia - - -	224	—	279	3	245	8	203	11	44	3	995	25	44,433	208,451	308,875	346,256	109,405	1,017,420
Total, to all ports -	21,806	8	6,557	24	2,166	17	487	14	100	7	31,116	70	5,072,462	4,741,975	2,500,528	821,449	232,598	13,369,012

COMBUSTION

TABLE

CASES of COMBUSTION previous to 1874, of which Particulars

Date of clearance.	Name of Vessel.	Owner or Master.	Tons.	How long built.	Wood or Iron.	Class.	Port of Loading.
January 1850	Columbia	John Ramsay (master)	517	2 years	Wood	A1	Liverpool
September 1850	Alfred of Belfast	Jas. Finlay (master)	1,073	5 „	Wood	—	London
(Autumn) 1852	Mary Shepherd	Gillespy and Scott	629	10 „	Wood	A1	London
June 1861	Admiral Lyons	Ross & Co.	1,133	5 „	Wood	A1	Sunderland
— 1861	Usk	Thos. Beynon	338	9 „	Wood	—	Swansea
August 1864	Joanna	Wm. Thomson	555	—	Wood	—	Leith
August 1864	Wanderer	„ „	639	—	Wood	—	„
August 1864	Annie Laurie	„ „	645	—	Wood	—	„
September 1864	Adelina		730	6 months	Iron	A1	Newcastle
September 1864	Lochee	Newton	1,501	9 years	Wood	—	Tay Port
September 1865	Glenlyon	Ross & Co.	1,440	—	Wood	—	Birkenhead
January 1866	Her Royal Highness	R. J. Hancock	1,244	8 „	Wood	A1	Cardiff
February 1868	Royal Family	Wm. Buchanan (master)	1,750	6 „	Wood	—	Birkenhead
September 1868	Persia	John H. Watt	1,290	7 „	Wood	A1	Greenock
— 1868	Hannah Law	Ross & Co.	—	—	Wood	—	Birkenhead
August 1869	Janet Cowan		1,278	—	Iron	A1	Greenock
October 1869	Serena	Geo. Symons (master)	493	3 „	Wood and iron	A1	Swansea
May 1870	Glaramara	Brymner & Co.	475	13 „	Wood	A1	Greenock
May 1870	Vanda	Stoddart Bros.	1,081	5 „	Wood	A1	Shields
June 1870	Malabar	J. Galloway (master)	1,200	2 „	Iron	A1	London
August 1870	Serena	Geo. Symons (master)	493	4 „	Wood and iron	A1	Swansea
October 1870	L'Agouhanna	W. Kennedy (master)	1,103	5 „	Wood	A1	Birkenhead
August 1872	Cereal	Wilfred Hine	290	13 „	Wood	A1	Swansea
October 1872	Bivouac	Wm. Laing	721	1 „	Iron	—	Newcastle
December 1872	Henry Bath	W. Pease (master)	486	8 „	Wood	A1	Swansea
May 1873	County of Nairn	Messrs. Craig	999	3 „	Composite	A1	Glasgow
May 1873	Persia	Mr. George Duncan	1,704	20 „*	Wood	A1	Cardiff
June 1873	Prince Alfred	Gates	—	—	Wood	—	Hull
June 1873	St. Mungo	Gilmour & Co.	1,375	8 „	Iron	A1	Dundee
July 1873	George Douthwaite	Ralph Curry	362	13 „	Wood	A1	Hull
July 1873	Walter Baine	J. J. Grieve	898	5 „	Iron	AA1	Dundee
August 1873	Channel Light	Chas. Blades	553	6 „		—	Hull
August 1873	Isabella Kerr†	John Kerr & Co.	1,415	9 „	Iron and wood	A1	Dundee
September 1873	Arracan	Colin Caird (master)	1,010	12 „	Wood	A1	North Shields
September 1873	Indian Empire	Geo. Duncan	1,515	8 „	Iron	A1	Shields
December 1873	Mooresfoot	W. and R. Wright	1,183	10 „	Iron	A1	Shields
December 1873	John	John Frostick	367	13 „	Wood	A1	Porthcawl

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* Almost rebuilt in 1865.

† The Isabella Kerr sailed again on 23rd Sept. 1874 with coal from Greenock for Bombay, and was posted at Lloyd's as missing 20th May 1875.

OF COALS.

I.

have been sent in answer to the Circulars of the Commission.

Port of Destination.	Quantity shipped, tons.	Description of Coal.	How loaded, Barrows or Tips.	Whether ventilated, and how.	How long Coal had been on board.	Supposed Cause and extent of Casualty.	Weather when loading.
Aden	750	North Welsh	From craft	Ventilated	Over 3 months	Ventilation; fire	Wet.
Valparaiso	1,050	Tyne	Dropped from deck 20 ft. first fall.	Not ventilated	Nearly 4 months	Want of ventilation and small coal.	Fine.
Shanghai	—	Resolven (hand picked)	Baskets and lighters.	Ventilated	4 or 5 months	Wetness of coal, ship burnt.	Wet.
Bombay	1,000 & 400 coke	Howard's West Hartley (double screened).	Spout	Ventilated	1¾ months	Wetness of coal, imperfect ventilation, height of fall; severe heating.	Wet.
Valparaiso	450	Smelting coal (not screened).	Waggon and shoot.	Ventilated	Over 3 months	Wet state of cargo; ship burnt.	Raining.
Quebec	442	Duke of Buccleugh (unscreened).	Waggon by crane.	Not ventilated	2 months	Quality of coal.	—
„	abt. 500	„ „ „	„ „	„ „	„ „	„ „	—
„	434	„ „ „	„ „	„ „	„ „	„ „	—
Shanghai	1,102	Bebside West Hartley and Cowpen Hartley.	Waggon	Ventilated	6 months	Ship scuttled	Pretty dry.
Calcutta	1,500	Townhill Colliery	Tubs	Partially ventilated	3½ months	Wetness of coal; ship abandoned.	Very wet.
Bombay	2,050	Westminster Brymbo (double screened).	Tips	Ventilated	5½ months	Loading by tips; fire extinguished.	Fine.
Valparaiso	1,804	Rhonda No. 3, thro' and thro'.	Barrows	Well ventilated	3 months	Nature and wetness of coal; heating, cargo discharged and sold at Monte Video.	Rain.
Aden	2,500	Welsh steam	Wheeled on board.	Ventilated but ventilationstove in and broken.	Nearly 3 months	Injury to ventilation and wet state of coal.	Rainy.
Bombay	1,830	Wishaw Coal, Lanark, (single screened).	Waggon	Ventilated	2 months	Ship burnt and abandoned.	Very showery.
Bombay	—	- - - -	- - -	- - -	5 months	Fire, extinguished by water.	—
Bombay	1,776	Splint coal from Scott and Gilmour's Collieries, Wishaw District.	Waggons	Ventilated	2½ months	To nature of coal which has fired in many vessels, and to accumulation of dust and small coal. Great heating.	Very dry.
Valparaiso	720	South Wales smelting coal.	Waggons, 40 feet fall.	Ventilated	Nearly 2 months	Sulphur in the coal; heated, put into Rio.	Very fine and dry.
Matanzas	660	Wishaw main coal	Partly by hand	Partially ventilated with wood.	2 months	Cause unknown; fire	Fine.
Calcutta	930 part cargo.	- - - -	Waggon spout	Not ventilated	Over 2 months	Ship abandoned.	—
Batavia, Java	1,274	Langley Mill	By hand basket from barges.	Ventilated horizontally on each side of keelson and perpendicularly at each hatchway.	1 month	Bad quality of coal and damp condition.	Fine, a portion of coal taken in wet from barges.
Chili	720	Avon Vale coal (not screened).	Boxes, about 20 feet fall.	Cargo divided into 3 separate compartments.	14 days	Not ascertained; put into Rio and ship sold.	Dry.
Bombay	1,600	- - - -	Waggon, fall 20 feet.	Ventilated	3½ months	Height coals fell in loading; fire.	Showery.
Santos	488	Merthyr Dare, ½ Grovesend, and Gelli and Cadoxton.	By spout	Not ventilated	2 months	Coal fresh wrought, wet, and of fiery nature; ship burnt.	Wet.
Odessa	1,229	- - - -	Spout	Well ventilated.	10 days	Wetness of coal, and possibly proximity to boiler bulkheads; fire extinguished.	Wet.
Chili	650	Smelting coal, Mynhyddy	By boxes	Ventilated	2½ months	Coal small and wet	Wet.
Java	600	Hamilton Coal Co., (double screened).	Wheeled on board.	Ventilated with iron.	3¾ months	No cause attributed	Fine and dry.
Aden	2,087	Insoles Merthyr and Steam coal.	Waggon	Ventilated	16 weeks	Not ascertained; partial damage.	Fine.
Galle	600	- - - -	- - -	Not ventilated	2½ months	No cause known; fire	Dry.
Point de Galle	1,700	Townhill Colliery steam coal (double screened)	Waggon	9 fixed ventilators, besides plank ventilators.	Nearly 3 months	Coal coming fresh from pit; ship abandoned.	Fine and dry.
Constantinople	626	S. & W. Yorkshire pits (double screened).	- - -	- - -	2 months	Wet state of cargo; fire	Raining incessantly.
Calcutta	1,170	Wishaw Ell coal (Lanark district (single screened).	Waggon	Through ventilation.	2¾ months	Ventilation, and wet state of coal.	Very wet.
Rio	750	- - - -	Tips	Ventilated	2 months	Freshness of coal from pits; ship scuttled at Rio.	Fine and dry.
Calcutta	1,955	Ell coal from Lanarkshire.	Waggon	Well ventilated.	7 weeks	Water getting in through side lights.	Wet during voyage.
Bombay	1,441	Cowpen	Waggon, 27 feet run, 26 feet fall.	Fairly ventilated.	Over 5 months	Dampness of coal; fired two days.	Fair, but wind and rain on voyage.
Bombay	1,977	Howard's West Hartley best quality steam (double screened).	By spout	Ventilated	3½ months	Coals too fresh from pits, and not sufficiently screened; ship beached and scuttled.	Average weather.
Bombay	1,664	Howard's West Hartley.	Tips and loading apparatus.	Well ventilated with Venetian shafts.	3 months	No information.	—
Valparaiso	564	Alner smelting coal (unscreened).	Waggon, fall 30 feet.	Ventilated	Over 3 months	Wet state of cargo; fire	Very wet.

TABLE II.

List of Coal-laden Vessels which sailed from the United Kingdom for Foreign Ports during the year 1874 to which Casualties occurred through Spontaneous Combustion in the Coal.

Date of Clearance or Sailing.	Name of Vessel.	Owners.	Flag.	Ton-nage.	How long built.	Wood or Iron.	Class.	Port of Loading.	Port of Destination.	Quantity shipped.	Description of Coal.	How loaded, Barrows or Tips.	Whether ventilated, and how.	How long Coal had been on Board.	Supposed Cause and Extent of Casualty.	Lives lost, so far as reported.	Weather when loading.
1874				Tons.						Tons.							
Feb. 11	Antigua	J. Kerr & Co.	British	643	16 yrs.	Wood	A1	Greenock	Cuba	910	—	—	—	—	—	—	—
Mar. 31	Lady Louisa	Wilson & Co.	British	542	9 „	Wood	A1	Cardiff	Shanghai	800	Powell Duffryn	- - -	Ventilated	181 days	—	—	—
April 22	Mogul	J. H. Sears & Co.	American	1,320	5 „	Wood	A1 America	Liverpool	San Francisco	1,555	Barnes's Lancashire	Tips	- - -	3 months	Total loss.	—	—
„ 29	Lucayas	T. O. Hunter	British	416	5 „	Iron	*A1	Dundee	Calcutta	680	Auldton Ell coal	- - -	- - -	4 months	—	—	—
„ 30	Lady Heathcote	J. Ransom	British	498	6 „	Wood	A1	Cardiff	Caldera	527	Insoles, No. 3 Rhonda	- - -	Ventilated	2 months	Total loss.	—	—
May 2	Centaur	Fletcher & Co.	British	1,212	10 „	Iron	*A1	Liverpool	San Francisco	1,310	Barnes's Lancashire	Tips	- - -	Over two months.	Total loss	16	—
„ 2	Noumea	Matthieu & Co.	French	1,155	12 „	Iron	*A1	Newcastle	Madras	1,530	Cowpen Hartley	- - -	Ventilated	115 days	Damaged.	—	—
„ 4	Anna	- - -	Dutch	969	- - -	Wood	- - -	Cardiff	Singapore	1,420	Insoles Merthyr steam coal, double screened.	Waggons, very slight fall.	Well ventilated	60 days	Cargo damaged; part thrown overboard.	- -	Fine and dry.
„ 6	Unione	Vianello Bros.	Italian	560	6 „	Wood	3·3	Shields, N.	Callao	784	- - -	- - -	- - -	- - -	Damaged.	—	—
„ 8	Mary Goodell	W. McGilvery	American	761	20 „	Wood	5·6	Greenock	Rio Janeiro	998	- - -	- - -	- - -	- - -	Damaged.	—	—
„ 26	Androcles	- - -	Greek	288	- - -	- -	- - -	Swansea	Odessa	406	- - -	- - -	- - -	- - -	Damaged.	—	—
June 1	John Elliot	J. Malcolm	British	1,198	9 „	Wood	A1	Dundee	Calcutta	1,706	Auldton steam coal	- - -	Well ventilated	3 months	Total loss.	—	—
„ 10	Susanna Johanna	von Zeylen & Co.	Dutch	1,145	15 „	Wood	A1 (red)	Newcastle	Acheen	1,600	Hartley coal	- - -	Ventilated	- -	Total loss.	—	—
„ 10	Euxine	E. P. Bates	British	1,594	27 „	Iron	A1	Newcastle	Aden	2,062	Howard's West Hartley	Tips and shoot.	Iron permanent ventilators; wooden additional ones.	60 days	Total loss	4	Fine.
„ 12	Palmyra	W. Thompson	British	932	5 „	Wood	A1	Liverpool	Madras	1,206	Hartley steam coal	Tips	Ventilated	3 months	Damaged.	—	—
„ 12	Michael	P. A. Cocali	Greek	312	19 „	Wood	- - -	Swansea	Alexandria	483	- - -	- - -	- - -	- - -	Vessel sold.		—
„ 13	Maasluis	G. H. Uitden-Boogaardt.	Dutch	797	8 „	Wood	3·3	Newcastle	Acheen	1,160	Bower's West Hartley	- - -	- - -	- - -	Cargo heated.	—	—
„ 16	Oliver Cromwell	W. R. Price & Co.	British	1,112	8 „	Wood	A1	Newcastle	Aden	1,530	Howard's West Hartley	Tip	Thoroughly ventilated.	83 days	Nature of coal; total loss.	- -	Fine.
„ 19	Ecta	W. & J. Smith	British	331	6 months	Wood	A1	London	Berbice	288	Swadlincote, 106 tons Baddersley 50 „ Babbington 96 „	Baskets	Well ventilated	38 days	Ship saved.	—	—
„ 22	Alexandre	A. Bordes	French	726	5 yrs.	Iron	*A1	Liverpool	Valparaiso	1,135	Lancashire best Orrell steam coal, double screened.	Waggon	Well ventilated	2 months	Damaged.	—	—
„ 22	Brenhilda	Hendry, Ferguson, & Co.	British	1,320	1 month	Iron	100A1	Glasgow	Calcutta	1,766	Ell coal	Waggon	Well ventilated	7 weeks	Coal too fresh from pits; cargo heated.	- -	Very fine.
„ 26	Merchant	I. S. Tumulty	British	1,059	12 yrs.	Wood	- - -	Liverpool	Acapulco	1,448	N. Wales	- - -	- - -	- - -	Damaged.	—	—
„ 27	Moss Trooper	Ismay & Co.	British	510	10 „	Wood	A1	Liverpool	Valparaiso	685	Ince coal, 4 ft. seam	- - -	Well ventilated	3 months	Total loss.	—	—
„ 27	Admiral Fitzroy	Burgess & Co.	British	377	10 „	Wood	A1	Cardiff	Valparaiso	521	Insoles No. 3 Rhonda, worked from Cymmer colliery, and from No. 3 Rhonda seam, through and through.	Waggon	Cargo divided by deal boarding.	10 weeks	Total loss	- -	Wet.
„ 27	Genitore	- - -	Italian	508	- - -	- -	- - -	Hull	Odessa	830	- - -	- - -	- - -	- - -	Total loss.	—	—
„ 29	David Brown	P. Pendleton, jr.	American	905	10 yrs.	Wood	A1 Amer.	Cardiff	Singapore	1,233	Merthyr steam coal	- - -	- - -	- - -	Cargo heated.	—	—
„ 30	Calcutta	W. R. Price & Co.	British	1,372	10 „	Wood	A1	Newcastle	Aden	1,800	Howard's West Hartley	Tips	Thoroughly ventilated.	80 days	Nature of coal; total loss.	- -	Fine.
July 2	Sierra Nevada	H. D. & J. Brookman	American	1,672	11 yrs.	Wood	3·3A	Liverpool	San Francisco	1,392	Barnes's Lancashire	Tips	- - -	40 days	Total loss.	—	—
„ 3	County of Perth	R. and J. Craig	British	1,626	1 month	Iron	100A1	Glasgow	Calcutta	1,617	Roughrigg's steam coal, double screened.	Barrows, fall 27 ft.	Well ventilated; wood and iron.	114 days	- - -	- -	Dry.
„ 4	Waldridge (s.)	Clark & Co.	British	432	6 yrs.	Iron	A1	Sunderland	Cronstadt	850	- - -	- - -	- - -	- - -	Cargo heated.	—	—
„ 8	Pocahontas	W. Dixon	British	1,132	19 „	Wood	- - -	Newcastle	Bombay	1,579	Hastings Hartley	- - -	Well ventilated	65 days	Total loss.	—	—
„ 9	Willie S. Thompson.	R. Thompson	British	846	2 „	Wood	3·3L	Newcastle	Iquique	1,300	West Hartley	- - -	Ventilated	3 months	Total loss	7	—
„ 10	Camano	- - -	Italian	487	2 yrs.	Wood	3·3A	Liverpool	Odessa	807	—	—	—	—	—	—	—
„ 10	Dumbartonshire	T. Law & Co.	British	914	2 months	Iron	100A1	Glasgow	Buenos Ayres	530	Splint coal, Russell's colliery.	Barrows	Partially ventilated	2 months	Damaged	- -	Good Weather.
„ 10	Workington	Hargrove & Co.	British	1,252	1 month	Iron	100A1	Port Glasgow	San Francisco	1,640	Splint coal, mixed. Woodside field, Hellfield, Lougher field, and Skellyton field.	- - -	Ventilated	56 days	Vessel and cargo sold.	—	—

1874. July 13	Industry	R. Robbins	British	1,138	16 yrs.	Wood	-	Newcastle	Bombay	1,455	Davidson's West Hartley steam coal from Bidlington colliery, probably low main seam, single screened.	Waggon and spout.	Carefully ventilated	98 days	Freshness of coal; total loss.	-	Fine and dry.
„ 13	Blair Athol	Thomson & Co.	British	1,697	1 month	Iron	100⋀1	Glasgow	Bombay	2,249	Watson's Hartley	-	Well ventilated	2½ months	Cargo heated.	—	—
„ 15	Mendora	Stoddart Brothers	British	970	9 yrs.	Wood	A1	Liverpool	Bombay	1,460	Tryddyn colliery, Coed Talon.	-	Ventilated	2½ months	Damaged.	—	—
„ 16	Staffordshire	D. Stuart & Co.	British	1,167	12 „	Iron	*⩓1	Liverpool	San Francisco	814	Lancashire steam coal, White Moss coal, single screened.	From high level. Only slight fall.	Ventilated (wood)	100 days	Newness of coal; damaged.	-	Very fine.
„ 21	Victorine	Lequellec and Bordes	French	1,621	16 „	Comp.	-	Liverpool	Valparaiso	2,480	Orrell steam coal	Tips	Well ventilated	-	Cargo fired.	—	—
„ 22	Go-ahead	C. Ahrens	German	385	20 „	Wood	-	Liverpool	Constantinople	602	-	-	-	89 days	Damaged.	—	—
„ 24	Pelligrina	G. Mortola	Italian	319	13 „	Wood	-	Swansea	Piræus	521	-	-	-	-	Damaged.	—	—
„ 27	Alpha	Bath & Co.	British	397	10 „	Wood	A1	Swansea	Valparaiso	550	Rhonda, No. 3	-	-	2 months	Damaged.	—	—
„ 28	Glentilt	Hughes & Co.	British	991	9 „	Wood	A1	Cardiff	Coquimbo	607	Non-screened smelting coal, Penygraig colliery.	By shoot	Ventilated with platforms under cargo.	103 days	Inferiorquality, and damp state of coal.	-	Very fine and dry.
„ 30	Glory-of-the-Seas	J. H. Sears & Co.	American	2,102	5 „	Wood	A1 Amer.	Liverpool	San Francisco	2,100	Barnes's, Lancashire	Tips	-	-	Cargo heated.	—	—
„ 30	Ermenigilda Danovaro.	A. Danovare	Italian	813	19 „	Wood	5·6⋀	Cardiff	Valparaiso	1,102	No. 3, Rhonda, (smelting coal).	-	-	-	Damaged.	—	—
Aug. 4	Laine	C. A. Berntedt	Russian	520	8 „	Wood	3·3⋀	Hull	Athens	781	-	-	-	-	Vessel sold.	—	—
„ 6	Ontario	Stuckey & Co.	British	1,062	13 „	Wood	-	Shields, So.	Bombay	1,526	West Hartley, Main coal.	Tips	Well ventilated	5½ months	Damaged	-	Fine, but showery.
„ 6	Ascolta	F. Marciani	Italian	302	9 „	Wood	-	Hull	Constantinople	527	-	-	-	-	Total loss.	—	—
„ 10	Respigadera	Hargroove & Co.	British	1,629	1 month	Iron	20 (red)	Sunderland	San Francisco	1,974	Cowpen Hartley	-	Well ventilated	-	Damaged.	—	—
„ 13	Northbrooke	Adamson & Ronaldson	British	1,820	4 months	Iron	100 A1	Shields, No.	Madras	1,657	Cowpen Hartley, and Hastings Hartley.	-	Well ventilated	4 months.	Damp state of coal.	-	Changeable.
„ 13	New Lampedo	T. Seed	British	1,099	10 yrs.	Wood	A2 Amer.	Liverpool	Panama	1,499	Barne's, Lancashire	Tips	Deck ventilators fore and aft, grain lining left in 'tween decks.	3 months	Total loss	-	Very wet.
„ 14	Enthusiast	J. Ditchburn	British	283	20 „	Wood	A1	London	Demerara	193	Swadlincote, (single screened).	Basket	-	53 days	Total loss.	—	—
„ 15	Monmouthshire	D. Stuart	British	1,161	11 „	Wood	-	Liverpool	San Francisco	1,385	Barnes's, Lancashire	Tips	Well ventilated	6 weeks	Damaged.	—	—
„ 19	La Escosesa	Balfour & Co.	British	946	6 „	Iron	⩓1	Cardiff	Valparaiso	1,453	No. 3, Rhonda	Tips	Well ventilated, (Venetian ventilators).	130 days	Coal green and wet, damaged.	-	Raining heavily.
„ 24	Clothilde		Italian	568	-		-	Cardiff	Valparaiso	751	Rhonda, No. 3	-	-	-	Total loss.	—	—
„ 29	St. Nicholas	Chapman & Flint	American	1,798	5 „	Wood	3/3 A	Liverpool	San Francisco	2,015	Brancker's, Pemberton coal.	Tips	Ventilated and platformed.	3½ months	Damaged.	—	—
About end of Aug.	Skandia	-	Fin	281	-	Wood	-	Gloucester	Copenhagen	250	-	-	-	6 weeks	Vessel damaged.	—	—
Sept. 4	Try	W. and J. Wright	British	428	„	Wood	A	London	Demerara	107	57 tons Swadlincote 50 „ Stockley Hall	Basket From craft.	Probably (not certain).	60 days	Cargo heated.	—	—
„ 5	Crystal	W. and J. Wright	British	263	2 „	Wood	A1	London	Berbice River (New Amsterdam).	239	136 „ Swadlincote 102 „ Stockley Hall	Basket From lighter.	Ventilated	80 days	Cargo heated.	—	—
„ 15	Khersonese	G. Duncan	British	1,710	19 „		100⋀1	Newcastle	Bombay	2,000	Bebside West Hartley, double screened.	-	Well ventilated	160 days.	—	—	—
„ 18	P. J. F. Burchard	L. Burchard and Son	German	459	13 ,	Wood	-	Leith	Monte Video	720	Splint coal.	—	—	—	—	—	—
„ 19	The Foundling	E. Bates	British	1,185	8 „	Iron	⩓1	Liverpool	Bombay	1,281	Mixed:—795 tons Westminster Brymbo; 306 tons Oak Pits coals; 200 tons coke.	Tip and shoot.	Permanent iron ventilation; additional wooden ones.	2 months	Total loss	-	Wet.
„ 28	May Queen	Thompson	British	1,055	12 „	Wood	A1	Newcastle	Rangoon	1,415	Cowpen Hartley	-	-	153 days	Total loss.	—	—
„ 29	Chili	-	French	1,441	-	-	-	Liverpool	Valparaiso	2,000	Orrell steam coal	Tips	Ventilated	3 weeks.	—	—	—
Oct. 2	Eugenie	-	German	734	-	Iron	-	Liverpool	Valparaiso	860	-	-	-	-	Damaged.	—	—
„ 3	Levant	Jones & Co.	British	314	8 „	Wood	A1	Swansea	Valparaiso	470	Smelting coal; not screened.	Waggon, 20 ft. fall.	Ventilated with wood.	100 days	Damaged	-	Partly wet.
„ 5	Tilde	A. Poschich	Austrian	636	10 months	Wood	A1	Hull	Maulmain	869	—	—	—	—	—	—	—
„ 16	Fedalma	Scrutton, Sons & Co.	British	478	5 yrs.	Iron	*⩓1	Porthcawl	Valparaiso	654	Smelting coal from the "Aber Coal Co.," not screened.	Tip	Cargo was divided fore and aft by stanchions from deck to keelson, and ½-inch boards nailed to them, thus leaving space 6 inches broad from top to bottom, dividing the whole cargo in the middle.	-	Fire; slight damage.	-	Fine and dry.
„ 23	City of Richmond	D. Kennedy	British	1,241	10 yrs.	Wood	-	Liverpool	Bombay	1,638	Laisols' West Hartley, double screened.	Tip	Well ventilated and platformed.	142 days	Cargo in state of combustion.	—	—

TABLE III.

LIST of COAL-LADEN VESSELS which sailed from the United Kingdom for Foreign Ports during the Year 1875, to which Casualties occurred through Spontaneous Combustion in the Coal.

Date of Clearance or Sailing.	Name of Vessel.	Owners.	Flag.	Tonnage.	How long built.	Wood or Iron.	Class.	Port of Loading.	Port of Destination.	Quantities shipped.	Description of Coal.	How loaded, Barrows or Tips.	Whether ventilated and how.	How long Coal had been on Board.	Supposed cause and extent of Casualty.	Lives lost, so far as reported.	Weather when loading.
1875 March 9	Quatters Fratelli	Dillorto Frères, Genoa	Italian -	500	6 years	Wood	3/3 A. -	Cardiff -	Callao -	790	279 tons of Rhondda, No. 3; 511 of tons Aber Rhondda, No. 2.	- - -	- - -	- - -	Fire; vessel damaged; cargo sold.	—	—
„ 18	Independenza -	G. Lisario, Malta -	Italian -	505	18 „	Wood	- - -	Cardiff -	Valparaiso -	860	Aber Rhondda, No. 2 -	- - -	- - -	- - -	Cargo much heated.	—	—
April 24	Angelo Antonio -	E. Razetto, Genoa -	Italian -	598	5 „	Wood	- - -	Liverpool -	Callao -	900	Best Orrell coal -	- - -	- - -	9 weeks -	Burnt; total loss.	—	—
„ 27	Pilgrim - -	H. Hastings, Boston, U.S.	American	1,050	1 „	Wood	1 Amer. -	Shields -	Hong Kong -	1,404	Cowpen's West Hartley	By spout -	Ship ventilated fore and aft; no ventilators for cargo.	Nearly 5 months.	Fire under main hatch; slight damage.	- -	Fine.
May 13	Antelope - -	B. McNear & Co., Boston, U.S.	American	1,306	2 „	Wood	3/3 L. -	Clyde -	San Francisco	1,273	Lanarkshire hard splint coal from Mr. John Watson's pits.	- - -	- - -	Nearly 5 months.	Took fire whilst discharging; vessel damaged.	—	—
June 3	Frankfurt Hall -	W. P. Coleborn, 2, Tower Chambers, Liverpool.	British -	728	6 „	Iron	*A 1 -	Liverpool -	Valparaiso -	602	Shippers state steam coal from Tawd Vale Colliery, 7-ft. seam. Double screened.	Wagon -	Eight mooring bitts constructed as ventilators, and three large open galvanized iron ventilators.	Nearly 4 months.	Fire; cargo damaged.	- -	Fine and dry.
„ 5	Helen Morris -	C. T. Child & Co., New York.	American	1,285	7 „	Wood	1 Amer. -	Shields -	San Francisco	1,650	Cowpen West Hartley steam coal.	- - -	- - -	Nearly 6 months.	Burnt; total loss.	—	—
„ 8	Senator - -	- - - -	American	- -	- - -	Wood	- - -	Philadelphia -	Panama -	1,600	- - - -	- - -	Ventilators below the hatches.	3 months -	Burnt; total loss.	Two	—
„ 8	Willem Poolman -	Van Zeyland & Co., Rotterdam.	Dutch -	1,244	9 years	Wood	A - -	Shields -	Java -	1,600	- - - -	- - -	- - -	- - -	Burnt; total loss.	—	—
„ 18	Aracana - -	G. N. Roberts, St. John's, N.B.	British -	1,192	13 „	Wood	2¹ Amer. -	Liverpool -	Bombay -	1,756	- - - -	- - -	- - -	4 months -	Burnt; total loss.	—	—
„ 20	Aurelie - -	Etienne Michou, Quebec.	American	546	6 „	Wood	L 11 Veritas	Liverpool -	Barbadoes -	701	Bituminous steam coal	- - -	Ventilated by hatches; no ventilators for cargo.	6 weeks -	Burnt; total loss.	- -	Fine.
„ 25	Clevedon - -	E. C. Friend, 2, Tower Chambers, Liverpool.	British -	698	7 „	Iron	*A 1 -	Liverpool -	Valparaiso -	1,037	Shippers state steam coal from Tawd Vale and Crow Orchard Collieries. 7-ft. seam. Double screened.	250 tons by by lighter rest by boxes from tips.	Eight mooring bitts constructed as ventilators, and three large open galvanized iron ventilators through deck.	3 months -	Burnt; total loss.	- -	Fine while loading, heavy weather off Cape Horn.
„ 30	Martin Wienert -	J. Johnasson, 5 Gloucester Sqre., London.	British -	506	3 „	Iron	90 A 1 -	Shields -	Mühlgraben -	1,048	Bebside screened Hartley steam coals.	From wagon by spout.	All holds and other parts of vessel ventilated by several metal pipes carried above deck and having swan necks.	A week -	Fire; slight damage; part cargo jettisoned.	- -	Good weather.

July 10	Dominicho Galliano.	Galliano Fils, Genoa	Italian	537	10 yrs.	Wood	3/3 A	Cardiff	Singapore	812	Large steam coal	- - -	-	3 months	Fire; vessel much damaged.	—	—
„ 16	River Boyne	J. Hargrove, Hargrave's Buildings, Chapel St., Liverpool.	British	499	8 „	Iron	*A1	Liverpool	Valparaiso	766	Orrel coal	- - -	Hatches, &c. forward and aft.	3 months	Fire; part cargo jettisoned.	—	—
„ 25	Powhattan	G. Meager, Swansea	British	399	9 „	Wood	A1	Swansea	Valparaiso	580	Smelting, Trycher, through and through.	Shoot	Ventilated bulkhead to bulkhead with deals from keelson to deck.	11 weeks	Cargo heated; part jettisoned.	- -	Very fine.
Aug. 14	Beucleugh	W. Thomson, Alloa	British	1,350	4 months	Iron	100 A1	Greenock	Bombay	1,781	Watson's Hartley	By wagon and steam crane.	In addition to ventilation suitable for emigrant trade she had four wooden venetianed ventilators down to ceiling.	3 months	Fire; part cargo jettisoned.	- -	Wet weather.
Sept. 18	Aberaman	Capper & Alexander, Cardiff.	British	1,090	6 „	Iron	100 A1	Shields	Bombay	1,620	Cowpen Hartley steam coal, single screened.	Tips	Ventilated through masts, bitts, &c., and iron ventilators both ends of ship with cowls.	4 months	Cargo much heated, almost on point of ignition.	- -	Fine.
„ 25	Theresa	B. French, 44, Castle St., Liverpool.	British	704	18 yrs.	Wood	5/6 L. 11 in Veritas.	Birkenhead	Rangoon	920	Chill Colliery Co., Thurstall, Staffordshire.	From tips	Excessively ventilated; square venetianed deal ventilator in each hatch to keelson, and a trunkway on each side of keelson, and three rose - mushroom ventilators in each hatch.	2½ months	Burnt. Total loss.	- -	Part of the time raining.
Oct. 10	Le Baron	J. W. Paynter, Liverpool.	British	907	5 „	Wood	- - -	Hull	Valparaiso	1,378	Steam coal of second class. West Riding screened Hartley, Messrs. Pope & Pearson's pit near Normanton.	- - -	- -	- - -	Burnt. Total loss.	One	Fine.
„ 16	Patmos	Lunt Bros. & Co., Newbury Port, U.S.	American	738	16 „	Wood	1^1 Amer.	Hull	Valparaiso	- -	Steam coal second class. Featherstone's Main Hartley Collery.	- - -	- -	- - -	Burnt. Total loss.	—	—
—	Utrecht	Van Zeyland & Dreker	Dutch	1,873	7 „	Wood	A1	Newcastle	Batavia	- -	- - - -	- - -	- -	2 months	Fire; jettisoned part cargo.	—	—
—	Mabel	C. Hill & Son, Bristol	British	429	2 „	Wood	A1	Porthcawl	Caldera	647	Through and through smelting coal.	Tip and shoot.	Boarded longitudinally from keelson to upper deck having space between, also platform 14 or 15 inches high.	3 months	Fire; slight damage.	- -	Variable.
Sept. 18	San Rafael	Balfour, Williams, & Co., Liverpool.	British	955	13 „	Wood	A1 Amer.	Liverpool	Valparaiso	1,300	- - - -	- - -	A ventilator in each hatch.	3 months	Burnt. Total loss.	Ten	Fine and close.
—	Fanny	H. Holmes, Bishopswearmouth.	British	398	12 „	Wood	A1	Porthcawl	Valparaiso	- -	- - - -	- - -	- -	Not known; (casualty occurred 28th Dec. 1875.	Fire; great damage.	—	—
Dec. 26	John Clarke	- - - -	American	1,079	19 „	Wood	1^3 Amer.	Liverpool	Bombay	1,343	- - - -	- - -	- -	4½ months	Fire.	—	—

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† This vessel sailed again 31st August from the Tyne for Hamburg with coal, and was published as missing in Lloyds' List of 7th October. In Lloyds' List of 9th September a report appears of a lifeboat belonging to this vessel having been picked up off the Doggerbank, and in Lloyds' List of 10th September there is a paragraph stating that it was reported that an explosion had taken place causing her to founder, and that there were 22 hands on board.

TABLE IV.

List of Coal-Laden Vessels posted at Lloyd's as Missing, having sailed from the United Kingdom during the years 1874 and 1875 on over-sea Voyages.

Date of Sailing.	Vessel.	Tonnage.	Voyage.		Date of Lloyd's List when posted as Missing.
1874.					
January 17	Sidra (s)	923	Shields	Port Said	March 26, 1874.
February 1	Pilgrim	182	Mostyn	Santander	May 7, 1874.
„ 3	Lydia	100	Ayr	Malaga	July 30, 1874.
„ 6	Alejandro	436	Newport	Hong Kong	December 3, 1874.
„ 6	Alida	313	Liverpool	Buenos Ayres	Part coal.
„ 6	George Duckles	172	Liverpool	Lagos	September 24, 1874 (part coal).
„ 7	Eliza Thornton	429	Cardiff	Cadiz	August 20, 1874.
„ 9	Zelie	172	Cardiff	Bordeaux	May 7, 1874.
„ 9	Anna	75	Liverpool	Reikjavik	July 30, 1874 (part coal).
March 18	Maria	197	Tyne	Stockholm	July 9, 1874.
„ 20	Forest King	1,085	Newcastle	Rio Janeiro	September 24, 1874.
„ 24	Sea Queen	372	Liverpool	Genoa & Rio Janeiro	July 30, 1874.
„ 31	Rhuma	138	Liverpool	Lisbon	July 9, 1874.
April 1	Everdina	212	Sunderland	Stockholm	August 13, 1874.
„ 2	Franklyn	284	Peterhead	Iceland	August 6, 1874.
„ 4	Freak	99	Shields	Cadiz	July 30, 1874.
„ 6	Raven	101	Cardiff	Lisbon	October 8, 1874.
„ 7	Speedwell	668	Cardiff	Malta	August 13, 1874.
„ 7	Myrtle	417	Sunderland	Palermo	August 13, 1874.
„ 8	Atlantic	996	Liverpool	Aden	November 12, 1874.
„ 8	Eliza Forbes	149	Tyne	Stralsund	July 16, 1874.
„ 9	May Queen (s)	577	Tyne	Carthagena	May 28, 1874.
„ 9	Hyaline	171	Newcastle	Guadeloupe	December 3, 1874.
„ 10	The Barton (s)	417	Greenock	Licata	June 11, 1874.
„ 11	Pontiac	1,050	Sunderland	Bombay	December 10, 1874.
„ 12	Jules de Rontaunay	437	Cardiff	Bahia	November 12, 1874.
June 17	North Glen	499	Liverpool	Caldera	March 11, 1875 (part coal).
July 13	Eastern Star	340	Troon	Singapore	April 1, 1875.
„ 15	Maypocho	699	Liverpool	Valparaiso	March 4, 1875.
August 15	Carmainia	729	Liverpool	Valparaiso	March 25, 1875.
September 23	Isabella Kerr	1,415	Greenock	Bombay	May 20, 1875.
October 13	Nora Graeme	1,001	Liverpool	Bombay	June 3, 1875.
„ 16	St. Mirren	526	Troon	Demerara	February 25, 1875 (part coal).
November 7	Romeo	210	Granton	Berbice	April 9, 1875.
„ 20	Anna	157	Cardiff	Villafranca	July 8, 1875.
„ 27	Success	474	Liverpool	Shanghai	September 2, 1875.
„ 28	Solide	294	Cardiff	Port Mahon	May 14, 1875.
„ 30	Ann Mitchell	189	Cardiff	Bari	April 22, 1875 (part coal).
December 5	Alice (s)	627	Cardiff	Constantinople	March 18, 1875.
„ 5	Alfred	79	Swansea	Granville	March 11, 1875.
„ 15	Argosy	394	Shields	Mauritius	August 5, 1875 (part (coal).
„ 31	Marmion	292	Tyne	Buenos Ayres	July 8, 1875.
1875.					
January 10	Hans Christen Orsted	163	Tyne	Messina	May 27, 1875.
„ 22	George Batters (s)	714	Porthcawl	Gibraltar	March 18, 1875.
June 7	Lothair	859	Liverpool	Valparaiso	June 29, 1876.
July 27	Rosewood	276	Troon	Matanzas	December 30, 1875.
August 31	Martin Wiener (s)	755	Tyne	Hamburg	October 7, 1875.
September 24	Mette	91	Tyne	Korsoer	December 16, 1875.
October 26	Acapulco	598	Liverpool	Valparaiso	June 15, 1876.
November 8 (Yokahama Roads)	Yokohama	154	Tyne	Rotterdam	January 13, 1876.
November 9	Cavalier (s)	115	Swansea	Nantes	December 16, 1875.
„ 15	Lousitania	206	Newcastle	Oporto	March 2, 1876.
„ 20	Benton	952	Birkenhead	Bombay	July 6, 1876.
December 3	Magic	180	Tyne	Lisbon	March 9, 1876.

This List is made up to July 6th, 1876; but there may be other Vessels posted as Missing during the remainder of the year 1876, which had sailed in the year 1875.

Vessels with Patent Fuel not included in the List.

APPENDIX VI.

Form of Specification for Coal Cargoes.

* Specification (in lieu of Shipping Bill and Customs Bill of lading) for *British and Irish* goods only.†

Port of ____________ In the ship ____________ ____________Master, for ____________

* The Specification of Goods exported must be delivered to the proper Officers of Customs within six days from the time of the final clearance of the ship, as required by the 23 Vict. cap. 22.

† The Goods must be described in accordance with the denomination in the Official List of British and Irish Produce and Manufactures exported.

Marks.	Numbers.	No. and Description of Packages.	Quantity and Description of Goods.	Value.
			Total -	

I declare that the particulars set forth above are correctly stated.

Date ____________ 187 . Exporter or Agent____________ Address ____________

G O $\frac{139}{1870}$ and $\frac{11}{1873}$.

APPENDIX VII.

Selected Correspondence.

(*a*.) Letter from Mr. Charles Icely, R.N., on Combustion of Coal in Plymouth Gasworks.

8 Hyde Park Terrace, Mutley,
near Plymouth, 1st April 1876.

Sir,

In accordance with the wish of the Commission for a report in regard to the case of spontaneous combustion at the Plymouth Gasworks, which manifested itself actively during the month of February last, I beg to submit the following statement as the result of my observation on the spot, together with information given by Mr. Browning, the engineer of the works. There are at these works six coal stores, two of which are severally 116 feet 6 inches long, 66 feet broad, and 26 feet in height, and will contain each 4,750 tons. The remaining ones, which are smaller, vary in capacity. The whole of them will contain in the aggregate 13,000 tons.

On the occasion of my visit the smaller ones were clear of coal, and the two larger (which are contiguous) were about two-thirds empty, but still containing from 700 to 800 tons each, represented by heaps 26 feet in height. The coal in both was in process of combustion, and presented this singular feature, "That the fire was mainly confined " to a stratum about 18 inches wide, and this at a height " of 10 feet from the basement, traversing the heap " laterally, and identical in height and width in both " stores, although a stone partition wall two feet in " thickness divided them." There were inconsiderable patches of fire (if they may be so termed) in other parts of these heaps above this *belt*, but none that attracted my attention at this time below it.

I elicited from Mr. Browning that it had been the custom of the Company for some years past to keep the stores filled up by a continuous supply during the summer months, while freights were low, and that in consequence a great portion of the coal would be stored from five to six months before being required for use. In such case it invariably heated, and year after year it had been his experience, when digging into the mass, to discover combustion more or less advanced. He had observed, too, on these occasions, that in places where the combustion was in its incipient stage the new supply of air rendered by the turning over of the coal soon induced activity.

To show the inertness of water in extinguishing fire of this nature, he drew my attention to isolated heaps of coal in other parts of the store, of from one to two tons, then smouldering, which he assured me had been well damped with water a week previously.

The coals usually bought by the Plymouth Company are of three varieties, viz.:—Peareth, Pelaw Main, and New Pelton. Mr. Browning was of opinion that "Peareth" was in the largest quantity in this case.

The large heaps in which the fire was present were closely lying masses of small coal. A sample was found to be very friable, and largely impregnated with iron pyrites.

Viewing the facts of this case as herein narrated, they appear to be fairly confirmatory of the evidence already before the Commission, viz:—"The danger accruing from " long storage of finely divided coal of certain classes."

"The manifest tendency the so-called through ventila- " tion would have in favouring the development of heat " in such coal," and,—

That water when poured on the surface of a body of coal that has spontaneously ignited, though it may arrest the progress of the fire, does not effectually quench it.

In view also of the tenacity with which spontaneous fire seems to cling to the combustible substance of coal, I submit with much deference, for the consideration of the Commission, that if the contemplated steam experiment for dealing with an artificial fire proves successful, a further trial of its efficacy should be made (if practicable) on a mass of coal under the altered conditions of spontaneous ignition.

I am, &c.
Chas. Icely.

J. M. Carmichael, Esq.

(*b*.) Letter from Professor Ferguson on Chemical Analysis of Coal.

The following is a letter from Mr. Ferguson, Professor of Chemistry at Glasgow University, giving the results of his examination of four samples of coal submitted to him, and at the same time expressing his opinion that conclusions obtained from such limited experiments should not be relied upon.

The samples submitted to him were "Hamilton Ell," "Watson's Ell," "Russell's Splint," and "Roughrigg coal." "Hamilton Ell" and "Watson's Ell," or "Hartley" as it is sometimes called, would represent the cargoes shipped in the "County of Nairn," "Walter Bain," "Isabella Kerr," "Lucayas," "Brenhilda," and "Blair Athole," but it was stated by those supplying the samples that they would show a greater proportion of pyrites than would be found in the mass, although pyrites would be found to a greater or less degree in all Ell coal.

University of Glasgow,
May 16th, 1876.

Dear Sir,

In reply to your favour of the 15th inst., I beg to send you the results of my examination of four samples of coal furnished me by Mr. John Ferguson of Whiteinch. You will observe that I have confined my examination to the general composition of the coals, in so far as that seemed to me to bear on the question of spontaneous combustion. No. 1. is Ell coal from the Hamilton Coal Company: No. 2 is Ell coal from J. Watson; No. 3 is Ell coal from Roughrigg; No 4 is Russell's Splint. The numbers express percentages.

	No. 1.	No. 2.	No. 3.	No. 4.
Moisture - - -	8·15	6·55	1·60	6·30
Ash - - -	19·50	17·20	2·90	7·20
Coke - - -	59·24	55·88	67·10	57·12
Sulphur (total) - -	14·66	10·84	—·809	—·589
Iron - - -	5·68	5·42	Trace.	Trace.
„ pyrites - -	12·17	11·61		

Nos. 1 and 2 accordingly contain much pyrites, indeed the mineral was quite apparent in the coal; Nos. 3 and 4 are practically free from pyrites.

In order to ascertain to what extent the pyrites in the coal undergoes oxidation, I made one or two experiments, with the following resuls. Portions of the coals were taken and the amount of oxidized sulphur (assumed to be sulphuric anhydride—OS_3—entirely) present estimated. The coals were then spread out to the air, one part being kept dry, another being moistened from time to time. Portions of these were taken at intervals and new deter-tions of SO_3 in them made. The numbers again denote per-centages.

——	No. 1. SO_3.	No. 2 SO_3.	No. 3. SO_3.	No. 4. SO_3.
In original sample at 21st December 1875.	—·198	—·218	Nil.	Nil.
After exposure, moist, till 21st January 1876.	—·280	—·346	—	—
After further exposure till 24th January 1876.	—·308	—·390	—	—
After exposure, dry, till 21st January 1876.	—·311	—·307	—	—
After further exposure till 24th January 1876.	—·342	—·350	—·034	—·099

A portion of No. 2 was also kept in air for a week at a temperature of 104° Fahr., at the end of which time the SO_3 amounted to —·548.

These numbers, if they are of any value at all, (for in an experiment on so small a scale and for so short a time the smallest accidental circumstance might produce considerable variations and anomalies, which could be got rid of only after many repititions,) show that the oxidation was most rapid at first. Thus No. 1 in the dry state increased at first from ·198 to ·311, that is, by ·113; afterwards it increased from ·311 to ·342, or by ·031 only, and similarly with all the others. It is observable that No. 1 moist oxidized less rapidly than when dry, whereas No. 2 oxidized at first more rapidly than when dry. The results therefore with regard to dampness of the coal are contradictory. The oxidation of the pyrites appears to go on slowly and at a diminishing rate. The only other inference that forces itself upon us is, that if all four are samples of coal which have caught fire spontaneously, it can hardly have been due to the oxidation of pyrites alone, seeing that No. 3 and 4 are free from that substance.

My opinion is that conclusions of any worth could be arrived at only after very many experiments on different kinds of coal, placed under varying conditions of temperature, &c., and that great care is requisite in interpreting the results of two or three experiments like the above. The next point to examine is the extent to which the carbon and hydrogen of the coal itself undergo slow oxidation, and whether spontaneous combustion be not due to that action.

I am, &c.

J. M. Carmichael, Esq. JOHN FERGUSON.

(c.) LETTER from MR. POOLE, INSPECTOR of MINES, NOVA SCOTIA.

Halifax, Nova Scotia,
January 12th, 1876.

SIR,

IN accordance with the request conveyed in your letter of August 3rd, 1875, I have the honour to forward you the following information on the subject before the Commission, to be submitted, should it meet with your approval, before the Royal Commission on the Spontaneous Combustion of Coal at Sea.

I am happy to be enabled to give, besides the results of my own observations, information bearing on the subject from several gentlemen of the United States. I am more particularly indebted to Estes Howe, Esq., M.D., President of the Cambridge Gaslight Company, for his assistance in obtaining the opinions of others. He has sent me the accompanying paper (I.) read before the New England Association of Gas Engineers, in February 1875, by J. H. Armington, Esq., Superintendent of the Brooklyn Gas Company.

II. A statement, with a diagram, from Mr. Lees, President of the New York Gas Company.

III. A letter from the Hon. Knowlton S. Chaffee on the Spontaneous Combustion of Charcoal.

IV. Is Dr. Howe's own statement.

V. A statement of R. C. Adams, Esq., of Montreal, on the heating of Nova Scotia coals in the sheds at Montreal and Quebec.

VI. A letter from W. W. Greenough, Esq., Treasurer of the Boston Gaslight Company, on the Spontaneous Combustion of Albertite.

On my own part, I will first refer to the coals of this province, which are all bituminous, and which may be classified according to the two chief districts in which they are mined, Pictou and Cape Breton, into non-caking and caking coals. The following analyses may be taken as typical of their composition:—

	Pictou.		Cape Breton.
Volatile matter	- 30·48	-	33·44
Fixed carbon	- 61·23	-	62·32
Ash -	- 8·29	-	4·23
	100·00		100·00

The per-centage of sulphur in the Pictou coals varies from ·26 to 1·68; an average of seven analyses gives ·72 per cent. The quantity contained in Cape Breton coals is greater; an average of fourteen analyses gives 2·05 per cent. of sulphur.

The Pictou coals are harder, and stand handling and transportation better than those of Cape Breton. They are chiefly used for steam purposes, and so far as is known they have never heated, either in store or on board ship. The quantities exported south of the equator have been very small: 4,388 tons in 1874, 753 tons in 1873, and 1,600 tons in 1875. Large quantities have, however, been stored for months in the chief towns of New England, and in Montreal and Quebec, and in no case is the coal known to have heated to any extent.

The same cannot be said of the Cape Breton coals, which are more friable than those of Pictou, and cases of spontaneous combustion are known to have occured with some of these coals. They are nearly all caking coals, and suitable for gas manufacture, though many or all find some service as steam and domestic coals. The quantities sent south of the equator have been small: 1,132 tons in 1873, 688 tons in 1874, and 3,446 tons in 1875.

A cargo of 1,000 tons, unventilated, arrived in good order at Singapore.

The general practice of the country during the winter months, when the ports are closed and shipping prevented by ice, is to bank the coal mined in heaps of ten, twenty, and even forty thousand tons, to a height of 18 to 25 feet. In no case has heat been known to have been generated in these banks, although some of them have not been all lifted until late in the autumn. As a confirmation of the absence of a great increase of temperature, it has often been noticed that when the coal has been negligently dumped over snow, ice is found in the heap even late in the summer. As, however, many coals so heaped do slack even in the centre of the banks, I suspect a slight increase of temperature does take place at some period. I shall suggest that tubes be inserted, and the temperature at several places in some heaps be taken and compared with the average of the atmosphere. At some mines the coal banked is first riddled in the pit, at others the duff is not removed, and the opinion of some managers is, that the per-centage of screenings is as large from that which has been riddled as from that which has not been riddled; in other words, that the deterioration is diminished by leaving the duff with the coal. Heating has been observed in piles of slack coal that have been accumulated during the summer for the use of the engines during the winter, when the season has been showery, but not when it has been dry. A pile of 3,000 tons of coarse coal of the same seam was kept in sheds for three years without heating. A heap of screenings 19 feet high, 250 feet long, and averaging 33 feet wide, collected within a couple of months, took fire within two months after being completed. Smoke was first observed on the side of the heap some 6 feet from the bottom. On shovelling away the surface layer, 6 inches thick, a crust a foot in thickness was found underneath, which, on being disturbed, broke into flames. A similar heap on drier ground, but four times as long, and slowly accumulated, never heated.

A cargo of coal from another seam which has never heated in the pit banks, was discharged on a wooden wharf at Boston, where air played over, under, and round it, and took fire within a very few weeks.

Mr. Adams, in Appendix V., describes the circumstances of cases of spontaneous combustion at Montreal and Quebec, and shows that fires occurred in positions similar to both the cases mentioned by Mr. Armington, at Brooklyn and Troy, in the fine, compact, and damp coal directly under the dump, and in the coarse coal with freer ventilation, as Dr. Howe confirmed in his statement. Mr. Lee's diagram shows how very local the origin of a fire may be, and Mr. Armington mentions a similar case, though it is evident there is at the same time a general elevation of the temperature of the mass. Dr. Howe and Mr. Armington agree that the best American gas coals, containing less sulphur than our provincial coals, are just as liable to spontaneous combustion. Messrs. Perkins and Job of Boston and New York, who have been in the coal trade for over 30 years, hold similar views, and further state that they do not know of one case of fire at sea in a vessel laden with Nova Scotia coal. The heating of the Cumberland coal of Maryland in the dockyard of Halifax, and its ignition at Bermuda, prove that that that excellent steam coal is not free from danger. The heating of the albertite, which is entirely free from sulphur, was thought to be due to the absorption of oxygen, as in the case of charcoal, but a doubt has been raised since it has been shown that 5 per cent. of the shipments consist of bituminous shale (the walls of the vein from which the albertite has been extracted by heat and pressure), which are said to contain a small per-centage of sulphur. The percentage, however, is so small that it is hardly sufficient to account for the general heating observed.

The loss of the barque "Senator," in October 1875, was stated to be due to the spontaneous combustion of semibituminous coal, but inquiries disclosed that besides the 1,200 tons of Broadtop semibituminous coal, she carried 400 tons of small Charfield bituminous, which was taken on board in wet weather. She was ventilated. Respecting anthracite, Major Morris, of the Reading railway, writes, he has loaded a large number of vessels up to 2,000 tons capacity with anthracite for Chili, San Francisco, Japan, &c., and in no case were precautions taken or proved to be in future necessary. The Hon. S. Sloan, President of the Delaware, Lackawanna, and Western Railway confirms the opinion that there is no necessity for care against fire with anthracite. Dr. Howe, in his statement, points out the opinion of the workmen, that all coal heaps pass through a heating process. I would remark in connexion with this, that this stage must be slight in the heaps of fresh mined coal, as it has not hitherto been noticed, but that it appears to be developed when the coal is turned over and wetted, just as heaps of pyrites and decaying vegetable matter. Regretting that the information on the matter at my command is not more specific and relevant in character,

I have, &c.
HENRY S. POOLE, M.A., F.G.S., &c.
Inspector of Mines for Nova Scotia.

The Rt. Hon. HUGH C. E. CHILDERS, M.P.,
Chairman of the Royal Commission
on the Spontaneous Combustion of Coal at Sea.

I.

A PAPER read before a MEETING of the NEW ENGLAND ASSOCIATION of GAS ENGINEERS, February 1875, by Colonel J. H. ARMINGTON.

During the month of December 1874 it was discovered that the coal stored in the coal sheds of the Brooklyn Gaslight Company was on fire. It has been the practice for many years at these works to introduce gas tubing, 1½ inches in diameter, in the various piles of coal, in order that the temperature should be known, and a daily record made of it. In this way a careful watch has been kept, and whenever the temperature changes it has been immediately noticed, and the necessary steps taken to prevent further danger, by at once using the coal showing the extra heat. The coals purchased by this Company during 1874 were supposed to be so free from sulphur and iron that the tubing was not introduced, and consequently the high temperature was not known until that particular odour was noticed which is always given off when spontaneous combustion occurs in gas coals. At the same time that the peculiar odour was apparent, smoke appeared in the shed, and operations were at once commenced to find the fire. After three days and nights of continual labour, and the removal of about 900 tons of coal, the fire was discovered to have taken place in a pile of Penn coal; and, when reached, it proved to be confined to a space of less than 12 feet in diameter and 4 feet in height. The entire mass was so thoroughly baked together that it had to be broken in pieces with bars in order to remove it. After removing it from the shed it was piled in a heap on the wharf, and in a few minutes after being exposed to the air the whole mass was in a blaze. Only two instances of this kind have ever come to the knowledge of the writer, of spontaneous combustion in our domestic coals; and I believe it is the general opinion of gas engineers that no danger is to be apprehended from stowing these coals in piles of the ordinary height that we are accustomed to see about the gasworks in this country.

The first instance of this kind that came to my knowledge occurred in Troy, N.Y., and the cause was said to have been from its being so solidly packed, the fire having taken place directly underneath the dumping hatch of the coal shed. The other was in the works of the New York Gas Company, and the cause of this fire I have never heard stated. The fire in the shed of the Brooklyn Gas Company commenced between the tracks, and in that portion of the coal which rolled from the piles when it was dumped, on each side of it; it must therefore be evident to us all that this fire could not have occurred from the same cause as that mentioned in the coal sheds at Troy.

From whatever cause the fire did proceed (and in this case I am unable to give any satisfactory reason for it), it shows us that there is danger of spontaneous combustion in our coal stores, and that the necessary precautions should always be taken to discover any overheating which may be likely to recur. Having been asked how the pipes are prepared in which the thermometers are inserted, I will state, that in one end of a pipe a solid pointed plug is welded to close the end and to better adapt the pipe for being driven down through the coal; above this plug a dozen or more holes, ½ inch in diameter, are then drilled, giving the necessary openings for the air to enter.

The temperatures, which are taken regularly, vary from day to day, and are always found to be higher than the atmosphere. By an examination of the tables of temperature accompanying this paper, it will be seen that this variation is very great, and proceeds from some cause which is well worthy of our consideration.

During the past week it was discovered that the temperature was rising in a pile of Newcastle coal, and when the thermometer indicated 200° men were set at work to turn the coal over, and the fire was found in one of the spruce uprights which supported the trestlework of the track from which the coal was dumped. This coal had been piled about seven months, and the fire was directly under the track from which the coal was dumped. The fire was confined to a space about 8 inches in diameter, and had not, so far as could be discovered, affected the coal except by disintegration. It will be noticed in this case that the heat was sufficient to ignite the wood, and had it not been discovered, the burning of the wood would have ignited the coal. The high temperature that had been noticed in this pile of coal previous to the discovery of the fire was 120°; and while the coal was being removed, another thermometer, within 10 feet of the one indicating 200°, only indicated 118°.

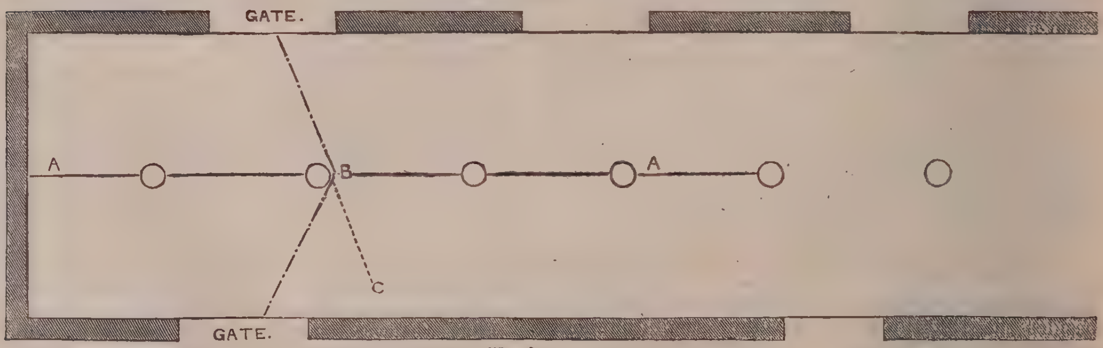
TABLE of TEMPERATURES of DOMESTIC COALS (Penn and Westmoreland), NEWBURGH PROVINCE (Nova Scotia) and NEWCASTLE, G.B.

Date.	Domestic Coals.	Atmosphere.	Date.	Domestic Coals.	Atmosphere.	Newburgh, outside.	Date.	Domestic Coals.	Atmosphere.	Province, outside.	Date.	Domestic Coals.	Atmosphere.	Date.	Domestic Coals.	Newcastle.	Atmosphere.	Date.	Domestic Coals.	Newcastle.	Atmosphere.
1873			1873				1873				1874			1874				1874			
July 3	84	84	Aug. 1	95	78		Sept. 1	101	74		Jan. 2	76	42	Nov. 5	74	122	45	Dec. 1	69	112	23
,, 5	85	78	,, 2	93	79		,, 2	98	70		,, 3	76	38	,, 6	90	126	51	,, 2	69	114	32
,, 7	87	68	,, 4	95	74		,, 3	98	73		,, 4	76	50	,, 7	93	129	49	,, 3	69	113	42
,, 8	87	70	,, 5	93	70		,, 4	98	75		,, 5	76	44	,, 12	91	126	38	,, 4	69	112	42
,, 9	89	72	,, 6	92	74		,, 5	98	78	148	,, 6	76	35	,, 13	91	121	31	,, 5	69	112	27
,, 10	90	75	,, 7	91	74		,, 6	98	68	148	,, 7	76	55	,, 14	91	121	25	,, 6	69	111	35
,, 11	89	68	,, 8	92	76		,, 8	100	60	148	,, 8	76	52	,, 16	92	116	38	,, 7	68	120	40
,, 12	89	68	,, 9	91	75		,, 9	100	64	148	,, 9	76	32	,, 17	88	115	44	,, 8	68	110	39
,, 14	93	76	,, 11	93	70		,, 10	99	60	146	,, 10	76	38	,, 18	87	116	54	,, 9	68	110	29
,, 15	93	78	,, 12	93	72		,, 11	99	60	148	,, 11	76	35	,, 19	86	118	34	,, 10	66	108	37
,, 16	93	76	,, 13	93	65		,, 12	99	64	148	,, 12	74	30	,, 20	87	119	38	,, 11	67	107	36
,, 17	93	74	,, 14	95	64		,, 13	99	65	148	,, 13	64	26	,, 21	84	116	42	,, 12	67	108	30
,, 18	93	70	,, 15	95	65	114	,, 14	98	48	148	,, 14	66	30	,, 23	84	114	43	,, 14	63	112	44
,, 19	93	65	,, 16	95	72	114	,, 15	98	48	144	,, 15	66	22	,, 24	85	117	53	,, 15	66	112	15
,, 21	93	68	,, 18	95	66	114	,, 16	98	60	140	,, 16	64	15	,, 25	84	115	40	,, 16	66	107	17
,, 22	92	72	,, 19	95	70	112	,, 18	98	55	140	,, 17	64	18	,, 26	84	115	27	,, 17	64	105	36
,, 23	92	74	,, 20	95	70		,, 19	98	60	140				,, 27	81	114	29	,, 18	65	105	26
,, 24	93	78	,, 21	95	70		,, 20	98	62	140				,, 28	73	114	38	,, 19	64	106	34
,, 25	93	82	,, 22	95	75		,, 22	98	52	138				,, 29	70	112	35	,, 21	63	107	26
,, 26	93	82	,, 23	95	77		,, 23	98	55	132				,, 30	69	114	25	,, 22	62	105	26
,, 28	93	78	,, 25	93	60	124	,, 24	98	60	132								,, 23	60	109	28
,, 29	93	78	,, 26	101	68		,, 25	98	64	132								,, 24	60	107	35
,, 30	93	78	,, 27	102	68		,, 26	98	64	132											
,, 31	93	78	,, 28	102	65		,, 27	98	64	132											
			,, 29	101	66		,, 28	98	68	132											
			,, 30	102	65	88	,, 29	98	70	132											
							,, 30	98	60	132											

II.

A STATEMENT made by MR. LEES, President of the New York Gas Company.

21st Street.



Yard.

STATEMENT from New York Gas Company.

A A is a wooden partition to separate coal.

Over B is an open ventilator in the roof through which the smoke was first seen. The gates in the yard and on 21st Street were opened and the coal removed to B (see red dots), where the partition was found charred and burning at about 15 or 20 feet from the ground. The fire was then traced from B to C (see black dots), growing deeper all the way, until at C it was found in the form of a red-hot coke on the floor. There was no wood at or near C. Nor was any burned except on the partition at B. The floor is of stone.

The whole quantity of coal burned did not exceed (say) 30 tons, of which the centre, C, was thoroughly carbonized.

The coal was Penn and Westmoreland mixed and had been in about six months.

III.

On the SPONTANEOUS COMBUSTION of CHARCOAL, by the HON. KNOWLTON S. CHAFFEE, of Boston.

In answer to your questions on the spontaneous combustion of charcoal I will say, First, that I have been engaged in the business about 30 years, and have annually handled nearly a million of bushels. Second, the coal is made at all seasons of the year, and chiefly burned in brick-kilns. Third, the Boston market requires that the coal shall be free from dust, therefore it is screened over half-inch mesh screens. The screenings if dry and piled in heaps of 100 bushels or more, whether on the ground or a floor of any kind, either under cover or outside, will heat and produce spontaneous combustion in a very few days. The heat will extend through the whole pile. You ask, whether wet or dry? Of course if very wet it cannot heat, and if perfectly dry it would not as long as it remained so, but it will inhale moisture enough from the ground and atmosphere to produce heat; and if in a large pile, it will bear several heavy showers before it would become wet enough to prevent heat, as the water would not penetrate sufficiently through it. There is no danger of regular charcoal heating, as its size will admit the circulation of air. If pulverized to a fine powder it will be so solid that it cannot heat, but the screenings to which I refer, when dry, will heat under almost any circumstances.

Coal burned in pits the old-fashioned way, covered with earth, never troubled us, and I do not believe will heat.

IV.

STATEMENT of ESTES HOWE, ESQ., M.D., President of the Cambridge Gas Company.

* * * * In November 1873 a spontaneous fire occurred in the coal shed of the Cambridge Gaslight Company, Massachusetts. The shed is of wood, 300 feet long, 70 feet wide, situated on a wharf, and receives the coal directly from the vessel. The coal is hoisted to the height of the run which extends the whole length of the building through the middle, at a height of 22 feet. The floor at this time was of earth. The coal affected was a mixture of Glace Bay and Caledonia, Nova Scotia coals. The run is supported by posts of spruce 10 inches square and 10 feet apart each way. The coal had been put in rapidly and was all dumped from the run. It was thought to be finer than usual, and probably more damp. There was some idea that rain had blown in through the hatchways and dripped down at the place affected. This is not certain. The fire was first indicated by steam and smoke escaping. Thinking the trouble might be superficial, men were put on to shovel the coal over, but at one point, at which one of the posts was the centre, it was found that the heat increased on going down. At a point about five feet from the surface the post was completely converted into charcoal, and that without any appearance of redness, no live fire being found for two or three feet below this point. The coal surrounding the post was agglutinated, and in the first stage of coking. The fire was followed down to the floor, and a very large quantity of coal was removed. At a point about 20 feet distant, around another post, the same heating, though to a less degree, occurred. Some heating in the upper part of the coal heap at a point nearly 200 feet distant was also observed, but shovelling over about four or five feet of the top put an end to it. A similar difficulty occurred in a pile of Cape Breton coal in August of this year. It was early discovered, and was confined to a limited space directly *under the dump*. The coal was fine and closely packed, and possibly unusually damp. There was no post or other wood concerned in this case. Nor was there any tendency to heat shown in the rest of the coal. For many years I have noticed my coal piles showed a tendency to heat, and I have had to remove coal which otherwise I feared would ignite.

I have visited the Brooklyn, New York, and Manhattan gas works, and have made inquiries of many in relation to the matter of spontaneous combustion. I find that it has taken place in charcoal, albertite, Newcastle coal, all Cape Breton coals, and all trans-Allegheny coals, that is, in Penn, Westmoreland, Youghiogheny, Newburgh, Orrel, Fairmount, &c. The first three are taken from the Pittsburgh main seam at different points, and they are the purest and best American gas coals. In every case, except one at the Manhattan, the coal affected was fine and very closely packed. In many, but not in all cases, the trouble seemed to originate where the coal was dumped.

My own observations have led me to think that a certain process of heating in piles of coal is very general, if not universal. The observations of Mr. Armington, by means of gas pipes driven down through or placed in the coal as it is stored, confirms this idea, and the engineer of the Manhattan gasworks makes the same statement. Mr. Armington has in his coal shed, which is 210 by 180 feet, and now stored 22 feet deep with coal, mostly American, 60 of these tubes, with thermometers in them, of which a daily register is kept, As having a bearing on this matter I give you some figures from his register. The tubes are numbered. A group of tubes quite near each other, numbered 1, 2, 3, 4, were placed at a point which seemed hot, which was filled 14 feet deep, and the filling discontinued on account of the heat.

PENN COAL.

1875.

No.	Oct. 13.	Oct. 14.	Oct. 22.	Nov. 13.	Nov. 30.
	°	°	°	°	°
1 - -	116	116	98	90	84
2 - -	120	126	120	100	92
3 - -	116	118	104	98	80
4 - -	74	76	76	80	68

FAIRMOUNT COAL.

	118	118	112	56	56
	106	106	102	62	58
	102	102	100	70	56
	98	98	90	52	62
	90	88	92	54	64
	86	84	84	56	70

I would add that the labourers in the coal sheds all agree that the coal heaps go through a heating process, and when that is once over they think it does not recur.

There is a very general impression that the presence of wood leads to spontaneous combustion. It seems to me that this impression arises from the fact that wood, especially all varieties of fir, ignites at a much lower temperature than coal, and if coal is heated in the presence of wood, the wood first takes fire.

I may add that my experience shows that if air is excluded the process of combustion in the compact heap of coal is slow, and time may be taken. I have yet to hear of a case where there was a general heating to the blazing point of a pile of coal. There seems to be a nucleus of heated coal, which gradually extends upwards, spreading as it rises. When, however, any considerable quantity is heated, gas is given off, which coming to the air may be lighted and burn rapidly.

It seems to me that the plan of observations of temperature by means of gas tubes in the coal is easily applicable to coal on board ship, and would afford most valuable information of the condition of the cargo.

V.

STATEMENT of ROBERT C. ADAMS, ESQ., of Montreal.

The Nova Scotia coals imported in 1873 by the Grand Trunk Railway were piled in open spaces in three localities. One pile, all of Cape Breton coal, of 12,000 to 15,000 tons, another of 12,000 tons, chiefly of Cape Breton, and the third chiefly of Nova Scotia (Pictou) with some Cape Breton. Much of the coal was wet when piled, for the season was rainy. Late in the fall, when the coal had been two or three months in yard, the two piles consisting of Cape Breton coal were discovered to be on fire, and the third smoked. The coal was dug through, turned over, and the fire extinguished by this means. Water was also used freely, but without much effect. The coal was very fine. The third heap did not ignite.

The gas company last winter had about 9,000 tons of Cape Breton coal stored quite dry in a covered building, piled 12 to 14 feet high. The coal was small. After being stored five or six months fire broke out among the larger sized coal that ran out to the walls, and against the posts supporting the roof. The fire ran up the posts, ignited the roof and the platforms over the coal. This caused the whole surface of the coal to catch fire, and it burned to a depth of 2 feet, being finally extinguished by water. The fire below was subdued by digging through and carting away the coal. It was found that among the small coal in the centre of the pile there was no fire. This causes the engineer to think that ventilation assists combustion, the fire having burned in the large coal where there was the most circulation of air. The Grand Trunk Railway adopting the opposite theory, have placed ventilating platforms and chimneys through their coal heaps.

An illustration of the danger arising from wet coal is furnished by a vessel lately arrived with Cape Breton coal. She loaded in the rain, and the wet coal under the hatches was nearly in a state of combustion, while the drier coal in the ends of the vessel was cool.

VI.

Office of the Boston Gas-Light Company,
No. 24, West Street, Boston,
30th Nov. 1875.

DEAR SIR,

WE have been purchasers of Albert coal for more than twenty years, and during the larger proportion of that time have received a very considerable share of the products of the mine.

The published analysis of the coal shows an entire freedom from sulphur.

It has usually been stored by us in large sheds, and piled to a height of twenty or more feet. Latterly, it has been piled in the open air, without any protection from the weather.

During the period of time that we have had the coal, it has heated four times—but more seriously during the autumn of 1874 than ever before. On two seasons before this, it heated and fired the timber supports of the shed in which it was stored, for which no explanation at the time could be found.

Last year a special examination of the coal was made for the purpose of ascertaining the cause. It was found that a certain portion of the lining of the mine, as well as the seam in the coal, contained pyrites. Thin shale formed about five per cent. of the mass, and with the disintegrated condition of the coal, being mostly of the description called "slack" at the mines, sufficient elements were found to account for heating, fusion, or even fire.

The following analysis, Dr. S. Dana Hayes and Prof. P. Sterry Hunt shows the effect of "heating":—

	Albert coal of good quality.	Albert coal after "heating."
Bituminous and volatile matter	56·05	4·65
Coke	40·81	85·89
Ash (red and mostly oxide of iron)	3·14	9·46
	100·	100·

Whenever the coal showed symptoms of heating, it was shovelled over in the heaps for the purpose of cooling it.

Yours respectfully,
W. W. GREENOUGH.

Henry S. Poole, Esq.,
Inspector of Mines,
Halifax, Nova Scotia.

(d.) PAPER and STATISTICS from the ROYAL MAIL STEAM PACKET COMPANY.

THE following information and statistics were furnished by the Royal Mail Steam Packet Company with reference to the Inquiry of the Commission.

Royal Mail Steam Packet Company,
No. 55, Moorgate Street, London, E.C.
16th November 1875.

SIR,

I AM instructed by Mr. Marshall to forward you the following documents which you wished to be furnished with for the information of the Royal Commission on the Spontaneous Combustion of Coals, viz., Copy of instructions to superintendents at ports of shipment; Table showing the description, quantities, destination, &c., of the various shipments for the Company during the last three years; and I will be glad to furnish you with any further information you may wish for the information of the Committee.

I am, sir, your obedient servant,
T. C. W. DYER,
pro Secretary.

James Carmichael, Esq.,
32, Abingdon Street,
Westminster.

Royal Mail Steam Packet Company.

Instructions for Mr. Pope, Coal Inspector.

1. By accompanying form, marked No. 1, you will observe that all coals purchased by the Company should be of the best quality, and fresh wrought; and it will be your duty to see that the coals shipped are the produce of the mines and of the description stipulated for. Moreover, that all conditions of purchase are duly fulfilled. In order that you may have the necessary power to ascertain this, you have permission to examine the pit-head tally books at all times, and you will be duly advised of all new contracts.

2. The loading of each vessel must be personally superintended during the shipment.

3. Each vessel must be examined before receiving stiffening to see that the hold is properly prepared and cleaned to receive it, and that the coals are kept strictly clear of the ballast, which, if of sand, is injurious to them.

4. The vessel to be again examined when totally unballasted to ensure the hold being clean throughout.

5. The weight of the coals at the tips to be carefully superintended and checked, and subsequently compared with the colliery weights, the result to be reported on Form No. 2, and it is important that the weighing machines at the tips should be tested as to their correctness as often as opportunity arises.

6. In cases where the coals have to be screened at time of shipment, it must be seen that this is properly done, the screens kept open and in no way choked up with the small. The small coal to be drawn, weighed, and properly allowed for.

7. A check must be kept of the tares on the coal waggons, as differences have been frequently found to exist between the weights painted on them and the actual tares. Care must be taken that this is not done in dry weather, and you can exercise your own discretion as to the mode and time of doing this. You will find a memo. marked No. 3, showing how the weights are recorded and the averages arrived at in cases of this kind.

8. The Alexandra Docks at Newport being fitted up with tips and weighing machines, it is preferable that the ships chartered by the Company should load there, and if possible under those tips having anti-breakage boxes. You will see that the latter are always used at first to convey the coal easily into the hold, and, in conjunction with this, that the staith is lowered sufficiently, or as much as possible into the hold, and so prevent much breakage.

9. Although the Company undertake to load the vessels chartered by them in a certain number of days, it must be understood that the contractors hold them harmless in this respect (see Form No. 4), and in case of any question of this kind arising the captains must be referred to the contractors for a settlement, but the facts are nevertheless to be fully reported to the secretary.

10. When a ship is chartered, copy of the charter-party will be at once sent you, and no coals are to be shipped (except those required for stiffening) until the ship be wholly unballasted and ready for cargo, when the loading days may be said to commence, supposing the ordinary notice for cargo has been given to the contractors.

11. You will carefully ascertain what coals the vessel has on board (if any) previous to cargo being shipped. This should be kept apart, and made up to one per cent. of cargo. Should the captain have any wood on board it can stand against a proportionate quantity of coal, if you think it advisable.

12. Bills of lading are issued on the Company's form, No. 5, in sets of four; two of which are to be sent to the Company's office, one by the ship for the Company's agent, with the copy of charter-party, and the other for the captain. The captain must sign for the cargo on the bills of lading as per dock or railway weights.

13. The captain must sign (on the back of the bills of lading) for the advance which will be paid in London in terms of clause in charter-party. Great care is to be taken in calculating the freight, which is to be inserted on the bills of lading, deducting the advance if any. It is seldom more than one third of the freight is advanced, but in no case (unless with the Company's sanction) is the advance to exceed the amount stated in charter-party. The captains can, however, take what lesser amounts they choose, provided the same be not lower than 50*l.*, except in cases of very small ships.

14. A form of certificate, No.   , is to be filled up for each vessel which may be loaded, and sent to the Company's office with the bills of lading. On the back of this certificate the colliery and tip weights are to be inserted, also the quantity of small extracted, which deduction from the tip weights would, in the case of double-screened coal, form the bill of lading quantity.

15. The contract for coals required at Southampton has been made with Messrs. D. Davis and Sons for their double-screened Merthyr coal. A steamer is sent by Messrs. Wm. Hill & Co. about once a week for these supplies, and the Company pay for the same in accordance with the *delivery* weight at Southampton. It is, however, necessary that in addition to the usual inspection you should ascertain the quantity shipped, and inform the Company's superintendent at Southampton (Captain Bevis) immediately a steamer is loaded of the quantity she has on board.

16. A daily report (No. 6) should be made to the Company stating the quantity of coals shipped during the day, vessels in port when stemmed for cargo, and all other information which you may consider the Company should be acquainted with. It would be interesting also if you stated any information you might glean about new

pits being opened, alterations in wages, strikes, &c. of colliers.

17. Before the invoices for coals are sent to this office for payment they should be certified by you as correct.

18. You will send a note of your travelling expenses to the secretary monthly, as well as a note of your salary, a cheque for which will be sent to you.

19. A demand is to be forwarded to the secretary every three months for all stationery required, and the supplies will be sent from the Company's printers. Samples of same are to accompany your demand, but in case you should have to purchase any stationery, &c., a receipted invoice for the same is to accompany the monthly account.

20. The Company's agents at their coaling depôts are:—

Lisbon -	Messrs. R. Knowles & Co.
Cape de Verds	Messrs. Millers and Nephew.
Rio de Janeiro	Thomas Hollocombe, Esq.
St. Thomas -	J. B. Cameron, Esq., Superintendent.
Jamaica -	Captain Dix, Superintendent.
St. Lucia -	F. Peter, Esq.
Colon -	J. H. Leverick, Esq.
Havana -	J. V. Crawford, Esq., H.B.M. Consul.

STATEMENT showing the NAMES and QUANTITIES of the various Descriptions of COAL shipped by the ROYAL MAIL STEAM PACKET COMPANY during the Three Years from November 1872 to October 1875.

Name of Coal.	Tons shipped.	Destination.								
		Southampton.	Jamaica.	St. Thomas.	Havana.	St. Lucia.	Colon.	Rio.	C. Verds.	Lisbon.
Davis' Merthyr - -	97,395	12,355	23,211	8,331	6,686	7.584	1,524	20,326	12,228	5,146
Ricca Rock and Black Vein	85,847	47,362	12,500	2,957	3,447	878	1,627	6,559	5,564	4,953
Ocean Merthyr - -	15,632	—	2,582	3,697	442	1,302	632	3,911	—	3,066
Tredegar - - -	13,269	—	2,600	2,953	705	1,300	—	2,515	1,661	1,535
Powell's Duffryn - -	22,784	22,784	—	—	—	—	—	—	—	—
Ebbw Vale and Abercarn -	17,510	—	5,281	2,705	—	3,181	—	1,860	2,959	1,524
Wilkesbarre (American) -	7,588	—	3,159	3,125	706	598	—	—	—	—
Bryn Mally - -	5,307	—	4,468	839	—	—	—	—	—	—
Westminster Brymbo -	16,248	—	4,632	6,166	1,699	—	—	3,247	504	—
Jonassohn's W. Hartley -	457	—	—	—	—	457	—	—	—	—
Russell's New Black Vein -	550	—	—	—	—	—	—	—	376	174
Fothergill's Aberdare -	3,900	—	418	1,261	300	—	—	—	519	1,402
Coed Talon - - -	2,215	—	896	—	—	505	—	—	814	—
Cyfartha - -	2,012	—	968	500	—	—	—	—	544	—
Wyndham Merthyr - -	803	803	—	—	—	—	—	—	—	—
Hood's do. - -	35,284	20,776	1,299	2,397	599	1,370	—	6,101	2,742	—
Nantyglo and Blaina -	22,009	4,408	5,360	2,545	—	—	—	4,662	2,559	2,575
Grange Coal - -	1,343	—	552	791	—	—	—	—	—	—
Wear do. - -	335	335	—	—	—	—	—	—	—	—
Insolis Merthyr - -	779	—	—	779	—	—	—	—	—	—
Wills' Merthyr - -	587	—	—	—	587	—	—	—	—	—
Blaenavon - - -	157	—	—	—	—	—	—	—	—	157
Dinas Merthyr - -	17,001	816	5,845	4,135	1,203	—	—	3,499	1,503	—
Tylacoch - - -	817	817	—	—	—	—	—	—	—	—
Pumpquart Anthracite -	475	—	—	—	—	—	475	—	—	—
Maesteg Merthyr - -	1,330	—	—	—	—	—	—	—	742	588
Davison's W. Hartley -	378	—	—	—	—	—	—	—	—	378
Cowpen Hartley - -	477	—	—	—	—	—	—	—	—	477
Stewart's West do. - -	470	—	—	—	—	470	—	—	—	—
Ewloe Hall - - -	223	223	—	—	—	—	—	—	—	—
Bebside W. Hartley -	198	—	—	—	—	—	—	—	—	198
Londonderry Hartley -	2,181	836	1,095	—	—	250	—	—	—	—
Scotch (name unknown) -	456	456	—	—	—	—	—	—	—	—
Hartley Main - -	2,343	1,901	—	—	—	—	—	—	—	442
Bentinck W. Hartley -	470	470	—	—	—	—	—	—	—	—
Wrexham and Acton -	335	—	—	335	—	—	—	—	—	—

Number of vessels employed for foreign ports - - 454
Do. do. Southampton (estimated) - 138

The only known case of coals heating or taking fire on board or in stores took place at Cape de Verds in 1873 amongst a heap of Coed Talon and Bryn Mally coals.

(e.) PAPER and STATISTICS from the PENINSULAR and ORIENTAL STEAM NAVIGATION COMPANY.

The following information and statistics were furnished to the Commission, with reference to their Inquiry, by the Peninsular and Oriental Steam Navigation Company:—

MEMORANDUM relative to SPONTANEOUS COMBUSTION of COAL on SHORE, and in BUNKERS of the COMPANY'S STEAMERS.

Cases of spontaneous combustion in the Company's coal stacks have taken place, but taking into consideration the heavy stocks kept up, and the extremes of heat and moisture to which they are exposed, such instances are very rare.

About 10 years ago a case of firing occurred near the bottom of a large stack of coal at Calcutta, when about a thousand tons of coal had to be removed; it was then discovered that for a space of about six feet square the coal was in a very heated and charred condition.

At Aden, in July last, two cases of heating occurred, one in the heart of a stack of North Wales coal, and the other near the surface of a heap of Bebside Hartley (Newcastle) coal.

In the course of the last 10 years three or four cases of explosion in the bunkers of the Company's steamers have taken place owing to the *close* covers of the bunkers having been kept on too long; it is, however, one of the Company's standing orders to their chief engineers, that "the close "covers of the bunkers must only be put on when the "decks are being washed, or in very bad weather, *grated* "covers being used on all other occasions, so as to permit "of the escape of the gas generated in the coals."

(For the same reason it would be well were the hatches of colliers lifted on all occasions where practicable.)

In the Company's experience, cargoes of the South Wales coal are much less liable to spontaneous combustion than either Newcastle or North Wales coal.

The arrangements for stacking the Company's coal are as follows:—

Malta -	- In sheds in the open, and in barges ready for shipment.
Brindisi -	- In the open.
Aden -	- " "
Bombay -	- Under sheds.
Calcutta -	- " "
Galle -	- In sheds and in the open.
Singapore	- In sheds.
Hong Kong	- "
Shanghai	- "
Yokohama	- In floating hulk, and in sheds on shore.
King G. Sound	- In floating hulks, and in the open on shore.

P. & O. S. N. Company, London,
5th November 1875.

STATEMENT showing the NUMBER of COLLIERS LOADED on COMPANY'S ACCOUNT, TONS SHIPPED, VESSELS LOST at SEA or BURNT, with the DESCRIPTION of COAL loaded.

1873–74.

Number of colliers loaded	436

TONS SHIPPED.

North Country	154,628
North Wales	117,747
South Wales	279,125
	551,500

VESSELS LOST AT SEA.

Sailed.	Ship.	Destination.	Description of Coal.
January 4th, 1873	"Oneiza"	Aden	Cowpen Hartley.
March 24th, 1874	"Atlantic"	"	Hancock's Brymbo.
October 14th, 1873	"Shalimar"	Bombay	West Hartley Main.
December 27th, 1873	"Broughty Castle"	"	"
September 23rd, 1874	"Norah Graeme"	"	Battersby's N.W. Main.
August 27th, 1874	"Nellie"	Brindisi	Powell Duffryn.
March 5th, 1875	"Nellie Hastings"	Singapore	Scott's Brymbo.
	VESSELS BURNT AT SEA.		
June 12th, 1874	"Euxine"	Aden	Howard's W. Hartley.
" 18th, 1874	"Oliver Cromwell"	"	"
July 2nd, 1874	"Calcutta"	"	"
December 8th, 1873	"Moorefoot"	"Bombay"	"

P. & O. S. N. Company,
July 6th, 1875.

A. M. BETHUNE,
Secretary, P. & O. S. N. Co.

STATEMENT showing QUANTITIES and DESCRIPTION of all COALS shipped by the Peninsular and Oriental Steam Navigation Company from Great Britain to the under-mentioned Ports from 1st January 1874 to 1st January 1875.

To	Colliers.	Description of Coals.	Tons.	Remarks.
Alexandria	1	Bebside West Hartley	1,363	
	2	West Hartley Main	760	
	1	Davidson's West Hartley	586	
	3	Insole's Merthyr	2,301	
	1	Jaynes' Tillery	543	
	1	Battersby's N. W. Main	598	
Totals	9		6,151	
Aden	2	Bebside West Hartley	4,019	
	5	Howard's "	8,917	3 burnt.
	1	Carr's "	996	
	1	Bower's "	1,155	
	8	Battersby's N. W. Main	14,316	
	9	Scott's Brymbo	14,937	1 lost.
	1	Laird's Welsh Hartley	1,875	
	4	Powell Duffryn	5,992	
	4	Insole's Merthyr	7,136	
	1	Davis' "	1,412	
Totals	36		60,755	
Brindisi	1	Davidson's West Hartley	1,167	
	1	Carr's "	1,194	
	1	Bebside "	1,016	
	6	Buddles "	6,474	
	18	Powell Duffryn	19,011	1 lost.
Totals	27		28,862	
Bombay	1	Bedside West Hartley	1,500	
	2	West Hartley Main	2,069	100 tons destroyed by fire near Bombay.
	1	Buddles' West Hartley	1,704	
	2	Howard's "	3,729	
	7	Scott's Brymbo	10,732	1 lost and 1 missing.
	6	Battersby's N. W. Main	11,416	
	1	Laird's Wesh Hartley	1,775	
	2	Insole's Merthyr	3,785	
	1	Powell Duffryn	637	
	1	Patent fuel	817	
Totals	24		38,164	
Calcutta	1	Scott's Brymbo	1,100	
	1	Powell Duffryn	2,186	
	1	Davis Merthyr	1,704	
	1	Insole's "	1,804	
Totals	4		6,794	

To.	Colliers.	Description of Coals.	Tons.	Remarks.
Hong Kong	2	Bowers' West Hartley	3,278	
	2	Battersby's N. W. Main	3,261	
	2	Scott's Brymbo	3,146	
	2	Insole's Merthyr	3,061	
	2	Powell Duffryn	5,079	
	1	Hood's Merthyr	1,248	
Totals	11		19,073	
Malta	1	Londonderry Hartley	963	
	3	Buddles' West Hartley	2,383	
	2	Bebside ,,	1,865	
	2	Carr's ,,	2,460	
	1	West Hartley Main	1,940	
	1	Battersby's N. W. Main	1,875	
	32	Powell Duffryn	33,044	
	1	Insole's Merthyr	1,277	
	1	Davis' ,,	994	
	2	Davis' Maestag	2,927	
	1	Aberdeen Merthyr	1,204	
Totals	47		50,932	
King George's Sound	1	Davidson's West Hartley	1,605	
	3	Battersby's N. W. Main	5,895	
	2	Scott's Brymbo	3,846	
	2	Powell Duffryn	3,750	
Totals	8		15,096	
Point de Galle	2	Battersby's N. W. Main	1,373	
	5	Scott's Brymbo	6,005	
	1	Cowpen Hartley	702	
	4	Howard's West Hartley	4,243	
	3	West Hartley Main	3,657	
	10	Insolve's Merthyr	12,822	
Totals	25		28,802	
Shanghai	2	Powell Duffryn	2,996	
	1	Insole's Merthyr	1,140	
Totals	3		4,106	
Singapore	1	Davidson's West Hartley	1,245	
	1	West Hartley Main	1,640	
	1	Scott's Brymbo	1,775	
	9	Insole's Merthyr	12,837	
	1	Powell Duffryn	1,337	
Totals	13		18,834	
Suez	1	West Hartley Main	1,357	
	2	Powell Duffryn	2,860	
Totals	3		4,217	
Yokohama	2	Powell Duffryn	3,345	
	1	Insole's Merthyr	1,872	
Totals	3		5,217	

122, Leadenhall Street, London, E.C.
15th November, 1875.

A. M. Bethune,
Secretary, P. & O. S. N. Co.

Summary.

Ports.	No. of Colliers.	Tons.
Alexandria	9	6,151
Aden	36	60,755
Brindisi	27	28,862
Bombay	24	38,164
Calcutta	4	6,794
Hong Kong	11	19,073
Malta	47	50,932
King George's Sound	8	15,096
Point de Galle	25	28,802
Shanghai	3	4,106
Singapore	13	18,834
Suez	3	4,217
Yokohama	3	5,217
Totals	213	287,003

(*f.*) PAPER and STATISTICS from the MERSEY DOCKS and HARBOUR BOARD.

STATEMENT as to the SHIPMENT of COAL at the BOARD'S HIGH LEVEL RAILWAY, LIVERPOOL, and at the BOARD'S APPLIANCES, BIRKENHEAD.

DEAR SIR, Secretary's Office, Liverpool, May 15, 1876.

REFERRING to my letter of the 25th ultimo, the statistical information in relation to the shipment of coal at this port, which has been prepared on the suggestion of the Chairman of the Traffic Committee, is now complete, and I am instructed to hand you the enclosed returns, &c. containing the same, for the use of the Royal Commission.

I am, &c.
EDW. GITTINS,
Secretary.

J. M. Carmichael, Esq.,
Secretary of the Commission on the Combustion of Coal at Sea,
32, Abingdon Street, London, S.W.

SUMMARY OF INFORMATION.

1. List of Vessels and Lighters loaded from January 1st, 1875, to December 31st, 1875.
2. Time loading.
3. Weight and carrying capacity of Waggons.
4. Appliances, capacity of.
5. Rules, Regulations, and Charges.

——	Board's High Level Railway, Liverpool.	Board's Appliances, Birkenhead.
No. 1. LIST OF VESSELS AND LIGHTERS LOADED.	Vessels. See Table A. Lighters. See Table C.	Vessels. See Table B. Lighters. See Table D.
No. 2. TIME LOADING:		
(*a.*) Time required to take in each waggon of coal, say from the time the waggon is hooked to the crane until the coal is tipped down the hatch.	Two minutes	Three minutes.
(*b.*) Quantity per hour that can be loaded into one ship, per crane or hoist.	100 tons per hour per crane, at the quickest speed.	180 tons per hour per hoist is the maximum quantity.
(*c.*) The largest quantity of coal shipped into one vessel per day.	600 tons in one day of 8 working hours.	608 tons, maximum quantity shipped at one hoist in one day of 12 working hours.
No. 3. WAGGONS:	Tons. cwts.	Tons.
Weight of the largest waggon that can be discharged by the appliances.	12 tons gross— Weight of waggon about 4 5 Weight of coal 7 15 12 0	15 tons gross. Weight of waggons 5 Weight of coal 10 15
No. 4. APPLIANCES:		
Capacity of appliances to ship per day of 12 working hours, if fully supplied with coal and ships.	2,200 tons per day	The maximum work done at the coal appliances has been given above (see No. 2), but the speed of tipping being dependent upon the trimming, and taking into consideration the class of vessels that load here, 350 tons of coal per day of 12 hours is a fair average work for each hydraulic tip, which will give a daily shipment by the three tips of 1,050 tons, equal to 326,550 tons per annum. The five hand tips are capable of loading 2,500 tons of coal per day of 12 hours, equal to 777,500 tons per annum.
No. 5. RULES, REGULATIONS, AND CHARGES:		
(*a.*) Rules and Regulations, and Charges.	Copies herewith. See Printed documents marked E. and F.	——
(*b.*) Regulations	- - - - -	See printed document marked G.
(*c.*) Charges	- - - - -	See written statement marked H.

TABLE A.

LIST of VESSELS loaded with COAL, &c. at the Board's High Level Railway, Liverpool, from the 1st January 1875 to the 31st December 1875.

Ship.	Register Tonnage.	Draft.		Coal.	Cannel.	Coke.	Port of Destination.
		Feet.	Inches.				
Maria Casabona	700	19	6	987	—	—	Genoa.
Peerless	860	20	0	1,230	—	—	Calao.
Cheechino	634	19	0	890	—	—	Genoa.
Roberto	422	16	0	550	—	—	Havana.
Lenore	1,236	18	6	776	—	—	Callao.
Jason	490	16	6	722	—	—	Valparaiso.
Esmeraldo	280	15	0	365	—	—	Havana.
Hero	658	19	6	865	—	—	Mantanzes.
Pontiac	698	17	0	844	—	—	Valparaiso.
Tre Lorelle	446	16	6	814	—	—	Genoa.
Laurel McLennan	796	19	0	1,130	—	—	Valparaiso.
Caroline Sainty	219	13	0	327	—	—	Jersey.
Andrea Papa	592	18	6	923	—	—	Genoa.
Dartmouth	873	20	0	1,305	—	—	Valparaiso.
St. Mark	1,870	23	3	400	—	1,092	Coquimbo.
Sir Jno. Laurence	1,090	20	6	1,206	—	—	Valparaiso.
Franceschina	496	19	6	764	—	—	Genoa.
Kerelaw	316	16	0	444	—	—	Monte Video.
Havelock	454	17	0	606	—	—	Havana.
Asteria	210	13	0	345	—	—	Orico.
Carl Gustaff	673	19	0	951	—	—	Havana.
Eleanor Wheatley	111	12	0	116	—	—	Lisbon.
Themis	290	13	0	257	—	—	Demerara.
Croydon	375	16	6	542	—	—	Valparaiso.
Friuso	430	17	6	620	—	—	Havana.
Alfred	232	13	0	350	—	—	Lisbon.
Geo. Skolfield	1,320	20	7	1,372	—	—	Valparaiso.
Dryad	265	15	0	301	—	—	Gibraltar.
Dario	449	16	0	736	—	—	Genoa.
Eurique	582	—	—	659	—	—	Havana.
Emogene	481	—	—	723	—	—	Rio de Janeiro.
Lammerstragen	845	—	—	635	—	—	Hamburgh.
Golden Sheaf	454	14	6	525	—	—	Havana.
Cameos	446	—	—	440	—	—	Oporto.
Jane	636	—	—	917	—	—	Valparaiso.
St. Lucien	276	14	0	333	—	—	Cuba.
Ferdemde Lesseps	387	17	6	501	—	—	Bordeaux.
H. D. Brookman	687	—	—	751	—	—	Havana.
J. E. Brett	531	17	6	672	—	—	Havana.
Manilla	473	17	0	774	—	—	Genoa.
Agincourt	446	—	—	553	—	—	Mauritius.
Jessie Goodwin	327	15	0	454	—	—	Havana.
Angela	673	—	—	898	—	—	Genoa.
Yennia	315	12	4	203	—	—	Demerara.
Montemuza	208	14	6	248	—	—	Bordeaux.
Harry Baily	713	18	3	973	—	—	Havana.
Oracle	1,133	20	8	1,219	—	49	Valparaiso.
Maori	701	20	0	882	—	—	Madras.
Patua	321	—	—	452	—	—	Demerara.
Thomas Woods	520	—	—	689	—	—	Valparaiso.
Kennelworth	694	—	—	910	—	—	West Coast.
Hjemnet	383	—	—	480	—	—	Constantinople.
Vibilia	553	—	—	602	—	—	Cienfugos.
Maria	678	19	0	918	—	—	Genoa.
Lima	892	19	0	1,118	—	—	Valparaiso.
Rio Grande	326	16	0	467	—	—	Cuba.
Don Quixote	425	14	0	432	—	—	Havana.
Quebec	1,454	22	6	1,850	—	—	Valparaiso.
Constantia	560	17	0	581	—	—	Pisagua.
Eudymion	1,298	20	4	319	—	815	Coquimbo.
Helen Marion	693	19	0	—	520	—	Valparaiso.
Queen of Commerce	277	17	0	420	—	—	Havana.
Wimbledon	1,494	—	—	396	—	—	Quebec.
O. D. Witherell	631	15	9	773	—	—	Mantanzas.
W. H. Thomdike	677	15	3	732	—	—	Havana.
Maria Adal	168	—	—	211	—	—	Bordeaux.
Ricardo	148	13	0	175	—	—	Gibraltar.
Chieftain	670	13	0	295	—	—	Quebec.
Voyager	1,356	18	5	1,202	—	—	New York.
Springwood	989	20	10	1,322	—	—	Valparaiso.
Emily	351	13	0	349	—	—	Mantanzas.
Aurelie	548	16	0	693	—	—	Barbadoes.
Antoinette	1,118	19	7	217	—	736	Coquimbo.
Ville de Dieppe	240	16	0	336	—	—	Barbadoes.
Angelo Antonio	595	18	6	864	—	—	Calao.
Simla	1,110	18	3	—	997	—	New York.
Deux Sophie	212	15	0	319	—	—	Bordeaux.
Alexandra	268	14	3	385	—	—	Cardenas.
Ludwig	313	14	0	160	—	—	Antwerp.
Alice Emma	200	13	0	234	—	—	Bordeaux.
Eugene Maria	524	16	9	570	—	—	Bordeaux.
Madre Cerrute	631	19	0	793	—	—	Bordeaux.
Nova Goa	600	19	0	786	—	—	Lisbon.

Ship.	Register Tonnage.	Draft.		Coal.	Cannel.	Coke.	Port of Destination.
		Feet.	Inches.				
Stanhope	770	20	0	1,113	—	—	Calao.
Try Again	153	13	0	221	—	—	Seville.
W. L. Bradley	432	15	0	553	—	—	Mantanzas.
Maori	700	20	2	848	—	—	Madras.
Ricardo	358	16	6	572	—	—	Constantinople.
Trevere	426	18	0	663	—	—	Constantinople.
Wooloomooloo	627	18	0	840	—	—	Cronstadt.
Golden Fleece	1,474	18	6	—	914	—	New York.
Wonder	165	13	0	258	—	—	Oporto.
Aristide	408	16	6	641	—	—	Madras.
Brodrene	673	15	0	271	—	—	Meranichi.
Arfacsad	610	19	0	873	—	—	Constantinople.
Francois Marie	113	12	0	193	—	—	Bordeaux.
Victoria Nancy	111	12	0	174	—	—	Brest.
Cecilia	611	14	0	488	—	—	Valparaiso.
Lothair	860	19	6	1,028	—	—	Valparaiso.
Oueota	1,031	21	7	1,528	—	—	Cape of Good Hope.
Frankfort Hall	727	17	0	491	—	—	Valparaiso.
Conte Serra	300	17	0	423	—	—	Gibraltar.
Birmingham	1,400	18	0	1,226	—	—	Quebec.
Sophie and Emily	98	12	0	165	—	—	Aviro.
Star	1,214	19	6	—	—	796	Coquimbo.
Rona	633	17	0	754	—	—	Anjer.
Memory	255	14	0	286	—	—	Cuba.
Triton	539	19	0	777	—	—	Constantinople.
Compadre	799	17	6	1,061	—	—	Valparaiso.
Erwood	187	13	0	305	—	—	Trieste.
Augusta	200	14	0	341	—	—	Constantinople.
Simari	599	17	0	303	—	—	Valparaiso.
West Derby	820	19	6	943	—	—	Ascension.
Jno. Ritson	513	17	1	674	—	—	Valparaiso.
Grimalda	540	19	0	801	—	—	Calao.
Peckforton Castle	1,088	21	7	1,170	—	—	Valparaiso.
Clevedon	697	17	7	890	—	—	Valparaiso.
Siberia	1,271	15	0	655	—	—	Quebec.
Hedwidge	898	19	0	1,026	—	—	Callao.
Wellington	393	17	0	627	—	—	Alexandria.
Celeste	597	17	0	853	—	—	Madras.
Camelo	200	16	0	306	—	—	Constantinople.
Vanguard	687	18	3	676	—	—	Valparaiso.
Castlehead	825	18	0	1,156	—	—	Valparaiso.
Biagguio	496	19	0	794	—	—	Genoa.
Antigua	287	16	0	368	—	—	Demerara.
Japan	394	17	0	559	—	—	Valparaiso.
W. H. Harkiss	482	16	0	594	—	—	Valparaiso.
Lady Franklin	364	16	6	608	—	—	Alexandria.
Naomi	126	12	0	208	—	—	Restronguet.
Elizabeth Taylor	261	14	0	374	—	—	Havana.
Sophie Helena	536	16	4	679	—	—	Valparaiso.
Aveure	420	17	0	635	—	—	Genoa.
Archencairn	287	15	0	439	—	—	Constantinople.
Northumbria	599	16	0	825	—	—	Valparaiso.
Camana	592	16	6	676	—	—	Valparaiso.
Arrogant	360	13	0	433	—	—	Bordeaux.
Peppo	397	16	0	580	—	—	Varna.
Globe	736	18	0	836	—	—	Valparaiso.
Romola	88	12	0	—	152	—	St. Petersburgh.
J. S. Barker	1,059	21	7	1,554	—	—	Madras.
Herrington	288	16	0	449	—	—	Malta.
Onyx	81	—	—	137	—	—	Bordeaux.
Album	490	18	0	732	—	—	Genoa.
Spectre	149	11	0	—	228	—	St. Petersburgh.
Maria Joseph	87	11	0	131	—	—	Bayonne.
Gio Bonos	618	18	0	978	—	—	Genoa.
Ursule	94	11	0	147	—	—	Bayonne.
Bossuet	650	18	0	732	—	—	Bordeaux.
Times	681	17	3	756	—	—	Valparaiso.
Bootle	248	13	0	433	—	—	Messina.
Margaret Hain	169	14	0	299	—	—	Smyrna.
Zercon	177	13	0	295	—	—	Malta.
Peppino	270	16	0	424	—	—	Malta.
Amore	560	17	6	779	—	—	Genoa.
Bruckburn Priory	431	17	0	587	—	—	Iquique.
F. W. Carlow	531	18	2	737	—	—	Rio Janeiro.
Matterhorn	1,327	19	6	—	—	618	Coquimbo.
Grace	93	12	0	—	142	—	Newfoundland.
Stranger	348	15	0	604	—	—	Varna.
Laurel	336	16	6	197	—	—	Poti.
Assyrian	294	15	0	493	—	—	Pernambuco.
Revival	136	12	0	230	—	—	Smyrna.
Fanny	431	19	0	625	—	—	Varna.
Callesto	536	16	0	691	—	—	Valparaiso.
Ben More	536	15	3	517	—	—	Valparaiso.
Carrie	240	13	0	351	—	—	Rio Janeiro.
Amy	167	13	0	301	—	—	Malta.
Nuova Speransa	438	16	0	651	—	—	Genoa.
Spirit of Morning	599	17	4	816	—	—	Valparaiso.
Nino Bixio	557	16	0	600	—	—	Rangoon.
Giuseppina P.	512	18	6	764	—	—	Genoa.
Albatross	314	13	0	456	—	—	Bahia.

Ship.	Register Tonnage.	Draft.		Coal.	Cannel.	Coke.	Port of Destination.
		Feet.	Inches.				
Johan August	98	12	0	—	156	—	St. Petersburgh.
Cordova	521	15	11	719	—	—	Valparaiso.
Camelia	360	16	0	472	—	—	Naples.
Susie	320	14	4	456	—	—	Havana.
Cassowary	76	10	0	109	—	—	Colleraine.
San Rafael	955	20	10	1,162	—	—	Valparaiso.
Levanter	643	16	6	—	801	—	New York.
Normanby	449	16	6	644	—	—	Rio Janeiro.
Clementine	360	14	0	584	—	—	Bordeaux.
Jessie Goodwin	337	15	0	487	—	—	Trinidad.
Emma	480	16	0	669	—	—	Genoa.
Bengale	368	14	0	375	—	—	Havre.
Elvina	210	13	0	248	—	—	Naples.
Bertha	357	15	0	576	—	—	Genoa.
Adriatic	436	15	0	592	—	—	Havana.
Etlie	499	17	10	704	—	—	Valparaiso.
Mio	570	18	0	928	—	—	Genoa.
Arch Fuller	700	17	3	749	—	—	Valparaiso.
Woodfield	995	18	9	1,009	—	—	Callao.
Arica	479	17	6	670	—	—	Valparaiso.
Annie Richmond	694	18	6	779	—	—	Valparaiso.
Ganymede	568	16	6	772	—	—	Valparaiso.
Koncordia	436	17	0	594	—	—	Cienfugos.
Sjufma	346	17	6	500	—	—	Demerara.
Levi Hart	407	16	10	354	151	—	Cardenas.
Magnet	464	18	0	605	—	—	Valparaiso.
Coldstream	545	13	0	197	—	—	Valparaiso.
Barracouta	610	18	0	760	—	—	Valparaiso.
Sibima	767	18	0	937	—	—	Valparaiso.
Baltic	554	18	0	906	—	—	Genoa.
Elsie Mary	186	14	0	304	—	—	Smyrna.
Alpha	349	17	0	494	—	—	Rio Janeiro.
Sir Robert Hudson	194	14	6	331	—	—	Messina.
Hannah	242	15	0	401	—	—	Rio Janeiro.
Crimea	501	18	0	717	—	—	Havana.
Acapulco	598	18	6	566	—	—	Valparaiso.
Intrepid	91	12	0	150	—	—	Bayonne.
Forest Belle	1,115	20	6	1,639	—	—	Havana.
Warwickshire	679	18	6	871	—	—	Valparaiso.
Huron	609	18	0	857	—	—	Valparaiso.
Jules Maria	282	15	0	337	—	—	Malaga.
Der Sud	482	16	0	628	—	—	Havana.
Celine	200	10	0	202	—	—	Quanpeert.
South Glen	598	18	0	814	—	—	Valparaiso.
Madre	742	17	0	945	—	—	Genoa.
Pacific	397	17	0	594	—	—	Valparaiso.
Maipu	593	17	0	818	—	—	Valparaiso.
Dorega	694	17	0	876	—	—	Valparaiso.
Prince Regent	467	15	10	589	—	—	Havana.
M. A. McFarlane	381	14	0	584	—	—	Genoa.
Eliza Moreton	437	16	0	544	—	—	Havana.
Maria Clotilda	486	17	0	712	—	—	Genoa.
Pasithea	587	17	0	638	—	—	Valparaiso.
Bonnie Dundee	1,026	20	6	1,324	—	—	Havana.
Adelina	558	18	0	653	—	—	Genoa.
Argyra	228	14	0	344	—	—	Smyrna.
Hamilton	326	16	0	280	—	—	Demerara.
Minnie Cameron	436	15	6	606	—	—	Havana.
Esther	484	15	0	587	—	—	Havana.
Carnarvon Castle	728	17	3	887	—	—	Valparaiso.
Tranmere	822	20	0	1,352	—	—	Rangoon.
D. Chapin	540	17	0	778	—	—	Havana.
Premeir	1,392	18	0	1,144	—	—	Rangoon.
Buckhorn	790	19	0	1,191	—	—	Simon's Bay.
William Fisher	299	15	0	459	—	—	Demerara.
Craigmullen	780	17	6	846	—	—	Valparaiso.
Norton Stover	334	15	0	548	—	—	Havana.
Loch Dee	699	17	10	829	—	—	Valparaiso.
Merweed	313	15	0	437	—	—	Demerara.
Bat Madre	781	17	0	1,084	—	—	Genoa.
Chasea	737	17	6	793	—	—	Valparaiso.
Cascatelle	346	14	0	449	—	—	Havana.
Minero	478	16	0	404	—	—	Valparaiso.
Town of Liverpool	286	16	0	344	—	—	St. Malo.
Prince Arthur	544	19	0	498	—	—	Caldera.
Akbar	332	16	0	586	—	—	Havana.
Colchagua	597	17	0	701	—	—	Valparaiso.
D. H. Tolke	445	14	0	629	—	—	Havana.
Ingomar	1,175	20	6	1,373	—	—	Rio Janeiro.
Louisa Malcolm	699	19	9	—	1,070	—	Rio Janeiro.
Gusto-del-Argento	331	15	0	392	—	—	Trieste.
M. A. Franklin	522	15	0	633	—	—	Havana.
Annina	115	12	0	206	—	—	Naples.
Pampero	588	19	0	683	—	—	Valparaiso.
Com Dupont	434	16	0	506	—	—	Havana.
Dundonald	991	19	6	607	—	—	Valparaiso.
Emmanuel R.	403	17	0	594	—	—	Genoa.
Antonio Ferraro	509	17	6	653	—	—	Genoa.

TABLE B.

LIST of VESSELS LOADED with COAL, &c., at the BOARD'S APPLIANCES, BIRKENHEAD, from the 1st January 1875 to the 31st December 1875.

Ship.	Register Tonnage.	Draft.		Coal.	Cannel.	Port of Destination.
		Feet.	In.	Tons.		
S.S. Arbutus	236	13	0	17	418	Dublin.
Martha	268	14	6	358	—	Curacoa.
S.S. Santander	438	12	8	26	—	Bilboa.
Cassini	687	18	0	1,033	—	Brindisi.
Killarney	1,060	18	0	1,607	—	Bombay.
Sceptre	1,109	20	6	2,325	—	Bombay.
Susie	302	14	6	475	—	Demerara.
Vitesse	269	14	6	368	—	Odessa.
S.S. Alliance	198	12	6	8	363	Dublin.
Trent	1,410	15	3	572	—	Odessa.
Maude	495	10	0	79	—	Hull.
Lizzie Williams	827	18	3	893	—	San Francisco.
S.S. Alert	70	9	6	136	—	Newry.
Vidsjaaen	530	16	6	717	—	Callao.
S.S. Alliance	198	12	0	—	313	Dublin.
Lady Lycett	1,507	21	8	2,323	—	Bombay.
Ilion	544	18	10	802	—	Odessa.
G. W. Halls	456	16	6	646	—	Kingston.
Geerdma	123	11	0	191	—	Oporto.
S.S. Parsee	1,014	14	0	431	—	Bombay.
West	380	14	9	756	—	Oporto.
Malvino Schutt	372	15	6	502	—	Kingston.
S.S. Deva	151	12	0	16	293	Dublin.
Joven Adelaide	448	14	9	317	—	Oporto.
S.S. Emblehope	1,244	13	6	674	—	Bombay.
Potomac	1,504	21	6	2,323	—	Bombay.
Condar	199	14	0	194	—	Curacoa.
S.S. San Domingo	680	14	0	496	—	Pomaron.
Mary Watkinson	149	12	0	247	—	Dundalk.
S.S. Aston	89	10	0	170	—	Dublin.
City of Manchester	2,046	16	10	1,051	—	Calcutta.
Fordenskjold	1,118	23	6	1,240	—	Batavia.
Cato Annetta	158	10	6	172	—	Curacoa.
S.S. Harry S. Edwards	796	18	0	1,748	—	Malta.
Sheldrake	373	13	3	610	—	Dublin.
Emily Warbrick	152	12	0	287	—	Dundalk.
S.S. Alliance	198	12	6	8	300	Dublin.
Hermione	383	14	0	396	—	Bahia.
Braemar	838	17	3	1,007	—	Valparaiso.
S.S. Bywell Castle	891	15	0	425	—	Odessa.
Francesca	167	13	0	254	—	Gibraltar.
S.S. Newbiggin	875	11	0	285	—	Constantinople.
Emilien Marie	244	14	0	361	—	Rosario.
S.S. Alliance	198	12	6	9	355	Dublin.
Beltiste	412	14	6	504	—	Martinique.
Fannie	420	16	0	606	—	Martinique.
S.S. Capella	1,403	12	6	149	—	Malta.
Elizabeth	245	13	4	385	—	Martinique.
Alexandre Delphine	447	15	6	589	—	Martinique.
Cecil Brindley	94	10	0	172	—	Gibraltar.
S.S. Alliance	198	12	6	—	363	Dublin.
Elfin	279	16	5	423	—	Martinique.
Matchless	1,165	21	6	1,325	—	Callao.
S.S. Royal Minstrel	873	19	6	1,731	—	Brindisi.
Onyx	91	8	6	44	—	Glasgow.
Emelia	193	13	0	287	—	Seville.
Kilmarnock	303	12	6	425	—	Santander.
S.S. Alliance	198	12	0	—	363	Dublin.
Hindoo	2,504	17	2	453	—	Hull.
Pensacola	1,421	21	3	1,795	—	Callao.
Johanna	215	12	6	302	—	Rio Grande du Sol.
S.S. Alliance	198	12	0	6	370	Dublin.
Enmore	1,121	19	6	1,952	—	Brindisi.
Racer	61	9	10	109	—	Aveiro.
S.S. Alliance	198	12	6	10	371	Dublin.
Aphrodita	1,626	19	0	13	—	Calcutta.
S.S. Alliance	198	12	6	8	371	Dublin.
Serena	493	15	4	179 coke	—	Valparaiso.
S.S. Alliance	198	12	6	8	369	Dublin.
Ennerdale	797	19	0	1,303	—	Brindisi.
Wilhelmina Agatha	193	11	6	147	—	Curacoa.
Estrella de Chili	555	16	0	864	—	Singapore.
Navarino	2,965	17	6	423	—	Hull.
R. B. Fuller	1,360	23	3	1,014	—	San Francisco.
Mary Ellen	139	10	6	219	—	Oporto.
Wave	348	16	0	349	—	Cape Town.
S.S. Alliance	198	13	0	8	367	Dublin.
City of Glasgow	1,168	19	9	1,229	—	Calcutta.
Spirito	460	18	6	730	—	Odessa.
S.S. Cassini	687	17	10	470	—	Odessa.
Maresca	409	16	0	596	—	Odessa.
Keturah	140	9	0	108	—	Port Madoc.
King Arthur	1,047	17	6	699	—	Glasgow.
City of Benares	1,187	19	6	1,283	—	Calcutta.
S.S. Alliance	198	13	0	17	297	Dublin.
Ida	329	15	0	472	—	Monte Video.
Condor	199	12	0	260	—	Curacoa.

Ship.	Register Tonnage.	Draft.		Coal.	Cannel.	Port of Destination.
		Feet.	In.	Tons.		
S.S. Hibernian	204	10	0	16	360	Dublin.
Ernesti Parodi	610	17	0	910	—	Callao.
Kingston	1,208	20	7	1,170	—	Bombay.
S.S. Hibernian	204	13	6	—	372	Dublin.
S.S. Hibernian	204	13	6	16	368	Dublin.
Hagarstown	1,903	16	0	796	—	Bombay.
S.S. Hibernian	204	12	0	—	372	Dublin.
Cato Annetto	158	10	6	203	—	Curacoa.
S.S. Hibernian	204	13	0	12	370	Dublin.
S.S. Hibernian	204	13	0	6	352	Dublin.
Est	953	19	6	1,159	—	Callao.
Uberty	52	10	0	44	—	Jersey.
F. Scalla	416	14	0	441	—	Bermuda.
Melmerby	1,510	11	0	132	—	Bombay.
Ferdinande	229	15	3	287	—	St. Jago de Cuba.
Laura	174	12	6	293	—	Madeira.
S.S. Hibernian	204	13	0	10	301	Dublin.
Isla	292	16	4	—	313	Para.
City of Madrid	1,191	19	1	1,257	—	Calcutta.
Ravenscraig	1,263	18	0	850	—	Montreal.
Udsire	444	16	7	355	—	Port-au-Prince.
Theresa	704	18	10	920	—	Rangoon.
Matador	685	20	11	194	—	Rio Janeiro.
Albion	184	12	6	325	—	Gibraltar.
S.S. Hibernian	204	13	0	16	386	Dublin.
S.S. Hibernian	204	13	0	14	348	Dublin.
Coloma	852	17	8	999	—	San Francisco.
S.S. Mino	450	16	0	117	—	Lisbon.
Lancashire Witch	363	14	6	509	—	Cape Town.
Bacchus	1,250	17	8	1,005	—	Akyab.
Estrella	554	18	0	763	—	Manilla.
Cordore	673	18	0	1,031	—	Rangoon.
Octave Marie	199	13	0	285	—	Gibraltar.
Blair Drummond	1,450	20	0	1,695	—	Calcutta.
St. Louis	487	15	0	603	—	Pernambuco.
M. C. Nelson	590	16	0	723	—	Caldera.
S.S. "Mauritania"	480	16	6	139	—	Valencia.
C. Maggio	419	16	0	556	—	Monte Video.
Cape Comorin	1,199	20	2	1,432	—	Bombay.
Venus	143	9	6	217	—	Oporto.
Savernake	1,284	18	6	1,303	—	Rangoon.

TABLE C.

RETURN showing the NUMBER of LIGHTERS LOADED MONTHLY with COAL, &c. at the BOARD'S HIGH LEVEL RAILWAY, LIVERPOOL, for the Year 1875.

1875. Month.	No. of Lighters.	Coal Loaded.	Cannel Loaded.	Coke Loaded.
		Tons.	Tons.	Tons.
January	107	10,620	51	—
February	97	9,452	—	—
March	117	11,719	84	50
April	111	11,314	—	121
May	78	7,427	—	—
June	91	8,226	54	—
July	91	8,422	—	—
August	98	8,964	400	—
September	109	10,171	219	—
October	141	14,167	—	—
November	126	12,160	—	—
December	115	11,928	217	—

TABLE D.

RETURN showing the NUMBER of LIGHTERS LOADED MONTHLY with COAL at the BOARD'S APPLIANCES, BIRKENHEAD, for the Year 1875.

1875. Month.	No. of Lighters.	Tons.
January - -	384	47,747
February - -	331	42,889
March - - -	335	42,692
April - - -	359	45,681
May - - -	328	41,360
June - - -	365	46,695
July - - -	343	45,239
August - - -	363	48,805
September - -	358	49,556
October - - -	394	52,356
November - -	396	51,911
December - -	501	64,244

STATEMENT as to the QUANTITY of COAL SHIPPED by the BOARD'S APPLIANCES at LIVERPOOL and BIRKENHEAD, &c. for the Year ending the 31st of December 1875.

By the Board's Appliances, Liverpool, High Level Railway:—

	Tons.
By hydraulic cranes into ships and flats -	363,433
By hand tip into flats - - -	73,504
Total - - -	436,937

By the Board's Appliances, Birkenhead:—

	Tons.
By hydraulic tips into ships and steamers	77,479
By hand tips into flats and lighters -	579,175
Total - - -	656,654

NOTE.—The under-noted was shipped during the same period by other methods, viz.:—

	Tons.
By railway company's hand tip into flats and lighters - - - -	174,402
By licensed stevedores by barrow into sailing vessels - - - -	254,756
Total - - -	429,158

COAL TRADE STATISTICS.

Years.	Shipped at Birkenhead.	Shipped at High Level, Liverpool.	Total Birkenhead and Liverpool.	Board's Machinery.	Railway Coal Tips.	Shipped by Hand.
	Tons.	Tons.	Tons.	Tons.	Tons.	Tons.
1862	268,000	51,000	319,000	11,906	60,000	196,000
1863	342,000	71,000	413,000	46,000	93,000	203,000
1864	441,000	131,000	572,000	70,000	115,000	256,000
1865	395,000	181,000	576,000	51,000	136,000	208,000
1866	558,000	262,000	820,000	161,000	15,000	247,000
1867	798,000	306,000	1,104,000	372,000	158,000	267,000
1868	817,000	188,000	1,005,000	367,000	181,000	268,000
1869	674,000	282,000	956,000	300,000	180,000	194,000
1870	602,000	326,000	928,000	248,000	176,000	177,000
1871	822,000	386,000	1,208,000	382,000	192,000	247,000
1872	946,000	457,000	1,403,000	554,000	190,000	201,000
1873	1,001,000	422,000	1,423,000	651,000	177,000	189,000
1874	1,040,000	395,000	1,435,000	636,000	174,000	230,000

AVERAGE ANNUAL SHIPMENTS, PERIODS OF FIVE YEARS.

Five Years.	Average per Year at Birkenhead.	Average per Year at Liverpool.	Average Total per Year.
1862 to 1866	400,000	140,000	540,000
1867 to 1871	743,000	297,000	1,040,000

E

REGULATIONS to be observed in the loading of Coal and discharging of Ballast at the Coal Tips, Low Level, and Ballast Crane, at Birkenhead.

1. All vessels intended to be loaded with coal shall be berthed in the order in which they are booked for berths at the dock master's office, provided, however, that no coal agent shall load more than one vessel at the tips at the same time, if any other agent requires a berth.

2. No vessel shall be berthed until a certificate is lodged at the dock master's office, that there are fifty tons of coal ready for immediate shipment at the low level berths, and one hundred tons at the tips.

3. Until vessels in berth shall have discharged their ballast, the minimum quantity of coal shipped, or of ballast discharged, shall be one hundred tons as the average, daily at the low level, and two hundred tons at the tips; and

afterwards, the average minimum quantity of coal shipped daily at the low level shall be one hundred and fifty tons, and at the tips two hundred and fifty tons.

4. Vessels not complying with these regulations will be immediately removed from the berth, but will be entitled to the first vacant berth on having the required quantity of coal to proceed with their loading.

5. Vessels in ballast intending to load by the coal tips to take precedence at the ballast crane; and vessels intending to load coal for stiffening to have the use of the crane in preference to vessels to be loaded with an entire cargo by the crane.

6. The use of No. 2 tip is not to interfere with the working of Nos. 1 and 3 tips when the latter are being used by large ships.

By Order of the Board.

Dock Office,
26th September 1867.

F.

SHIPMENT of COAL and DISCHARGE of BALLAST, &c. at BIRKENHEAD.

Secretary's Office,
Liverpool, December 9th, 1874.

The Mersey Docks and Harbour Board are prepared to ship coal and discharge ballast at their hoists, hand tips, and ballast crane, at Birkenhead, also to move, under their regulations, vessels using the appliances in the west float, at the following rates, viz:—

COAL.

	Per Ton.	
	s.	d.
1. For loading coal into ships or steamers, by the hoists, including haulage of wagons, tipping and stowage, and the lowering of ten per cent. of the registered tonnage into the hold at the commencement of loading to avoid breakage - - - - -	0	5
2. Ditto for the bunkers of steamers, by the coal hoists - - - - - - -	0	7½
3. Ditto ditto, by hand, from wagons on the quay - - - - - - - -	1	1
4. Ditto into flats, by tips - - - - -	0	3
5. Ditto into steam barges or steam lighters, by hoists or tips - - - - - - -	0	3
6. Ditto for stiffening, if desired, by the ballast crane, into vessels discharged by the crane, to the extent of ten per cent. of the registered tonnage - - - - - - -	0	6
7. Ditto by the ballast crane, and trimming an entire cargo, or any excess over ten per cent. of the registered tonnage - - - -	0	8
8. Ditto into flats by the ballast crane - -	0	6
9. If weighed by the Board's weighing machines	0	0½
For loading, by the appliances, coal for home consumption (in addition to the above rates) - - - - - - -	0	2

NOTE.—For the shipment of coke double the above rates will be charged.

BALLAST.

	Per Ton.	
	s.	d.
10. For discharging ballast from ships or vessels by the hydraulic crane - - - - - (One tub of ballast in every ten to be weighed on one of the Board's weighing machines.)	0	6
11. Ditto by hand, when the cranes are not available, ballast from vessels berthed at the appliances - - - - - - -	0	10

MOVING VESSELS.

	s.	d.
12. For moving, under the Board's regulations, vessels in the west float requiring the use of the coal hoists or ballast crane, for each 100 tons register measurement, or for any fractional part over 50 tons - - - -	4	0

Vesels can be loaded with coal at the rate of from 300 to 600 tons a day, if a sufficient supply be brought down.

Ballast can be discharged at the rate of 200 tons and upwards a day.

Two of the hoists are provided with lowering tubs, worked by machinery, with the view to prevent the breakage of coal, and the use of these tubs, with the machinery, is allowed free of charge for landing from vessels any pyrites stones, &c. that may be found amongst the coal placed on board by the appliances.

An apparatus for screening coal is also provided.

At each of the hoists, and at the hand tips, Vittoria Creek, there is a machine for weighing coal, if required.

The contractor for shipping coal and discharging ballast, &c. is a licensed master stevedore and master lumper.

The rates are to be paid in advance to the board's treasurer, dock office, Liverpool. Every information can be obtained there, at the office of the traffic manager, district E, Duke Street entrance, and at the coal tips, Birkenhead.

The board recommend parties interested in the shipment of coal to inspect personally the working of the appliances, so that they may estimate the advantages to be gained by their use.

EDWARD GITTINS,
Secretary.

G.

REGULATIONS for working the HIGH LEVEL COAL TRADE at the BRAMLEY-MOORE and WELLINGTON DOCKS.

1. As many vessels and flats to be admitted into the Bramley-Moore, Wellington, and Wellington half-tide docks to work at the high level railway, as shall be consistent with the general work of those docks.

2. No vessel to be allowed to enter the Bramley-Moore, Wellington, or Wellington half-tide docks, nor to be entered on the Application Book for admittance into those docks, to load coal from the high level railway, except upon the production of a jerque note, or a certificate from the master of the dock in which the vessel is lying at the time, showing that she is discharged, and also a certificate from the coal agent that she is to load coal at the high level railway. No coal agent to be allowed to load more than two flats at the cranes at the same time, nor to have more than three vessels in the Bramley-Moore, Wellington, or Wellington half-tide docks (inclusive) loading and to load coal at the cranes, at one time.

No flat to be admitted until a certificate is produced from the coal agent that she is to load coal at the high level railway.

3. Applicants for vessels to be admitted into dock to call at the dock master's office 2 hours after high water, when high water happens between 8 a.m. and 2 p.m.; and when before or after these hours, to call at 10 a.m., when the list of vessels waiting to enter dock will be called over, and orders given for such as are on turn, and represented, to the enter dock.

4. No vessel to be entered in the Berthing Book, kept at the office of the Superintendent of the high level railway, before she is in the Bramley-Moore, Wellington, or Wellington half-tide docks, and each vessel is to be berthed in regular turn, as entered, if the specified quantity of coal is at the Sandhills Station; if not, the next vessel on turn, having sufficient coal ready, to take the berth; any vessel losing her turn in consequence of coal for her not being at the Sandhills Station, to be considered first on turn when the coal is ready. Flats and vessels to follow the same order as to turn for loading, whether entered for the cranes or the shoot.

5. When a crane is idle from want of coal, the first vessel on turn not in the dock having the quantity of coal specified by these regulations at Sandhills Station ready for shipment, (to be proved by a certificate from the coal agent which must also be endorsed by the Superintendent of the high level railway,) may, at the discretion of the dock master, be admitted into dock out of regular turn, as shown in the Application Book. The Superintendent of the high level railway is not to put coal that he has certified for one vessel into any other vessel.

6. No coal to be brought on the high level railway without the permission of the Superintendent, who will inform the railway clerk which vessel is first on turn for a berth, so that the coal may be sent to the high level railway previous to the vessel being berthed.

7. No vessel or flat to be berthed under a crane or a

shoot before the quantity of coal, according to the following scale, is at Sandhills Station, ready for shipment, viz.:—

Vessels or flats under 100 tons register	Full cargo.
Do. do. of 100 ,, ,, and under 300 tons register -	100 tons of coal.
Vessels of 300 tons register and upwards - - - -	150 ,, ,,

The Superintendent of the high level railway to be furnished with a return from the Sandhills Station, at 8 a.m., 1 p.m., and 4 p.m. each day, of the number of loaded wagons (distinguishing box from square wagons) at the station ready for shipment, with the names of the shippers of the coal.

8. Each coal wagon to have a card attached to it, stating the number and weight of the wagon, and weight of the coal thereon; the card to be taken off by the Superintendent, and filed, for a check on the weight shipped.

The gross weight of each waggon of coal intended to be lifted by a cradle crane must not exceed twelve tons; if it exceed that weight, such loaded wagon, will be liable to be returned to Sandhills Station. Wagons sent on to the high level railway without having the requisite cards attached thereto, will also be liable to be returned to that station.

9. Should any delay arise in working any crane or the shoot, from the want of coal for a vessel loading there, or by default of such vessel, that vessel to be immediately removed from the berth, and the next on turn for which coal is ready to be placed therein.

10. Each vessel entered for a berth, to undertake to provide a sufficient number of trimmers; or failing to have a sufficient number of trimmers, and thereby causing obstruction to the full working of the appliances, is liable to be removed from the berth.

11. No transhipment of coal, coke, or other articles from one wagon to another, to be allowed on the high level railway.

12. Vessels, when taking in coal or coke from the high level railway, to be allowed also to take in machinery or cargo from the low quay, when it can be done without causing any delay to the work of the high level railway.

13. Every vessel to leave the dock within 24 hours after she has received the quantity of coal intended to be shipped on board her, if called upon by the harbour master to do so.

14. In the event of a vessel having to vacate her berth, and requiring not over 120 tons coal to complete her loading which is to be proved to the satisfaction of the dock master, one or two flats may be allowed to bring coal into dock for such vessel to the extent of, but not exceeding, 120 tons.

By Order of the Board,
Dock Office Liverpool, EDWARD GITTINS,
March 25th, 1875. Secretary.

DUTIES of the SUPERINTENDENT of the HIGH LEVEL RAILWAY.

1. The Supintendent of the high level railway to keep an account of the vessels that use the coal cranes and shoot; and when a vessel has finished loading or discharging, the amount of tons of coal, or other articles, to be inserted in the Superintendent's Registry Book, and compared with the railway clerk's account; the entry in the Registry Book to be signed by the latter.

2. To enter vessels in the Application Book, and to berth them in regular turn as entered, as per No. 4 regulation; and to prevent vessels grounding under or near the appliances, so that they may be removed any time when required.

3. A dock gateman to be stationed at the east end of the Bramley-Moore and Wellington docks, to attend to the Superintendent's orders, so far as regards the removal and berthing of vessels in connexion with the high level railway.

4. The Superintendent to see that the regulations for working the high level railway are strictly carried out, and, in case of any obstruction or unnecessary delay, to send for the dock master, or the north assistant harbour master, and note the cause in his Memorandum Book.

By Order of the Board,
Dock Office, Liverpool, EDWARD GITTINS,
March 25th, 1875. Secretary.

H.

STATEMENT of CHARGES for the use of the BOARD'S HIGH LEVEL RAILWAY.

COAL EXPORTED.

1. FOREIGN:—

	Per ton.	
For use of High Level Railway -	2*d*.	Collected by the Lancashire and Yorkshire Railway Company, and paid to the Board.
Dock dues - - -	1½*d*.	Collected by the Board.
Town dues - - -	2*d*.	Ditto.

2. COASTWISE:—

For use of High Level Railway -	2*d*.	Collected by the Lancashire and Yorkshire Railway Company, and paid to the Board.
Dock dues - - -	Nil.	
Town dues - - -	2*d*.	Collected by the Board.

3. HOME CONSUMPTION, EXCEPT FOR SALT WORKS:—

For use of High Level Railway -	2*d*.	Collected by the Lancashire and Yorkshire Railway Company, and paid to the Board.
Ditto - - -	3*d*.	Collected by the Board.
	5*d*. per ton.	

4. FOR SALT WORKS:—

For use of High Level Railway -	2*d*.	Collected by the Lancashire and Yorkshire Railway Company, and paid to the Board.
Ditto - - -	2*d*.	Collected by the Board.
	4*d*. per ton.	

5. FOR USE OF SEA-GOING STEAMERS, OR FOR SHIP'S STORES:—

For use of High Level Railway -	2*d*.	Collected by the Lancashire and Yorkshire Railway Company and paid to the Board.
Dock dues - - - Town dues - - -	Nil.	

(*g*.) Letters and Statistics from Mr. R. Cooper Rundell, Assistant to the Secretary, Liverpool Underwriters' Association.

Sir, Liverpool, 30th October 1875.

When I had the honour of placing before the Royal Commission, last June, my evidence respecting spontaneous combustion of coal cargoes, I gave some statistics as to the number of vessels with cargoes of 500 tons of coal and upwards which had sailed from the Mersey on voyages beyond the Equator during the first nine months of 1873 and of 1874, and compared the total number of these cargoes with the number of cargoes in which spontaneous combustion had been reported.

I was then asked if I could inform the Commission how many of the vessels which sailed were iron, how many were loaded on the Liverpool side of the Mersey, and how many by the tip, in order that a further comparison might be made with regard to these particulars.

The tables which I have now the pleasure of sending you herewith supply this information for each month of the two periods. The voyages are classified as before.

Table I. gives the number of vessels which sailed from the Liverpool side of the Mersey, and the number which sailed from the Birkenhead side.

Table II. The number of those vessels which were iron, and the number which were wood.

Table III. The number loaded by tip, and the number loaded by hand.

Table IV. The number of tons of shipping and the number of tons of coal for each month.

Table V. The average tons of shipping and of coal for each month.

In each of the tables the number of cases of heating and fire are printed within parentheses.

My evidence before the Commission showed that the greatest increase in the number of cases of spontaneous combustion in 1874 occurred in shipments of coal from the Mersey for the West Coast of America, and in sending you these tables I take the opportunity to point out some facts supplied by these tables with regard to the shipments in question.

Table I. shows that 41 vessels sailed from the Liverpool side during the first period, and 82 during the second period, while from the Birkenhead side 34 sailed during the first period, and only 17 during the second. There were no casualities to any of the 1873 shipments, while for the second period there were 12 out of the 82 shipments from the Liverpool side, or about 15 per cent., and one only out of the 17 from the Birkenhead side, or 6 per cent.

It would appear therefore from this table that the Liverpool shipments had increased 100 per cent., while the Birkenhead ones had decreased in the same ratio, and that the per-centage of casualties in shipments from the Liverpool side was more than double that from the Birkenhead side.

Table II. shows that 40 iron vessels sailed from the Mersey during the first period, and 40 during the second. Also that 35 wooden vessels sailed during the first period and 59 during the second, representing an increase of 69 per cent. Three casualties occurred in the 40 iron vessels, *i.e.*, in 7½ per cent. of their number, and 10 in the 59 wooden vessels, or in 17 per cent. of their number.

This second table consequently shows that the increased number of vessels which sailed in the second period were of wood, and that the per-centage of casualties from heating was more than twice as great for wooden vessels as for iron.

Table III. shows that, during the first period, 19 vessels were loaded at the tip on the Liverpool side, and during the second period, 43. Also, that 22 were loaded at the tip from the Birkenhead side during the first period, and 10 during the second period. Casualties happened in 12 out of the 43 vessels loaded from the tip on the Liverpool side, or 28 per cent. of the number, and no casualty is reported in the 10 vessels loaded from the tip on the Birkenhead side. No cases of heating are reported in any of the 39 vessels loaded by hand on the Liverpool side during the second period, and although one case occurred in the 7 vessels loaded on the Birkenhead side, there are circumstances connected with it which indicate that the casualty had no connexion with the way in which the vessel was loaded. I allude to the case of the "Merchant.'

Table III. therefore shows that the number of shipments from the tip on the Liverpool side increased more than 100 per cent.; while on the Birkenhead side they decreased in about the same ratio, and that the per-centage of casualties in vessels loaded from the tip on the Liverpool side was 28 per cent., while in those loaded from the tip on the Birkenhead side there were none.

Table IV. shows that while there was but a slight difference in the average size of the vessels which sailed during the second, or of their cargoes, as compared with the average size of the vessels and their cargoes for the first period, yet the tonnage during the hot months of the year shows a great increase in 1874.

Table V. gives the average size of ship and cargo for each month of the periods in question. The average cargoes during the hot months of the second period are larger than the average cargoes during the same months of the first period; but it will be seen that they are not in any case so large as those of some of the early months of the first period.

In many cases, the vessels which sail for the West Coast take only a part cargo of coal, and consequently the average given above does not definitely indicate whether more large cargoes were shipped in the second period than in the first.

Now 10 out of the 13 casualties which occurred in cargoes from the Mersey for the West Coast were in cargoes of 1,300 tons and upwards; and, taking this limit, the following table gives the number of vessels, with 1,300 tons of coal and upwards, which sailed from the Mersey for the West Coast during each month of the two periods selected for comparison.

	1873.	1874.
January	4 sailed.	1 sailed.
February	1 "	None "
March	None "	None "
April	6 "	2 "
May	1 "	1 "
June	2 "	2 "
July	None "	5 "
August	1 "	8 "
September	1 "	5 "
	16 "	24 "

It will hence be seen that 16 such cargoes were shipped during the first period and 24 during the second, representing an increase of 50 per-cent. Also that 20 out of the 24 sailed during the hot months of 1874, while during the same months of 1873 only 4 sailed.

To sum up, therefore, the tables show that in the second period the greatest per-centage of casualties to cargoes from the Mersey to the West Coast occurred, (1), in vessels loaded on the Liverpool side; (2), in wooden vessels; (3), in vessels loaded at the tip; (4), in vessels with cargoes of 1,300 tons of coal and upwards.

Features (1) and (3) relate to *condition* of the coal; while (2) and (4) relate to the vessel and the size of cargo. With regard to these latter elements, it may be observed that several of the vessels, in the cargoes of which cases of spontaneous combustion were reported in 1874, sailed in previous years with cargoes of coal equally large, and no signs of spontaneous combustion were reported. I append the names of some of these vessels, and the kind of coal which they took.

I am, &c.,
R. Cooper Rundell.

The Secretary,
Royal Commission on Spontaneous
Combustion of Coals, London.

TABLE I.—Liverpool and Birkenhead Sailings.

Table showing the Number of Vessels with Cargoes of 500 tons of Coal and upwards which have sailed from the Mersey during each of the first nine Months of 1873 and 1874 on voyages to the South Atlantic, to the Eastward of the Cape of Good Hope, and to the West Coast of America; distinguishing between those loaded at Liverpool and those at Birkenhead.

Note.—The number of cases of heating are printed between parentheses.

Month.	South Atlantic.			East of Cape of Good Hope.			West Coast of America.			Total.		
1873.	Mersey.		Total.	Mersey.		Total.	Mersey.		Total.	Mersey.		Total for Month.
	Liver-pool.	Birken-head.		Liver-pool.	Birken-head.		Liver-pool.	Birken-head.		Liverpool.	Birken-head.	
January - -	0	1	1	1	4*	5	11	2	13	12	7	19
February - -	2	0	2	1	7	8	1	5	6	4	12	16
March - -	2	6	8	2*	8	10	1	0	1	5	14	19
April - -	2	2	4	4	5	9	4	5	9	10	12	22
May - -	3	3	6	1	9	10	3	2	5	7	14	21
June - -	1	0	1	1	7	8	4	2	6	6	9	15
July - -	6	2	8	1	4	5	5	8	13	12	14	26
August - -	2	3	5	0	11	11	8	5	13	10	19	29
September - -	3	1	4	0	14	14	4	5	9	7	20	27
Totals -	21	18	39	11	69	80	41	34	75	73	121	194
1874.												
January - -	2	0	2	1	10†	11	0	1	1	3	11	14
February - -	4	4	8	0	4	4	5	2	7	9	10	19
March - -	2	3	5	1	9	10	4	0	4	7	12	19
April - -	0	1	1	0	14	14	(1) 7	2	9	(1) 7	17	(1) 24
May - -	3	1	4	1	12	13	(1) 4	2	6	(1) 8	15	(1) 23
June - -	0	0	0	2	15	17	(2) 11	(1) 2	13	(3) 13	(1) 17	(4) 30
July - -	1	0	1	(1) 1	(1) 10	11	(3) 15	4	19	(3) 17	(1) 14	(4) 31
August - -	2	1	3	3	13	16	(3) 21	2	23	(3) 26	16	(3) 42
September - -	2	0	2	2	(1) 19	21	(2) 15	2	17	(2) 19	(1) 21	(3) 40
Totals -	16	10	26	(1) 11	(2) 106	117	(12) 82	(1) 17	99	(13) 109	(3) 133	(16) 242

* One steamer in January and one in March 1873 went by way of Canal with cargo of coals.
† Five steamers with cargoes of coals went through Canal in January 1874.

TABLE II.

List of the Number of Vessels which have sailed from the Mersey with 500 tons of Coal and upwards on voyages beyond the Equator during each Month of the period January 1st to September 30th, 1873; distinguishing Iron from Wooden Vessels.

1873.	South Atlantic Voyages.		East Indian Voyages.		West Coast Voyages.		Total for each Month.	
	Iron.	Wood.	Iron.	Wood.	Iron.	Wood.	Iron.	Wood.
January - -	—	1	2*	3	3	10	5	14
February - -	—	2	2	6	1	5	3	13
March - -	—	8	3*	7	1	—	4	15
April - -	—	4	3	6	1	8	4	18
May - -	—	6	3	7	1	4	4	17
June - -	—	1	3	5	5	1	8	7
July - -	—	8	1	4	9	4	10	16
August - -	—	5	5	6	12	1	17	12
September - -	—	4	4	10	7	2	11	16
	—	39	26	54	40	35	66	128

* One steamer in January and one in March.

Similar List for the same period of 1874.

1874.	South Atlantic Voyages.		East Indian Voyages.		West Coast Voyages.		Total for each Month.	
	Iron.	Wood.	Iron.	Wood.	Iron.	Wood.	Iron.	Wood.
January - -	—	2	6†	5	—	1	6	8
February - -	—	8	—	4	2	5	2	17
March - -	—	5	1	9	—	4	1	18
April - -	—	1	5	9	4	(1) 5	9	(1) 15
May - -	—	4	1	12	2	(1) 4	3	(1) 20
June - -	—	—	4	(1) 13	(1) 4	(2) 9	(1) 8	(3) 22
July - -	—	1	1	(1) 10	(1) 9	(2) 10	(1) 10	(3) 21
August - -	—	3	—	16	(1) 11	(2) 12	(1) 11	(2) 31
September - -	—	2	(1) 1	20	8	(2) 9	(1) 9	(2) 31
	—	26	(1) 19	(2) 98	(3) 40	(10) 59	(4) 59	(12) 183

† Five steamers in January.

TABLE III.

NUMBER of Vessels which have sailed from the Mersey with 500 Tons of Coal and upwards on Voyages beyond the Equator; distinguishing between those loaded at the Tip and those loaded by Hand. 1873 period.

1873. Month and Port.	South Atlantic.				East Indian.				West Coast.				Total loaded by			
	Tip.		Hand.		Tip.		Hand.		Tip.		Hand.		Tip.		Hand.	
January.																
Liverpool - - -	0		0		1		0		5		6		6		6	
Birkenhead - - -	1	1	0	0	*1	2	3	3	0	5	2	8	2	8	5	11
February.																
Liverpool - - -	0		2		1		0		0		1		1		3	
Birkenhead - - -	0	0	0	2	0	1	7	7	4	4	1	2	4	5	8	11
March.																
Liverpool - - -	0		2		1		*1		1		0		2		3	
Birkenhead - - -	1	1	5	7	0	1	8	9	0	1	0	0	1	3	13	16
April.																
Liverpool - - -	1		1		2		2		2		2		5		5	
Birkenhead - - -	2	3	0	1	0	2	5	7	3	5	2	4	5	10	7	12
May.																
Liverpool - - -	1		2		0		1		2		1		3		4	
Birkenhead - - -	1	2	2	4	0	0	9	10	0	2	2	3	1	4	13	17
June.																
Liverpool - - -	0		1		1		0		2		2		3		3	
Birkenhead - - -	0	0	0	1	1	2	6	6	2	4	0	2	3	6	6	9
July.																
Liverpool - - -	1		5		0		1		2		3		3		9	
Birkenhead - - -	0	1	2	7	0	0	4	5	6	8	2	5	6	9	8	17
August.																
Liverpool - - -	0		2		0		0		3		5		3		7	
Birkenhead - - -	3	3	0	2	1	1	10	10	4	7	1	6	8	11	11	18
September.																
Liverpool - - -	0		3		0		0		2		2		2		5	
Birkenhead - - -	1	1	0	3	3	3	11	11	3	5	2	4	7	9	13	18
Total for																
Liverpool - - -	3		18		6		5		19		22		28		45	
Birkenhead - - -	9	12	9	27	6	12	63	68	22	41	12	34	37	65	84	129

* One steamer in January and one in March.

TABLE III.—*cont.*

NUMBER of Vessels which have sailed from the Mersey with 500 Tons of Coal and upwards on Voyages beyond the Equator; distinguishing between those loaded at the Tip and those loaded by Hand. 1874 period.

1874. Month and Port.	South Atlantic.				East Indian.				West Coast.				Total loaded by			
	Tip.		Hand.		Tip.		Hand.		Tip.		Hand.		Tip.		Hand.	
January.																
Liverpool - - -	2		0		1		0		0		0		3		0	
Birkenhead - - -	0	2	0	0	5	†6	5	5	0	0	1	1	5	8	6	6
February.																
Liverpool - - -	3		1		0		0		3		2		6		3	
Birkenhead - - -	0	3	4	5	0	0	4	4	2	5	0	2	2	8	8	11
March.																
Liverpool - - -	0		2		0		1		1		3		1		6	
Birkenhead - - -	1	1	2	4	1	1	8	9	0	1	0	3	2	3	10	16
April.																
Liverpool - - -	0		0		0		0		(1) 1		6		1		6	
Birkenhead - - -	1	1	0	0	5	5	9	9	1	2	1	7	7	8	10	16
May.																
Liverpool - - -	0		3		0		1		(1) 1		3		1		7	
Birkenhead - - -	1	1	0	3	3	3	9	10	1	2	1	4	5	6	10	17
June.																
Liverpool - - -	0		0		(1) 1		1		(2) 7		4		8		5	
Birkenhead - - -	0	0	0	0	0	1	15	16	1	8	(1) 1	5	1	9	16	21
July.																
Liverpool - - -	1		0		0		1		(3) 10		5		11		6	
Birkenhead - - -	0	1	0	0	(1) 2	2	8	9	3	13	1	6	5	16	9	15
August.																
Liverpool - - -	0		2		0		3		(3) 10		11		10		16	
Birkenhead - - -	1	1	0	2	1	1	12	15	1	11	1	12	3	13	13	29
September.																
Liverpool - - -	1		1		1		1		(2) 10		5		12		7	
Birkenhead - - -	0	1	0	1	(1) 7	8	12	13	1	11	1	6	8	20	13	20
Total for																
Liverpool - - -	7		9		(1) 3		8		(12) 43		39		53		56	
Birkenhead - - -	4	11	6	15	(2) 24	27	82	90	10	53	(1) 7	46	38	91	95	151

† Five steamers from Birkenhead with cargoes of coals.

TABLE IV.

The Monthly Aggregate Tonnage of the Vessels which have sailed from the Mersey with 500 Tons of Coal and upwards on Voyages beyond the Equator during the first Nine Months of 1873 and 1874, and the aggregate Tons of Coal they carried.

1873.	South Atlantic Voyages.			East Indian Voyages.			West Coast Voyages.			Total of each Month.		
	No. of Ships.	Amount of registered tonnage.	Amount of tons of coal.	No. of ships.	Amount of registered tonnage.	Amount of tons of coal.	No. of ships.	Amount of registered tonnage.	Amount of tons of coals.	No. of ships.	Amount of registered tonnage.	Amount of tons of coal.
January - -	1	269	511	5	5,892	7,846	13	11,189	15,161	19	17,350	23,518
February - -	2	1,441	2,042	8	9,164	13,225	6	5,228	6,876	16	15,833	22,143
March - -	8	7,902	10,921	10	12,271	16,167	1	796	1,040	19	20,969	28,128
April - -	4	2,306	3,149	9	8,582	11,467	9	9,683	12,072	22	20,571	26,688
May - -	6	4,887	6,590	10	11,298	14,779	5	4,613	4,507	21	20,798	25,876
June - -	1	317	693	8	9,915	12,782	6	5,304	7,009	15	15,536	20,484
July - -	8	5,879	7,315	5	5,180	7,025	13	8,977	10,798	26	20,036	25,138
August - -	5	2,765	4,186	11	11,112	15,616	13	8,801	11,399	29	22,678	31,201
September	4	2,700	3,953	14	15,176	19,943	9	5,588	7,921	27	23,464	31,817
Total -	39	28,466	39,360	80	88,590	118,850	75	60,179	76,783	194	177,235	234,993
and Average -		730	996		1,107	1,486		802	1,024		914	1,211

1874.	South Atlantic Voyages.			East Indian Voyages.			West Coast Voyages.			Total of each Month.		
	No. of ships.	Amount of registered tonnage.	Amount of tons of coals.	No. of ships.	Amount of registered tonnage.	Amount of tons of coal.	No. of ships.	Amount of registered tonnage.	Amount of tons of coal.	No. of ships.	Amount of registered tonnage.	Amount of tons of coal.
January - -	2	1,283	1,685	11	11,416	15,065	1	827	704	14	13,526	17,454
February - -	8	6,370	8,598	4	5,203	7,930	7	4,278	5,517	19	15,851	22,045
March - -	5	3,497	5,094	10	11,419	14,784	4	3,384	4,131	19	18,300	24,009
April - -	1	571	623	14	15,012	19,928	9	9,294	8,143	24	24,877	28,694
May - -	4	2,176	2,891	13	13,300	17,362	6	4,304	5,504	23	19,780	25,757
June - -	—	—	—	17	16,938	22,581	13	9,244	11,522	30	26,182	34,103
July - -	1	414	633	11	10,130	13,099	19	18,146	20,204	31	28,690	33,936
August - -	3	1,693	2,600	16	16,171	24,042	23	23,897	25,985	42	41,761	52,627
September -	2	1,021	1,591	21	27,620	24,648	17	14,504	17,967	40	43,145	44,206
Total -	26	17,025	23,715	117	127,209	159,439	99	87,878	99,677	242	232,112	282,831
and Average -		655	912		1,087	1,363		888	1,007		959	1,168

TABLE V.

List showing the Average Tonnage of the Ships, and the Average Tonnage of their Cargoes, for each month of the period January 1st to September 30th, 1873. The ships being the same as those referred to in Table IV.

1873.	South Atlantic Voyages.		East Indian Voyages.		West Coast Voyages.	
	Average tonnage of ship.	Average number of tons of cargo.	Average tonnage of ship.	Average number of tons of cargo.	Average tonnage of ship.	Average number of tons of cargo.
January - -	269	511	1,178	1,569	861	1,166
February - -	721	1,021	1,145	1,653	871	1,146
March - - -	988	1,365	1,227	1,617	796	1,040
April - - -	577	787	954	1,274	1,076	1,341
May - - -	815	1,098	1,130	1,478	923	901
June - - -	317	693	1,239	1,598	884	1,168
July - - -	735	914	1,036	1,405	691	824
August - - -	553	837	1,010	1,420	677	877
September - -	675	988	1,084	1,425	621	880

Similar List of Averages for the same Period of 1874.

1874.	South Atlantic Voyages.		East Indian Voyages.		West Coast Voyages.	
	Average tonnage of ship.	Average number of tons of cargo.	Average tonnage of ship.	Average number of tons of cargo.	Average tonnage of ship.	Average number of tons of cargo.
January - -	642	843	1,038	1,369	827	704
February - -	796	1,075	1,301	1,983	611	788
March - - -	697	1,019	1,142	1,478	846	1,033
April - - -	571	623	1,072	1,423	1,033	904
May - - -	544	723	1,023	1,336	717	917
June - - -	—	—	996	1,328	711	886
July - - -	414	633	921	1,191	955	1,073
August - - -	564	867	1,011	1,503	1,039	1,129
September - -	511	796	1,315	1,174	853	1,057

PARTICULARS of COAL CARGOES taken on previous Voyages by some of the Vessels which were reported in 1874 with their Cargoes of Coal heated or on fire :—

"Centaur," left Liverpool, August 1871, for Rio Janeiro, with 1,771 tons of Westminster Brymbo main coal, loaded at Birkenhead.

"Glory of the Seas," left the Mersey in July 1872, for San Francisco, with 1,502 tons of Westminster Brymbo steam coal, loaded at Birkenhead.

"Mogul," left Cardiff for Rio Janeiro in November 1872, with about 1,750 tons of South Wales Coal, shipped by Messrs. Goodwin, Barnes, & Co.

"St. Nicholas," left the Mersey in June 1873, with 2,262 tons of South Wales coal, shipped under the tip at Birkenhead, for Callao.

"Euxine," left the Mersey 21st April 1873, for Aden, with 1,950 tons of North Wales coal, shipped at Birkenhead by hand.

"Monmouthshire," left Newcastle, 8th June 1871, for San Francisco, with a cargo of East Coast coal;—left Ardrossan, 13th August 1872, for San Frncisco, with a cargo of Scotch coal;—left Birkenhead, 14th August 1873, for San Francisco, with 1,382 tons of North Wales coal, loaded from the tip.

"Staffordshire," left Dundee, 3rd June 1871, for Calcutta, with a cargo of Scotch coal, and subsequently left Sydney, New South Wales, for San Francisco, with a cargo of Australian coal.

Litherland Park, near Liverpool,
3rd April 1876.

SIR,

IN continuation of the statistics before sent to the Royal Commission, I now beg to forward to you the accompanying tables.

My former statistics related to the number of vessels with cargoes of 500 tons of coal and upwards which had sailed from the United Kingdom on voyages beyond the Equator during the first nine months of the years 1873 and 1874, and to the number of these cargoes in which spontaneous combustion had been reported. I have now extended my data so as to comprise the whole twelve months of each of these years, and have further added similar statistics for the year 1875.

These additions do not materially affect the conclusions previously arrived at.

Tables I., II., III. show the number of vessels (under the same limiting conditions as before) which have sailed from each port for each month of the three years 1873, 1874, and 1875, and the number of cases of heating and fire which have occurred out of these sailings.

Table IV. shows the number of vessels which have sailed from each port for (1) the South Atlantic, (2) the East Indies, and (3) the West Coast, during each of the three years in question.

Tables V., VI., and VII. relate to further particulars respecting the Mersey shipments merely. The number of vessels which have sailed from the Mersey are here given, and the number which have sailed from the Liverpool side distinguished from the number which have sailed from the Birkenhead side, and whether loaded by hand or by tip. The cases of heating are printed between parentheses.

A comparison of Tables I., II., and III. shows that for 1873 there were 26 casualties out of 1,506 sailings, or about 1·7 per cent.; for 1874 there were 54 casualties out of 1,773 sailings, or just over 3 per cent., and for 1875 there were 22 casualties out of 1,619 sailings, or about 1·36 per cent.

Hence it would appear that the great increase of the per-centage of accidents in 1874 has not been maintained in 1875; but that, on the contrary, the per-centage for 1875 is lower even than that for 1873.

An examination of the statistics for the several districts indicates which district shows the greatest relative improvement in 1875 over 1874.

The per-centage for the East Coast has diminished from about 4 to about 1½. That for South Wales from about 1·3 to 0·96. That for Liverpool, from nearly 10 to 3·6; for Birkenhead, from nearly 2 to 1; for Scotland, from 6·6 to 1·3.

The greatest improvement has, therefore, been in Scotch coal. Lancashire coal comes next, and then the East Coast coal. The improvement in the East Coast coal is the more marked from the fact that the per-centage of loss was as high in 1873 as in 1874; while the other districts, particularly the Mersey and Scotch districts, showed such a great increase in 1874 over 1873. Further, if the Yorkshire and London statistics be taken from the returns for the East Coast with which they are included, the decrease in the per-centage of heatings in cargoes from the East Coast (Northern ports) becomes still greater.

Reference to Table IV. will show on what class of voyage the number of casualties have been greatest, and from which districts.

It will be seen that the West Coast voyages are still those which give the preponderance of casualties. The per-centage for West Coast voyages in 1875 is over 3; while, for the East Indian voyages it is under 1, and for South Atlantic voyages it is so low as 0·2.

The East Coast coal contributes the largest per-centage of West Coast casualties. The per-centage is even greater than that for 1874, and this is entirely owing to the Yorkshire shipments.

The remaining tables, V., VI., and VII., refer merely to the Mersey coal shipments, and show that in 1873, 86 vessels loaded at the tip, out of the 264 which sailed, or under 33 per cent.; that in 1874, 144 out of the 357 which sailed, or over 40 per cent.; and that in 1875, 118 out of the 335 which sailed, or about 35 per cent.

I believe these Liverpool statistics respecting the way in which the cargoes were loaded to be fairly accurate; but as they have been compiled within the last few days from the newspaper dock lists and other non-official sources, they may possibly be subject to a few slight errors.*

In addition, I also beg to send you a classified list of those vessels which have sailed from the United Kingdom during 1875, in the cargoes of which spontaneous combustion has been reported. The list gives particulars of ship and cargo.

I am, &c.

The Secretary, R. COOPER RUNDELL.
Royal Commission on Spontaneous
Combustion of Coals, London.

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* The sailings for the chief of the other coal ports have been obtained from Brown's Monthly Export Lists. In these lists, when more than one vessel have sailed during the month from either port for the same voyage, only the aggregate of their cargoes is given, and not the number of tons of cargo belonging to each vessel. In the tables each vessel is included if the aggregate tonnage of the cargo gives an average of more than 500 tons, and unless the average amounts to 500 tons the vessels are excluded. The tables are thus subject to any slight errors which this may occasion.

TABLE I.—TABLE showing the Number of Vessels which have sailed (S.) from the under-mentioned Ports with 500 Tons of Coal and upwards on Voyages across the Equator, and through the Suez Canal, during each Month of the Year 1873, and the Number of Cases of Heating and Fire (H.) which have occurred out of these Sailings.

——	January.		February.		March.		April.		May.		June.		July.		August.		September.		October.		November.		December.		Total for each Port.	
	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.
EAST COAST DISTRICT.																										
NORTHERN PORTS.																										
Amble (Warkworth) and Seaham	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Tyne Newcastle	21	1	10	0	10	0	12	0	19	1	10	1	25	0	7	0	25	4	22	0	14	0	9	1	184	8
Tyne North Shields	4		3		3		2		3		2		3		6		8		5		2		9		50	
Tyne South Shields	4		2		0		1		2		2		0		1		3		1		1		1		18	
Sunderland	6	0	5	0	4	0	5	0	5	0	5	0	7	0	4	0	5	0	10	0	8	0	16	0	80	0
Blyth	1	0	0	0	0	0	0	0	1	0	0	0	0	0	0	0	0	0	0	0	1	0	2	0	5	0
West Hartlepool (including Hartlepool)	2	0	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	0	1	0	0	0	5	0
Stockton and Middlesborough	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
YORKSHIRE PORTS.																										
Hull	1	0	0	0	0	0	0	0	1	1	1	1	1	0	2	2	0	0	1	0	2	0	1	0	10	4
Goole	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Grimsby	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	0	1	0	0	0	2	0
LONDON	1	1	5	0	2	0	0	0	4	0	2	1	4	0	4	1	0	0	0	0	1	0	0	0	23	3
Total for East Coast District	40	2	26	0	19	0	20	0	35	2	22	3	40	0	24	3	41	4	41	0	31	0	38	1	377	15
SOUTH WALES DISTRICT.																										
Newport	0	0	1	0	11	0	6	0	13	0	2	1	5	0	5	0	2	1	1	0	1	0	5	0	52	2
Llanelly, including Carmarthen, Pembrey, Lougher, and Ferryside.	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Cardiff, including Penarth	32	0	42	0	48	0	60	0	61	1	52	0	52	1	35	1	88	0	68	0	44	0	41	0	623	3
Swansea, including Neath, Port Talbot, and Porthcawl	6	0	7	0	7	0	6	0	8	0	8	1	12	0	8	0	10	1	2	0	7	0	6	0	87	2
Total for South Wales District	38	0	50	0	66	0	72	0	82	1	62	2	69	1	48	1	100	2	71	0	52	0	52	0	762	7
LIVERPOOL DISTRICT.																										
Liverpool	12	0	4	0	5	0	10	0	7	0	6	0	12	0	10	0	7	0	11	0	5	0	7	0	96	0
Birkenhead	7	0	12	0	14	0	12	0	14	0	9	0	14	0	19	0	20	0	18	1	13	0	16	0	168	1
Whitehaven	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Total for Liverpool District	19	0	16	0	19	0	22	0	21	0	15	0	26	0	29	0	27	0	29	1	18	0	23	0	264	1
SCOTCH DISTRICT.																										
EASTERN PORTS.																										
Leith and Granton	1	0	1	0	1	0	3	0	3	0	2	0	3	0	1	0	2	0	0	0	0	0	1	0	18	0
Grangemouth, Borrowstouness, Inverkeithing, Charlestown, and St. David's.	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Alloa (including Kennet Pans)	0	0	0	0	0	0	0	0	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	0
Dundee	0	0	1	0	1	0	0	0	0	0	2	0	3	2	1	1	1	0	1	0	1	0	0	0	11	3
WESTERN PORTS.																										
Ayr (including Girvan) and Irvine	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Troon	0	0	0	0	0	0	2	0	0	0	2	0	0	0	1	0	3	0	1	0	1	0	0	0	10	0
Ardrossan	1	0	1	0	1	0	2	0	4	0	1	0	0	0	0	0	1	0	0	0	0	0	0	0	11	0
Clyde Greenock	2	0	0	0	1	0	4	0	4	0	7	0	5	0	4	0	3	0	3	0	3	0	2	0	38	0
Clyde Port Glasgow	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	0	0	0	0	0	0	0	1	0
Clyde Glasgow	2	0	0	0	0	0	2	0	0	0	1	0	1	0	2	0	4	0	0	0	1	0	0	0	13	0
Clyde Bowling Bay	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Total for Scotch District	6	0	3	0	4	0	13	0	12	0	15	0	12	2	9	1	15	0	5	0	6	0	3	0	103	3
General total for each class of voyage	103	2	95	0	108	0	127	0	150	3	114	5	147	3	110	5	183	6	146	1	107	0	116	1	1,506	26

TABLE II.—Table showing the Number of Vessels which have sailed (S.) from the under-mentioned Ports with 500 tons of coal and upwards on Voyages
during the year 1874, and the Number of Cases of Heating and Fire (H.) which have occurred out of these Sailings.

—	January.		February.		March.		April.		May.		June.		July.		August.		September.		October.		November.		December.		Total for each Port.	
	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.
EAST COAST DISTRICT.																										
NORTHERN PORTS.																										
Amble (Warkworth) and Seaham	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Tyne: Newcastle	14	0	22	0	16	0	7	0	16	2	13	3	27	5	15	2	30	2	17	0	14	0	19	0	210	14
Tyne: North Shields	5		5		5		1		3		6		3		5		9		3		6		2		53	
Tyne: South Shields	0		1		2		0		0		2		1		3		1		2		0		0		12	
Sunderland	4	0	8	0	8	0	3	0	7	0	4	0	10	0	7	1	14	0	12	0	12	0	1	0	90	1
Blyth	0	0	0	0	2	0	0	0	0	0	0	0	0	0	0	0	0	0	1	0	0	0	0	0	3	0
West Hartlepool (including Hartlepool)	1	0	0	0	0	0	0	0	0	0	0	0	0	0	2	0	1	0	0	0	0	0	1	0	5	0
Stockton and Middlesborough	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
YORKSHIRE PORTS.																										
Hull	0	0	0	0	0	0	0	0	0	0	1	0	1	0	1	0	6	0	1	1	1	0	1	0	12	1
Goole	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Grimsby	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	0
LONDON	2	0	1	0	1	0	3	0	3	0	3	0	3	0	1	0	2	0	4	0	0	0	1	0	24	0
Total for East Coast District	27	0	37	0	34	0	14	0	29	2	29	3	45	5	34	3	63	2	40	1	33	0	25	0	410	16
SOUTH WALES DISTRICT.																										
Newport	3	0	2	0	4	0	5	0	3	0	4	0	6	0	3	0	7	0	6	0	4	0	5	0	52	0
Llanelly, including Carmarthen, Pembrey, Lougher, and Ferryside.	0	0	0	0	0	0	0	0	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	0
Cardiff, including Penarth	47	0	44	0	57	0	38	1	52	2	35	0	58	3	68	3	85	0	92	0	77	0	67	0	720	9
Swansea, including Neath, Port Talbot, and Porthcawl	3	0	8	0	8	0	5	0	6	0	9	0	8	1	13	0	7	0	8	1	11	0	11	0	97	2
Total for South Wales District	53	0	54	0	69	0	48	1	62	2	48	0	72	4	84	3	99	0	106	1	92	0	83	0	870	11
LIVERPOOL DISTRICT.																										
Liverpool	3	0	9	0	7	0	7	1	8	1	13	3	17	3	26	3	19	2	16	1	10	0	11	0	146	14
Birkenhead	11	0	10	0	12	0	17	0	15	0	17	1	14	1	16	0	21	1	26	1	24	0	28	0	211	4
Whitehaven	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Total for Liverpool District	14	0	19	0	19	0	24	1	23	1	30	4	31	4	42	3	40	3	42	2	34	0	39	0	357	18
SCOTCH DISTRICT.																										
EASTERN PORTS.																										
Leith and Granton	0	0	1	0	3	0	1	0	2	0	0	0	1	0	2	0	1	1	1	0	0	0	2	0	14	1
Grangemouth, Borrowstouness, Inverkeithing, Charlestown, and St. David's.	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Alloa (including Kennet Pans)	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Dundee	0	0	1	0	0	0	3	1	1	0	2	1	1	0	0	0	1	0	1	0	0	0	0	0	10	2
WESTERN PORTS.																										
Ayr (including Girvan) and Irvine	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Ardrossan	0	0	0	0	1	0	0	0	2	0	0	0	1	0	2	0	2	0	1	0	0	0	0	0	9	0
Troon	0	0	1	0	2	0	1	0	0	0	2	0	1	0	1	0	2	0	0	0	1	0	0	0	11	0
Clyde: Greenock	0	0	3	0	1	0	3	0	3	1	3	1	5	4	4	0	8	0	10	0	2	0	6	0	48	6
Clyde: Port Glasgow	0		0		0		0		0		1		1		0		0		1		0		0		3	
Clyde: Glasgow	3		3		0		3		6		1		3		3		5		3		7		4		41	
Clyde: Bowling Bay	0		0		0		0		0		0		0		0		0		0		0		0		0	
Total for Scotch District	3	0	9	0	7	0	11	1	14	1	9	2	13	4	12	0	19	1	17	0	10	0	12	0	136	9
General Total for each Class of Voyage	97	0	119	0	129	0	97	3	128	6	116	9	161	17	172	9	221	6	205	4	169	0	159	0	1,773	54

TABLE III.—TABLE showing the Number of Vessels which have sailed (S.) from the under-mentioned Ports with 500 Tons of Coal and upwards on Voyages across the Equator, and through the Suez Canal, during each Month of the Year 1875, and the Number of Cases of Heating and Fire (H.) which have occurred out of these Sailings.

—	January.		February.		March.		April.		May.		June.		July.		August.		September.		October.		November.		December.		Total for each Port.	
	S.	H.	S.	H.	S.	H.	S.	H.	S.	.	S.	H.	S.	H.	S.		S.	H.	S.	H.	S.	H.	S.	H.	S.	H.
EAST COAST DISTRICT.																										
NORTHERN PORTS.																										
Amble (Warkworth) and Seaham	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0		
Tyne { Newcastle	21	0	14	0	19	0	14	0	20	0	5	1	20	1	19	1	19	1	12	0	10	0	13	0	206	4
North Shields	5		5		3		3		3		2		3		1		2		1		2		3		33	
South Shields	0		1		0		2		0		23		0		0		0		0		0		0		6	
Sunderland	5	0	10	0	13	0	5	0	7	0	7	0	4	0	10	0	13	0	7	0	12	0	4	0	97	0
Blyth	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
West Hartlepool (including Hartlepool)	1	0	0	0	0	0	0	0	0	0	1	0	1	0	1	0	0	0	0	0	0	0	2	0	6	0
Stockton and Middlesborough	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
YORKSHIRE PORTS.																										
Hull	1	0	3	0	1	0	1	0	1	0	1	0	0	0	5	0	2	0	2	2	4	0	2	0	23	2
Goole	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Grimsby	0	0	0	0	1	0	0	0	0	0	0	0	1	0	1	0	0	0	2	0	3	0	0	0	8	0
LONDON	2	0	2	0	0	0	1	0	2	0	1	0	1	0	2	0	1	0	4	0	1	0	0	0	17	0
Total for East Coast District	35	0	35	0	37	0	26	0	33	0	40	1	30	1	39	1	37	1	28	2	32	0	24	0	396	6
SOUTH WALES DISTRICT.																										
Newport	2	0	2	0	0	0	2	0	2	0	3	0	11	0	3	0	3	0	3	0	3	0	3	0	37	0
Llanelly, including Carmarthen, Pembrey, Lougher, and Ferryside.	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	0	0	0	0	0	0	0	1	0
Cardiff, including Penarth	24	0	17	0	31	2	26	0	23	0	61	0	73	1	88	0	86	1	100	0	50	0	43	0	622	4
Swansea, including Neath, Port Talbot, and Porthcawl	8	0	4	0	6	0	3	0	12	0	6	0	1	1	3	0	7	2	7	0	9	0	5	0	71	3
Total for South Wales District	34	0	23	0	37	2	31	0	37	0	70	0	85	2	94	0	97	3	110	0	62	0	51	0	731	7
LIVERPOOL DISTRICT.																										
Liverpool	8	0	8	0	13	0	12	1	4	0	12	2	17	1	7	0	11	1	18	0	10	0	17	0	137	5
Birkenhead	15	0	19	0	15	0	12	0	14	0	19	1	21	0	6	0	17	1	23	0	17	0	20	0	198	2
Whitehaven	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Total for Liverpool District	23	0	27	0	28	0	24	1	18	0	31	3	38	1	13	0	28	2	41	0	27	0	37	0	335	7
SCOTCH DISTRICT.																										
EASTERN PORTS.																										
Leith and Granton	1	0	0	0	0	0	2	0	0	0	5	0	0	0	0	0	2	0	0	0	0	0	2	0	12	0
Grangemouth, Borrowstouness, Inverkeithing, Charlestown, and St. David's.	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Alloa (including Kennet Pans)	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Dundee	1	0	0	0	0	0	0	0	1	0	0	0	0	0	1	0	1	0	1	0	1	0	0	0	6	0
WESTERN PORTS.																										
Ayr (including Girvan) and Irvine	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Ardrossan	1	0	0	0	3	0	0	0	0	0	0	0	0	0	0	0	1	0	2	0	1	0	1	0	9	0
Troon	0	0	0	0	3	0	1	0	0	0	0	0	0	0	1	0	0	0	1	0	0	0	0	0	6	0
Clyde { Greenock	4	0	2	0	2	0	3	0	6	1	11	0	6	0	2	1	10	0	7	0	5	0	1	0	59	2
Port Glasgow	0		0		0		0		0		0		1		0		0		0		0		1		2	
Glasgow	0		2		6		7		11		6		3		8		6		7		5		2		63	
Bowling Bay	0		0		0		0		0		0		0		0		0		0		0		0		0	
Total for Scotch District	7	0	4	0	14	0	13	0	18	1	22	0	10	0	12	1	20	0	18	0	12	0	7	0	157	2
General total for each class of voyage	99	0	89	0	116	2	94	1	106	1	163	4	163	4	158	2	182	6	197	2	133	0	119	0	1,619	22

including the Cape of Good Hope; (2) for the East Indies, including Australia, China, and Japan; and (3) for the West Coast of America, during the Years 1873, 1874, and 1875; and showing out of these Sailings the Number, as far as can be ascertained, of Heatings and Fires (H.) from Spontaneous Combustion.

—	South Atlantic Voyages.						East Indian Voyages.						West Coast Voyages.						Total for each District.					
	1873.		1874.		1875		1873.		1874.		1875.		1873.		1874.		1875.		1873.		1874.		1875.	
	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.	S.	H.
EAST COAST DISTRICT.																								
NORTHERN PORTS.																								
Amble (Warkworth) and Seaham	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Tyne Newcastle	57	0	76	0	87	0	109	7	92	12	90	3	18	1	42	2	29	1	184	8	210	14	206	4
North Shields	8		3		1	0	40		38		26		2		12		6		50		53		33	
South Shields	1		2		1	0	11		6		4		6		4		1		18		12		6	
Sunderland	15	0	12	0	23	0	65	0	53	0	64	0	0	0	25	1	10	0	80	0	90	1	97	0
Blythe	2	0	1	0	0	0	3	0	2	0	0	0	0	0	0	0	0	0	5	0	3	–	0	–
West Hartlepool (including Hartlepool)	0	0	0	0	2	0	4	0	3	0	1	0	1	0	2	0	3	0	5	0	5	–	6	–
Stockton and Middlesborough	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	–	0	–
YORKSHIRE PORTS.																								
Hull	6	3	7	0	9	0	3	1	4	1	6	0	1	0	1	0	8	2	10	4	12	1	23	2
Goole	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	–	0	–
Grimsby	0	0	0	0	0	0	2	0	1	0	8	0	0	0	0	0	0	0	2	0	1	–	8	–
LONDON	1	0	0	0	0	0	22	3	23	0	16	0	0	0	1	0	1	0	23	3	24	–	17	–
Total for East Coast District	90	3	101	0	123	0	259	11	222	13	215	3	28	1	87	3	58	3	377	15	410	16	396	6
SOUTH WALES DISTRICT.																								
Newport	31	0	28	0	20	0	16	0	15	0	13	0	5	1	9	0	4	0	52	1	52	0	37	–
Llanelly, including Carmarthen, Pembray, Lougher, and Ferryside	0	0	0	0	0	0	0	0	1	0	1	0	0	1	0	0	0	0	0	1	1	0	1	–
Cardiff, including Penarth	289	2	340	0	254	1	242	1	309	3	297	1	92	0	71	6	71	2	623	3	720	9	622	4
Swansea, including Neath, Port Talbot, and Porthcawl	12	0	13	0	1	0	4	0	2	0	1	0	71	2	82	2	69	3	87	2	97	2	71	3
Total for South Wales District	332	2	381	0	275	1	262	1	327	3	312	1	168	4	162	8	144	5	762	7	870	11	731	7
LIVERPOOL DISTRICT.																								
Liverpool	30	0	21	0	15	0	12	0	14	1	14	0	54	0	111	13	108	5	96	0	146	14	137	5
Birkenhead	23	0	17	0	23	0	104	0	152	3	142	2	41	1	42	1	33	0	168	1	211	4	198	2
Whitehaven	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	–
Total for Liverpool District	53	0	38	0	38	0	116	0	166	4	156	2	95	1	153	14	141	5	264	1	357	18	335	7
SCOTCH DISTRICT.																								
EASTERN PORTS.																								
Leith and Granton	14	0	9	1	7	0	0	0	0	0	2	0	4	0	5	0	3	0	18	–	14	1	12	–
Grangemouth, Borrowstouness, Inverkeithing, Charlestown, and St. David's.	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	–	0	0	0	–
Alloa (including Kennet Pans)	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	–	0	0	0	–
Dundee	1	0	1	0	0	0	10	3	8	2	3	0	0	0	1	0	3	0	11	3	10	2	6	–
WESTERN PORTS.																								
Ayr (including Girvan) and Irvine	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	–	0	0	0	–
Ardrossan	1	0	6	0	0	0	2	0	0	0	2	0	8	0	3	0	7	0	11	–	9	0	9	–
Troon	7	0	11	0	4	0	2	0	0	0	1	0	1	0	0	0	1	0	10	–	11	0	6	–
Clyde Greenock	18	0	16	2	25	0	14	0	26	3	21	1	6	0	6	1	13	1	38	–	48	–	59	1
Port Glasgow	0		1		0		1		2		0	0	0		0		2		1	–	3	6	2	1
Glasgow	4		10		21		5		13		10	0	4		18		32		13	–	41	–	63	–
Bowling Bay	0		0		0		0		0		0	0	0		0		0		0	–	0	–	0	–
Total for Scotch District	46	0	54	3	57	0	34	3	49	5	39	1	23	0	33	1	61	1	103	3	136	9	157	2
General total for each class of voyage	521	5	574	3	493	1	671	15	764	25	722	7	314	6	435	26	404	14	1,506	26	1,773	54	1,619	22

TABLE V.

Showing for each Month of 1873 what number of the Vessels which Sailed from Liverpool and from Birkenhead with 500 Tons of Coal and upwards on voyages across the Equator, and through the Suez Canal, had been loaded by Tip or by Hand.

The Casualties are printed between Parentheses.

1873.	South Atlantic.				East Indies.				West Coast of America.				Totals.			
	Tip.		Hand.		Tip.		Hand.		Tip.		Hand.		Tip.		Hand.	
January.																
Liverpool - -	0		0		1		0		5		6		6		6	
Birkenhead - -	1	1	0	0	1*	2	3	3	0	5	2	8	2	8	5	11
February.																
Liverpool - -	0		2		1		0		0		1		1		3	
Birkenhead - -	0	0	0	2	0	1	7	7	4	4	1	2	4	5	8	11
March.																
Liverpool - -	0		2		1		1		1		0		2		3	
Birkenhead - -	1	1	5	7	0	1	8	9	0	1	0	0	1	3	13	16
April.																
Liverpool - -	1		1		2		2		2		2		5		5	
Birkenhead - -	2	3	0	1	0	2	5	7	3	5	2	4	5	10	7	12
May.																
Liverpool - -	1		2		0		1		2		1		3		4	
Birkenhead - -	1	2	2	4	0	0	9	10	0	2	2	3	1	4	13	17
June.																
Liverpool - -	0		1		1		0		2		2		3		3	
Birkenhead - -	0	0	0	1	1	2	6	6	2	4	0	2	3	6	6	9
July.																
Liverpool - -	1		5		0		1		2		3		3		9	
Birkenhead - -	0	1	2	7	0	0	4	5	6	8	2	5	6	9	8	17
August.																
Liverpool - -	0		2		0		0		3		5		3		7	
Birkenhead - -	3	3	0	2	1	1	10	10	4	7	1	6	8	11	11	18
September.																
Liverpool - -	0		3		0		0		2		2		2		5	
Birkenhead - -	1	1	0	3	3	3	11	11	3	5	2	4	7	9	13	18
October.																
Liverpool - -	0		5		1		0		3		2		4		7	
Birkenhead - -	0	0	0	5	2	3	12	12	(1) 2	5	2	4	(1) 4	8	14	21
November.																
Liverpool - -	1		1		0		0		2		1		3		2	
Birkenhead - -	1	2	0	1	2	2	8	8	0	2	2	3	3	6	10	12
December.																
Liverpool - -	0		2		0		0		2		3		2		5	
Birkenhead - -	1	1	3	5	4†	4	7	7	0	2	1	4	5	7	11	16
Totals :																
Liverpool - -	4		26		7		5		26		28		37		59	
Birkenhead - -	11	15	12	38	14	21	90	95	(1) 24	50	17	45	(1) 49	86	119	178

* 1 steamer through the Canal. † 2 steamers through the Canal,

TABLE VI.

Showing for each Month of 1874 what number of the Vessels which Sailed from Liverpool and from Birkenhead with 500 Tons of Coal and upwards on voyages across the Equator, and through the Suez Canal, had been Loaded by Tip or by Hand.

1874.	South Atlantic.				East Indies.				West Coast of America.				Totals.			
	Tip		Hand.		Tip.		Hand.		Tip.		Hand.		Tip.		Hand.	
January.																
Liverpool -	2		0		1		0		0		0		3		0	
Birkenhead -	0	2	0	0	*5	6	5	5	0	0	1	1	5	8	6	6
February.																
Liverpool -	3		1		0		0		3		2		6		3	
Birkenhead -	0	3	4	5	0	0	4	4	2	5	0	2	2	8	8	11
March.																
Liverpool -	0		2		0		1		1		3		1		6	
Birkenhead -	1	1	2	4	1	1	8	9	0	1	0	3	2	3	10	16
April.																
Liverpool -	0		0		0		0		(1) 1		6		(1) 1		6	
Birkenhead -	1	1	0	0	5	5	9	9	1	2	1	7	7	8	10	16
May.																
Liverpool -	0		3		0		1		(1) 1		3		(1) 1		7	
Birkenhead -	1	1	0	3	3	3	9	10	1	2	1	4	5	6	10	17
June.																
Liverpool -	0		0		(1) 1		1		(2) 7		4		(3) 8		5	
Birkenhead -	0	0	0	0	0	1	15	16	1	8	(1) 1	5	1	9	(1) 16	21
July.																
Liverpool -	1		0		0		1		(3) 10		5		(3) 11		6	
Birkenhead -	0	1	0	0	(1) 2	2	8	9	3	13	1	6	(1) 5	16	9	15
Forward :																
Liverpool -	6		6		2		4		(7) 23		23		(8) 31		33	
Birkenhead -	3	9	6	12	(2) 16	18	58	62	8	31	(1) 5	28	(1) 27	58	(1) 69	102

* 5 steamers through the Canal.

TABLE VI.—*continued.*

1874.	South Atlantic.				East Indies.				West Coast of America.				Totals.			
	Tip.		Hand.		Tip.		Hand.		Tip.		Hand.		Tip.		Hand.	
Forward :																
Liverpool	6		6		2		4		(7) 23		23		(8) 31		33	
Birkenhead	3	9	6	12	(2)16	18	58	62	8	31	(1) 5	28	(1) 27	58	(1) 69	102
August.																
Liverpool	0		2		0		3		(3) 10		11		(3) 10		16	
Birkenhead	1	1	0	2	1	1	12	15	1	11	1	12	3	13	13	29
September.																
Liverpool	1		1		1		1		(2) 10		5		(2) 12		7	
Birkenhead	0	1	0	1	(1) 7	8	12	13	1	11	1	6	(1) 8	20	13	20
October.																
Liverpool	0		3		1		1		6		(1) 5		7		(1) 9	
Birkenhead	2	2	0	3	(1) 8	9	11	12	1	7	4	9	(1) 11	18	15	24
November.																
Liverpool	1		1		0		0		6		2		7		3	
Birkenhead	0	1	0	1	6	6	7	7	5	11	6	8	11	18	13	1
December.																
Liverpool	0		0		0		1		7		3		7		4	
Birkenhead	2	2	3	3	3	3	11	12	5	12	4	7	10	17	18	22
Totals :																
Liverpool	8		13		(1) 4		10		(12) 62		(1) 49		(13) 74		(1) 72	
Birkenhead	8	16	9	22	(3) 41	45	111	121	21	83	(1) 21	70	(3) 70	144	(1) 141	213

TABLE VII.

SHOWING for each month of 1875 what number of the Vessels which sailed from Liverpool and from Birkenhead with 500 tons of Coal and upwards on Voyages across the Equator, and through the Suez Canal, had been loaded by Tip or by Hand.

1875.	South Atlantic.				East Indies.				West Coast of America.				Totals.			
	Tip.		Hand.		Tip.		Hand.		Tip.		Hand.		Tip.		Hand.	
January.																
Liverpool	0		2		0		0		5		1		5		3	
Birkenhead	0	0	4	6	1	1	9	9	1	6	0	1	2	7	13	16
February.																
Liverpool	0		1		1		0		4		2		5		3	
Birkenhead	0	0	0	1	2	3	14	14	1	5	2	4	3	8	16	19
March.																
Liverpool	1		0		2		0		6		4		9		4	
Birkenhead	1	2	1	1	1	3	8	8	0	6	4	8	2	11	13	17
April.																
Liverpool	0		0		0		0		(1) 6		6		(1) 6		6	
Birkenhead	1	1	2	2	0	0	6	6	0	6	3	9	1	7	11	17
May.																
Liverpool	0		1		1		0		0		2		1		3	
Birkenhead	0	0	3	4	0	1	4	4	2	2	5	7	2	3	12	15
June.																
Liverpool	2		0		1		2		(2) 6		1		(2) 9		3	
Birkenhead	0	2	1	1	1	2	(1) 13	15	1	7	3	4	2	11	(1) 17	20
July.																
Liverpool	0		0		1		0		12		(1) 4		13		(1) 4	
Birkenhead	0	0	1	1	2	3	15	15	1	13	2	6	3	16	18	22
August.																
Liverpool	1		0		1		0		2		3		4		3	
Birkenhead	0	1	0	0	0	1	5	5	1	3	0	3	1	5	5	8
September.																
Liverpool	1		1		1		0		(1) 4		4		(1) 6		5	
Birkenhead	0	1	1	2	(1) 4	5	11	11	1	5	0	4	(1) 5	11	12	17
October.																
Liverpool	1		1		0		1		12		3		13		5	
Birkenhead	1	2	0	1	2	2	17	18	2	14	1	4	5	18	18	23
November.																
Liverpool	1		0		2		0		4		3		7		3	
Birkenhead	3	4	1	1	2	4	10	10	0	4	1	4	5	12	12	15
December.																
Liverpool	0		2		0		1		7		7		7		10	
Birkenhead	0	0	3	5	1	1	14	15	1	8	1	8	2	9	18	28
Totals :																
Liverpool	7		8		10		4		(4) 68		(1) 40		(4) 85		(1) 52	
Birkenhead	6	13	17	25	(1) 16	26	(1) 126	130	11	79	22	62	(1) 33	118	(1) 165	217

CLASSIFIED LIST of VESSELS which have been reported to have their Cargoes of Coal Heated or Fired from Spontaneous Combustion, and have sailed from the United Kingdom for Voyages South of the Equator, during the year 1875.

District. Port of Shipment. Name of Vessel.	Nationality.	When built, and how classed.	Iron or wood.	Net Register Tonnage.	Depth of Hold.	Number of Decks and No. laid.	Quantity and Description of Coal.	Name of Coal Agent or Shipper.	Kind of Ventilation, if any.	Date of Sailing.	Destination.	Date of Casualty, or when the signs of Heating were first discovered.	Days out.	Where the Casualty occurred.	Nature of Casualty.	Remarks.
EAST COAST PORTS.																
TYNE.																
Helen Morris	Am.	1868 3/3	W.	1,285	23·9	2 l.	1,653, Cowpen Hartley	J. Fothergill	Ventilated	7 June	San Francisco	24 Nov.	About 140	29° 14′ S. 87° W.	Burnt	Abandoned 28th November.
William Poolman	Dutch	1866 3 3	I.	1,546	27·3	3 1l.	1,611, Bowers W. Hartley	—	Ventilated	14 June	Java	24 Sept.	102	11° S. 9° E.	Ditto	Abandoned 27th September.
P.V.W.S. (Utrecht ?)	Dutch	1868 A1	C.	1,873	23·8	—	———	—	—	27 Aug.	Samarang	3 Nov.	68	42° S. 46° E.	Fire	Telegram from Batavia, Dec. 1st, states fire in cargo on voyage of "Utrecht."
Aberaman	Brit.	1875 100A.	I.	1,090	21·9	—	1,625, Cowpen Hartley	J. Fothergill	Masts and bits ventilated.	16 Sept.	Bombay	During discharge.	About 120	Bombay.	Heating	New ship. Arrived 11th January, and during discharge coal found in highly heated condition and almost on point of ignition.
HULL.																
Le Baron	Brit.	1870 3/3	W.	907	19	2 1l.	— West Riding Hartley	Pope and Pearson	Ventilated	18 Oct.	Valparaiso	6 Feb.	111	Off Cape Horn.	Burnt	—
Patmos	Am.	1859 5/6	W.	738	21·6	2 2l.	— Featherstone Main	—	—	18 Oct.	Ditto	30 Jan.	104	59° S. 77° W.	Ditto	Abandoned 31st January.
SOUTH WALES PORTS.																
CARDIFF.																
Independencia	It.	1872	W.	616	—	—	877, Rhondda Coal	J. R. Davies.	Not ventilated	17 Mar.	Valparaiso	p. 2 June	Prev. 77	Near Falkland Islands.	Heated	Arrived 2nd June and sailed again August 11th.
Quattro Fratelli	It.	1869	W.	550	19·3	2 1l.	750, Aber Rhondda bit. coal, and 200 Coed Cae coal.	Ditto and Coed Cae Co.	—	9 Mar.	Ditto	About 20 June.	About 103	Valparaiso.	Fire	Discovered shortly after arrival.
Domenico Galliano	It.	1865	W.	537	18	2 2l.	869, Mostly bituminous coal, (11 kinds of coal.)	—	—	6 July	Singapore	17 Oct.	103	28° S. 90° E.	Fire	Cargo on fire on voyage.
Landsborough	Brit.	1853	W.	1,066	23·8	—	1,567, Dynas smokeless coal, and 19 tons No. 3 Rhondda	—	—	25 Sept.	Rio Janeiro	About 23 Nov.	About 59	Rio Janeiro	Heated	Fire confined to the Rhondda coal, and found out on arrival.
SWANSEA.																
Powhattan	Brit.	1866 A1	W.	400	17·1	—	580, ———	T. Davies & Co.	—	25 July	Valparaiso	—	Prev. 75	—	Heated	Put into Falkland prev. 8th October with cargo heated. Had thrown some cargo overboard.
PORTHCAWL.																
Fanny	Brit.	1863 A1	W.	397	17·0	2 1l.	— Aber Thro' and Thro'	E. David	—	15 Sept.	Ditto	Prev. 27 Dec.	Prev. 103	—	—	Put in Coquimbo 27th December; cargo on fire.
Mabel	Brit.	1873 A1	W.	454	—	2 1l.	— Ditto	Ditto	—	17 Sept.	Ditto	—	—	—	—	—
LIVERPOOL DISTRICT.																
LIVERPOOL.																
Angelo Antonio	It.	1870	W.	595	—	2 1l.	900, Orrell Steam Coal	Jothn. Blundell	—	30 April	Callao	2 July	63	600 miles off St. Catherines, S. America.	Burnt	Loaded at tip; run ashore 14th July near St. Catherines, South America.
Frankfurt Hall	Brit.	1869 ⩓1	I.	728	19·0	—	804, Ditto	Orrell Coal Co.*	—	3 June	Valparaiso	29 Sept.	118	Valparaiso	Fire	Arrived 21st; fire discovered 29th and again on 4th October.
Clevedon	Brit.	1868 ⩓1	I.	698	18·8	—	1,047, Ditto (Tawd Vale)	F. A. Widdows & Co.	Cowls over coal	25 June	Ditto	21 Sept.	88	40° S. 77° W.	Burnt	Loaded at tip.
River Boyne	Brit.	1867 ⩓1	I.	499	17·3	—	766, Ditto	Orrell Coal Co.	—	15 July	Ditto	12 Oct.	88	57° S. 72° W.	Fire	Loaded by basket; beached at Cape Horn.
San Rafael	Brit.	1862	W.	955	22	2 2l.	1,312, of Tawd Vale and Crow Orchard Coal.	Ditto	Ventilated	18 Sept.	Ditto	4 Jan.	108	53° S. 76° W.	Burnt	Loaded under the tip.
BIRKENHEAD.																
Arracana	Brit.	1862	W.	1,192	23·9	2 1l.	1,756, Coed Talon	H. N. Hughes	Ventilated	19 June	Bombay	22 Oct.	126	13° N. 73° E.	Burnt	Abandoned 24th October.
Theresa	Brit.	1857 A.1	W.	705	20·6	2 1l.	920, Chell Colliery, Staffordshire	French & Co.	Ventilated	25 Sept.	Rangoon	6 Dec.	82	34° S. 17 E.	Burnt	Abandoned at Sea.
CLYDE DISTRICT.																
GREENOCK.																
Beucleuch	Brit.	1875 100 A.	I.	1,350	22	—	1,781, Watson's Hartley	John Watson	Ventilated	22 Aug.	Bombay	22 Nov.	92	16° N. 68° 24′ E.	Fire	New ship; first voyage; arrived 5th December. Cargo found on fire 22nd. Scuttled to allow water to rise to 10 feet.
Antelope	Brit.	1873 3/3	W.	1,306	23	2 2l.	1,273, Ditto	Ditto	Not ventilated but not full.	13 May	San Francisco	4 Oct.	144	San Francisco	Fire	Arrived San Francisco 22nd Sept.; took fire while discharging.

* Formerly W. H. Brancker.

(*h*.) PAPER by Mr. A. P. Sandeman, Surveyor under the Marine Department of the Board of Trade.

COAL CARGOES.

THE liability to spontaneous combustion on board coal-laden ships, and the fearful consequences which have lately been reported, appear, and reasonably so, to threaten a serious restriction to our foreign export coal trade, if not to entirely suspend it. We may with truth say a week now seldom passes without its sad tale of loss in many instances of life, as well as of valuable ships, and all attributable to this cause.

The question has been asked in the House whether these startling facts have been noticed, and in reply (through the President of the Board of Trade) the public are informed that a Royal Commission will possibly be appointed to inquire into the subject of "coal cargoes." But "in the meantime" the evil will continue, and this, when a remedy may at once be applied, appears to be a hard case. Ships coal-laden continue to leave our ports daily, whose cargoes are loaded in the same manner, no change whatever occurring to remedy the evil, or indeed any action shown "whatever," as to uneasiness or a sense of any increasing cause for alarm beyond that taken by *underwriters*, who have raised their rates of premium—a clear demonstration of the increased risk.

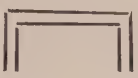
It may, and doubtless is, presumed by many that nothing can be done beyond what is being done to remedy or guard against serious evil, or it may be that they believe extra special care is being taken, or possibly they altogether ignore the practicability of doing anything that would further protect their vessels from the risk of being burnt and the lives of the crew from the perils which must necessarily attend such a calamity, and hence the evil continues and is likely to do so. I believe all shipowners are interested in this matter, but all are not agreed as to the causes which contribute to spontaneous combustion of coal cargoes. One class of shipowner will show constant interest by adopting all "ordinary precautions," a platform is laid, and vertical shafts are introduced at each hatch, and where no permanent 'tween deck is laid, fore and aft trunks are laid on or under the lower beams, with thwartship connexions to the vertical hatchway shafts. "This is all in the right direction," but it is inefficient, and requires to be extended more thoroughly through the cargo to be effective. In other instances the kelson *only* is ventilated; this is a matter of economy and is seldom of much use, as it is not frequently well or effectively done. The vertical hatch shafts are led into this, and if the line of kelson remains clear the circulation or current of air is to a limited extent maintained. In other cases again, and not a few, little or no interest is manifested "*whatever*" in ventilating, and no effort is made to prevent the coals heating or to carry off any combustible gas that may generate.

However carefully the ventilation may be arranged, even presuming it was sufficient to keep the cargo cool, so little interest is taken in the matter that it frequently gets damaged and is rendered useless before the loading is completed. I could instance cases where large vessels taking nearly 2,000 tons have been ventilated with all "ordinary" care, but that done no further interest was apparently manifested, the master may not have joined, the overlooker is merely on board occasionally, and the mate, as a rule, seldom knows anything about it. The coal trimmers are left in possession of the hold to load the vessel as they please, and they care little about the ventilators. One vessel in particular as a "case in point" came under my observation. She belonged to a Liverpool firm who are reputed for their care in ventilating; her kelson was ventilated, she had well-made ventilators in each hatch, and longitudinal shafts intermediate and in the wings which had connexion with the hatchway shafts, and appeared to be in order; as an underwriter's surveyor I had in ordinary course to report on the vessel, and I found the whole of the three hatch ventillators had got damaged and were filled with coal within a few feet of the top, and the coal had broken down the kelson ventilation both at the fore and after ends. I met one of the owners and the overlooker on board who were ignorant of the damage, and the mate could give no explanation.

It is very different with rice cargoes, for the preservation of which a most careful system of ventilation is adopted, which under ordinary circumstances in proportion to the cargo is about five times greater than what it is in coal-laden vessels. In the carriage of grain a loss may be attributed to the owner, or if the cargo sweats the reputation of the ship will suffer, and hence every care is taken to keep a cargo very liable to heat from doing so, master and officers are prompted and encouraged to superintend the laying of the ventilation, and every care is adopted to protect the owner from loss. But with coal cargoes, where life is frequently jeopardised, it appears to be almost completely the reverse; in most cases the coal is put on board heavily charged with moisture and liable to heat if confined under hatches. Ventilation may or may not be arranged, but as a rule no one seems to be employed to look after it, and to see that it is not damaged; the cargo is not unfrequently hurried in from the Birkenhead tips or high-level quays, and all is left to stevedores, and, the cargo in, the hatches are put on, and in many cases if combustion is the result it is little to be wondered at.

If coal-laden ships were placed on a par with rice-laden ships in the matter of ventilation, and the same observant care taken whilst loading, I am persuaded we should hear no more of these calamitous events. The system of ventilation would merely have to be more in detail, and if it is admitted that *perfect ventilation* will keep coal from heating, as it does a rice cargo, then detail becomes a matter of easy arrangement.

The platform, as in rice-laden ships, is most essential, as it affords a free circulation of air underneath. I would step large vertical shafts on the middle line in each of the hatchways to rise above the coamings, to be protected and secured so as to be open in all weathers. Other vertical shafts should be placed in proportion to the size of the vessel intermediate from the middle line in towards the wings spaced about 25 feet apart, all the vertical shafts to pass through the platform.



Section showing the vertical shafts and the intermediate fore and aft shafts leading into them; the fore and aft shafts are open underneath.

Fore and aft shafts should be placed in connexion with the intermediate and middle shafts on the lower deck beams and about half-way from lower deck to platform, and also fore and aft shafts in the wings on both sides; the whole of these longitudinal shafts to penetrate the cargo from end to end.

Transverse shafts should also be fitted to connect the fore and after with each other and with the vertical hatch shafts "always open," thus there would always be a current of fresh air passing through the cargo. Deck ventilators might also be fitted forward and aft to supply fresh and carry off heated air.

So ventilated there would be a column equal to a square foot of fresh air passing through every square block of 40 tons, and so ventilated it would be most improbable that coal, however wet or liable to sweat when confined, could do so so as to endanger spontaneous combustion. The cost would of course be much greater than it is at present constructed, but I apprehend it would not greatly exceed 1*s*. 6*d* per ton on her cargo, and when value is allowed for the material when sold at the port of destination it would be considerably less.

This system of ventilation has been advocated by the underwriters for years past, and their surveyors have been *specially instructed* to recommend its adoption; and the sooner it is insisted upon and made compulsory the sooner will we cease to hear of the sad and revolting catastrophes which have lately resulted from spontaneous combustion with coal cargoes on foreign voyages.

In meantime I would submit that until some remedy is adopted so that coal cargoes can be carried to distant ports without danger of fire, vessels so laden should be required to carry properly fitted "life-boats," found with mast and sail, and having provision tanks, water breakers, and boat compasses ready for use in case of need; and considering the painful evidence which we have had of the danger to which such cargoes expose the vessel, and consequently the crew, they appear to be special cases almost calling for supervision under section 12 of the Merchant Shipping Amendment Act of 1873, as defective in equipment (boats) for their intended voyage.

(Signed) A. P. SANDEMAN.

Liverpool, 3rd March 1875.

(*i.*) PAPER by Mr. Jeula. (See his Evidence, page 141, Q. 12.)

LOSSES OF COAL-LADEN VESSELS.

THE only way of really ascertaining the facts as to losses of ships and lives which have already occurred is to tabulate the number and dates of clearances for, say, five years, stating the vessel, her flag, size, crew all told, the description and quantity of coal shipped in her, and her voyage, with any other available particulars; if this were done, reliable information would soon be obtained, but a comparison between the number of voyages entered upon and the number of casualties occurring is absolutely necessary to a correct estimate of results.

I venture respectfully to suggest that the following heads of inquiry would be of importance for future guidance:—

A. The special character of different coal ordinarily shipped, on long or short voyages, as to elements of combustion, hardness or softness, and probable crumbling and breaking up, or the contrary?

B. The results of shipping it wet or dry, large or small, and by barrows or from a tip?

C. The influence of recent strikes upon the out-put, and the results of more immediate transfer of coal from the mine to the ship consequent thereon?

D. Whether, when laden in large or small vessels, it is more or less dangerous, and whether explosions are more frequent in small vessels and spontaneous combustion in large vessels?

E. Whether usually ventilated or not; whether different customs as to ventilation prevail at different coal ports, consequent upon special kinds of coal being shipped therefrom, or otherwise; and, if ventilated, whether at the top over the coal only, or by vertical, with or without horizontal, ventilators, near the keelson, or higher up in the body of the cargo, or by any or all of these means, or by any others, and if so, whether by wooden, metal, or earthenware ventilators; and the results of the various systems?

F. What means are generally employed for putting out a fire in a coal-laden vessel when it occurs? Could steam be used with advantage (especially in steam vessels) in preference to water, and if so, could arrangements be made without very much difficulty or expense for applying it in case of need?

G. The relation of the vessel's registered tonnage and her crew to the quantity of coal shipped in her; and whether this relation varies in consequence of the different character of the coal, or with the different ports from or to which she sails, or with different seasons of the year?

H. Whether unguarded lights, or safety lamps, are generally used on board coal-laden vessels, and if any special rules are adopted?

HENRY JEULA,
20th April 1875. Lloyd's, E.C.

MINUTES OF EVIDENCE

TAKEN BEFORE THE

ROYAL COMMISSIONERS

APPOINTED TO INQUIRE INTO THE

SPONTANEOUS COMBUSTION OF COAL IN SHIPS.

Tuesday, 20th April 1875.

PRESENT:

THE RIGHT HONOURABLE HUGH C. E. CHILDERS, M.P., IN THE CHAIR.

Sir GEORGE ELLIOT, Bart., M.P.
HENRY HUSSEY VIVIAN, Esq., M.P.
FREDERICK AUGUSTUS ABEL, Esq., F.R.S.
RICHARD CORY, Esq.
GEORGE DUNCAN, Esq.
JOHN FENWICK, Esq.
JOHN FERGUSON, Esq.
CHARLES ICELY, Esq.
JOHN PERCY, Esq., M.D., F.R.S.
WILLIAM YOUNG, Esq.
J. M. CARMICHAEL, Esq., Secretary.

Captain HENRY M. HOZIER, Secretary of Lloyd's; HENRY JEULA Esq., Underwriting Member of Lloyd's, examined.

Captain H. M. Hozie and H. Julia Esq.

20 April 1875.

1. (*Chairman to Capt. Hozier.*) You are secretary to Lloyd's?—I am.

2. (*To Mr. Jeula.*) What position do you hold at Lloyd's?—I am an underwriting member of Lloyd's, and was honorary secretary to the Statistical Committee of Lloyd's at its commencement.

3. You have taken a great interest in the statistics of this question before us?—Yes, in this question with others.

4. (*To Capt. Hozier.*) Have you any information to lay before the Commission in connection with the subject of their inquiry?—I put in a list of coal-laden vessels posted at Lloyd's as lost or damaged through fire in 1874,* showing the results to the vessel and cargo. I also put in a list of missing coal-laden vessels (*handing in the same.*)†

5. Can you give us at present any further information about any of those ships?—I would rather do that later. I may mention that in the year 1872 only 14 coal-laden vessels were posted as missing, all being sailing vessels. In 1873 45 were posted as missing, 41 being sailing vessels, and four steam. In 1874 the number was 35, 31 being sailing vessels, and four being steam vessels. In the first quarter of 1875 the number was 10, seven being sailing, and three being steam vessels.

6. Could you give a similar list for any number of years previous to 1872?—We could furnish such a list for any number of years back.

7. The materials are available for carrying it back 10 years?—Yes.

8. The commissioners would be glad to be furnished with a similar list for 10 years back if you could give it them?—I will have such a list prepared.

9. (*Mr. Vivian.*) Could you give the proportion which those ships bear to the total number of ships laden with coal?—I will furnish that.

10. Could you also compare the number of coal-laden ships lost or damaged or missing in the years for which you give the return with the total number of ships that have sailed laden with other goods which have been also reported lost or damaged or missing, because many may have been lost, not by the spontaneous combustion of coals but by ordinary casualties of the sea?—I will furnish such a comparison.

11. One question which would arise would be whether the ships which load with coal are in the main of a lower class than other vessels; whether some portion of the excess of missing coal laden ships, if there is any such excess, is due to the class of ships; will you therefore give the classes of the vessels, and specify whether they were iron or wooden vessels?—I will give those particulars.

12. (*Chairman to Mr. Jeula.*) Have you any suggestions to make to the Commission?—I hand in a paper containing a few suggestions as to some directions in which I think inquiry would be useful (*handing it in*).‡

13. (*To Capt. Hozier.*) Have you anything else to put before the Commission at present?—I hand in a letter from the Coal Inspector of the Peninsular and Oriental Company to the Committee of Lloyd's (*handing in the same*).§

The witnesses withdrew.

NOTE.—The form and scope of the returns alluded to in questions 6 to 11 were subsequently modified, in consideration of the serious expense which their preparation would have incurred, but the returns which were prepared by Lloyds for the Commission will be found at pages 101 and 108.

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* *See* Appendix, page 104.

† *See* Appendix, page 108.

‡ *See* Appendix, page 140.

§ *See* Captain Steinson's letter quoted by Mr. Price, page 177.

T. H. Farrer, Esq.

20 April 1875.

THOMAS HENRY FARRER, Esq., examined.

14. (*Chairman.*) You are permanent secretary to the Board of Trade?—I am.

15. This Commission, I think, originates from the Board of Trade, though it is in form issued by the Home Office?—That is so.

16. Will you state to the Commission the circumstances under which you recommended its issue?—There had been a considerable number of cases in which coal-laden ships were lost by fire, in which the Board of Trade had ordered inquiries under the provisions of the Merchant Shipping Acts, and they took some pains in some of the later inquiries to get scientific evidence, and to go as fully into the question as they could. They found, however, that the tribunal before whom those inquiries were conducted, that is to say a magistrate and two nautical assessors, was scarcely competent to investigate the scientific and complicated questions that arose, and they therefore thought it was useless to order more inquiries which did not produce any great elucidation of the subject. Lloyd's Committee thereupon, I will not say remonstrated, but desired that there should be further inquiry, and we had some correspondence with them upon the subject, and also with the Salvage Association of which Mr. Harper is secretary. The result of that correspondence was that the Board of Trade ascertained that those bodies desired not so much further statutory inquiries under the Merchant Shipping Act, as a careful investigation such as could only be conducted by a body specially appointed for the purpose, and in the end this Commission was appointed.

17. Can you put that correspondence before the Commission?—I will put in the whole of the correspondence.*

18. Can you also furnish us with the minutes of the evidence taken in inquiries in special cases?—I put in the minutes of the whole proceedings containing the evidence on the two inquiries which throw most light upon the subject, viz., the inquiries into the cases of the Herbert Graham and the Minnie Graham, and I put in a list of the cases in which we have had inquiries. I also hand in copies of the abstract of the Wreck Register in which the results of those inquiries are stated generally (*handing them in*). If the Commissioners think it it desirable to have before them the evidence taken on the inquiries in the other cases, I can produce it.

19. (*Dr. Percy.*) I think it would be desirable to have the proceeding on the other cases?—I will send the minutes of the proceedings in the whole of those cases.†

20. (*Chairman.*) Besides holding those inquiries have you had any correspondence with the officers at the ports on the subject?—No, I think we have had no correspondence that throws any light upon this question.

21. So that the inquiries, and the correspondence with Lloyd's, and with the Salvage Association, tell the whole tale as far as the Board of Trade can give it?—They do.

22. You have held 12 inquiries into the causes of the burning of coal-laden vessels since 1868?—Yes.

23. Is there any special reason for selecting the two cases of the "Herbert Graham" and the "Minnie Graham"?—I took out those two because in looking through them it appeared to me that the subject was gone into more fully in those cases than in any others, and some scientific evidence was taken in those cases.

24. Have you anything else to put before the Commission at the present moment?—Only two returns, one of which has been presented to Parliament, and the other is on the point of being presented (*handing them in*).

25. That is the whole of the information which Parliament has about the burning of coal-laden vessels at sea?—Yes. I could of course give from our wreck returns the proportion of losses of coal-laden vessels to the losses of vessels laden with other cargoes. I could get a return of that sort made out; but without a great many more details than we could give, I doubt whether it would be of much value.

26. Lloyd's would probably be able to give such a return as completely as it could be given?—Yes. From some experience in trying to get at results from statistics of that kind I am not very sanguine about getting very important results from them.

27. (*Mr. Vivian.*) The Board of Trade take cognizance only of English ships?—Only of British ships.

28. (*Sir G. Elliot.*) You, generally speaking, have stated that 12 inquiries have taken place into losses by fire of coal-laden vessels; what proportion do you suppose they represent of the total number which have taken fire within the period?—I could tell you what proportion they bore to the total number that we know to have taken fire. I can tell you nothing about missing vessels.

29. Have you any impression what proportion it would be?—Not at this moment.

30. The ships which come under your jurisdiction are called British vessels; what proportion would the casualties we are speaking of in British vessels bear to similar casualties in the whole of the vessels carrying coal sailing from British ports?—I could not tell you that.

The wiitnss withdrew.

Adjourned.

Tuesday, 1st June 1875.

PRESENT:

THE RIGHT HON. HUGH C. E. CHILDERS, M.P., IN THE CHAIR.

SIR GEORGE ELLIOT, Bart, M.P.
HENRY HUSSEY VIVIAN, Esq., M.P.
FREDK. AUGUSTUS ABEL, Esq., F.R.S.
RICHARD CORY, Esq.
GEORGE DUNCAN, Esq.
JOHN FENWICK, Esq.
JOHN FERGUSON, Esq.
CHARLES ICELY, Esq.
JOHN PERCY, Esq., M.D., F.R.S.
WILLIAM YOUNG, Esq.

J. M. CARMICHAEL, Esq., Secretary.

Mr. R. C. Rundell.

1 June 1875.

Mr. R. COOPER RUNDELL examined.

31. (*Chairman.*) Will you tell the Commissioners what position you occupy?—I am assistant to the secretary to the Underwriters' Association of Liverpool.

32. I believe you have given a good deal of attention to the question which is specially referred to this Commission?—I have given it attention for some five or six months past, particularly to a point which is of special interest to underwriters, namely, the increased marine risk; and it was with the view to discover the probable cause of the spontaneous combustion of coal in ships, and to afford some means of estimating the increased risk, that I prepared some statistics which I have recently presented in the form of a report to the Underwriters' Association. The committee have desired me to state that they will be

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* *See* Appendix I., page 1.

† *See* Appendix II., page 4.

Mr. R. C. Rundell.

1 June 1875.

pleased to present the Commissioners with a copy of the report when it is finally printed; it is already in the hands of the printer.

33. That report will be issued to your own body, and published for private circulation?—Yes.

34. I think you have collected some statistics with reference to three-fourths of the year 1873, and you are able to compare those with the corresponding period of the year 1874?—Yes.*

35. What was your particular object in taking three-fourths of the year, and not the whole year?—At the time when I commenced this investigation sufficient time had not elapsed for the completion of the voyages for the year; it was with the view of having all the voyages completed that I took the nine months instead of the 12 months.

36. However, what you have is a complete analysis of the results in coal-laden ships, sailing with above a certain quantity of coal, and crossing the Equator, during similar periods, in the two years?—Yes. I have taken vessels carrying 500 tons of coal and upwards. The reason for selecting the first nine months of each year was, as already stated, to allow time to ascertain the result of each sailing, and the reason of selecting the two elements of carrying over 500 tons and crossing the Equator was because a preliminary investigation showed that the heating and fires chiefly occur in vessels with cargoes of 500 tons and upwards, and that a considerable time usually elapses before the signs of heating are discovered.

37. Will you give us the results of the statistics you have collected with regard to those vessels from this country which have crossed the Equator carrying 500 tons or more of coal during those two periods, namely, the three first quarters of 1873 and the three first quarters of 1874?—An examination of the facts collected show, first, that in the cases selected for comparison no fires occurred before the 37th day after leaving port, while in one case there was no evidence of fire before the 190th day. The greater number of fires have, however, occurred about the 60th day. Secondly, that the fires have not been confined to one kind of coal, but have occurred in most, if not all, of the varieties which are usually exported. In fact, the inquiries which have been made support the opinion that no kind of ordinary coal containing hydrogen has entire immunity from spontaneous combustion. Thirdly, we find that fires occur in the cargoes whether the coal be ventilated or not, whether loaded in summer or winter, by tip or by basket. Among the instances are vessels ventilated by shafts through the coal, both horizontally and vertically, by vertical shafts only, and by means of platforms and side linings which allow a circulation of air under the cargo from one end of the ship to the other. Some of the cargoes also are especially named as having been shipped in fine weather. It is probable, however, that there is a certain amount of moisture always present in all coal.

38. Those are the general conclusions which you have come to; will you give us the statistics upon which those conclusions are based. In the first place, how many vessels as to which you have information sailed with cargoes of above 500 tons on voyages crossing the Equator during the nine months in each year?—In the first period, that is to say, the first nine months of 1873, 1,133 vessels sailed. Out of those there were 23 casualties from spontaneous combustion, or about two per cent. In the second period, that is to say, in 1874, there were 1,240 sailings, out of which there were 50 casualties, or about four per cent.

39. Those are casualties connected with fire?—Yes.

40. Do those casualties include missing vessels?—No. I have a separate list of missing vessels out of those particular sailings. In the nine months of 1873 out of the 1,133 coal-laden vessels which sailed from the United Kingdom (not necessarily British ships) there were five missing vessels, and in 1874 out of the 1,240 there were seven missing vessels.

41. That is to say, in 1873 there were 23 disasters known to have arisen from fire, and five missing ships?—Yes.

42. And in 1874 there were 50 casualties known to have arisen from fire and seven missing ships?—Yes; several of those were ships that probably foundered at sea. I have examined each case on its merits, and in the case of some of them wreckage was picked up, or there was some sign from which it might be presumed that the vessel had not been burnt.

43. You have limited your inquiries to vessels that crossed the Equator?—Yes.

44. (*Mr. Young.*) You say they are "selected" cases; what do you mean by selected cases?—I have selected those ships carrying 500 tons and upwards, and I have also selected those crossing the Equator.

45. Do the 1,133 and the 1,240 comprise every ship carrying above 500 tons of coal that crossed the equator?—Yes, from the United Kingdom.

46. (*Chairman.*) Then you have classified those sailings according to the districts from which the coal was shipped?—Yes. I have adopted a classification of the coal ports already in use, viz., East coast ports for north country and Yorkshire coal, South Wales ports for South Wales coal, Liverpool and Birkenhead for Lancashire and North Wales coals respectively, and Scotch ports (both east and west) for Scotch coal. The facts brought out in this classification and comparison are shewn in the following figures: Out of 268 sailings from the east coast ports during the first period, there were 13 casualties, or 4·85 per cent., while during the second period, out of 312 sailings, there were 15 casualties or 4·81 per cent. This shows that although in each year there is a high percentage of casualties in coal from the east coast ports the percentage has somewhat decreased in the second period, that is as to the east coast. There is one fact that may be interesting with respect to the east coast shipments. It is by the addition of the Yorkshire and London statistics that the east coast returns show a decrease in the percentage of casualties. If these be taken from the returns of the east coast ports, the number of casualties for the first period will be about four per cent., while for the second period it will be about five per cent., thus showing an increase of 25 per cent.

47. On what?—On the casualties in the cases of vessels from the northern ports; that is to say, by taking away the South Yorkshire and London shipments.

48. (*Sir George Elliot.*) You mean the coals shipped at Grimsby have not given such a result as regards spontaneous combustion as the coals shipped at the northern ports?—The Yorkshire ports have shown a decrease, while the northern ports have shown a slight increase in the percentage of cases of heating.

49. (*Dr. Percy.*) Do the figures you are giving comprise cases of heating only, or do they include cases of explosion also?—They apply to cases of spontaneous combustion alone. I have not taken account of any case where I had reason to suppose that it was a case of explosion.

50. (*Chairman.*) Will you now pass to shipments from another district?—I will pass to the South Wales district. Out of 587 sailings from South Wales ports during the first period, there were seven casualties, or about 1·19 per cent.; while during the second period out of 589 sailings, there were 10 casualties or 1·7 per cent. This shows that in each period there has been but a small percentage of casualties in coal from the South Wales ports, but that the percentage has increased nearly 50 per cent. in the second period. Next taking the Liverpool district. From the Mersey there are two very distinct classes of coal shipped, namely, the North Wales coal and the Lancashire coal. Lancashire coal is shipped on the Liverpool side of the Mersey, and the North Wales coal on the Birkenhead side. Taking the shipments at Birkenhead there were 120 sailings from Birkenhead during the first period, and out of those there were no casualties. During the second period out of 133 sailings, there were three

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* Mr. Cooper Rundell's Statistics, revised and extended by him for the information of the Commission, will be found in the Appendix, at page 127.

Mr. R. C. Rundell.

1 June 1875.

casualties or 2·27 per cent. From Liverpool there were 69 sailings during the first period, and out of these there were no casualties, while during the second period, out of 109 sailings, there were 13 casualties or 11·9 per cent. Passing to the Scotch ports (east and west), out of 89 sailings from Scotch ports (east and west), during the first period there were three casualties, or 3·37 per cent.; while, during the second period, out of 97 sailings, there were nine casualties or 9·28 per cent. This shows that while the percentage of casualties in the coal from the Scotch ports is high in each period it has increased in the second period nearly threefold.

51. Can you give the Commission not only the class of coal or the seam from which the most dangerous coal seems to come as shown by the aggregate losses but the percentage on the shipments?—No. Underwriters would be very pleased to be able to get that information; it has been only in a few cases that I have been able to obtain it. From Liverpool there were 13 cases of heating, seven of which occurred in coals supplied from one firm and four of which occurred in coals supplied by another firm. A glance at the names of the different kinds of coal which have heated will show that while they are generally subject to this danger of spontaneous combustion, there are two or three which are conspicuous from the frequency with which they recur. It must not, however, be hastily concluded that the latter are of a more dangerous kind as the shipments may also be more numerous. I may mention that, although some Lancashire coal has been burning, there are some very distinct varieties of Lancashire coal. There is the cannel coal, which is considered a very safe coal.

52. (*Mr. Vivian.*) I presume there have been no cases of spontaneous combustion in the case of cannel coal?—I have not heard of any at all, it is looked upon by the Liverpool underwriters as a very safe class of coal. They have also in esteem several other kinds of coal that go from Liverpool which I could mention. The best Orrell coal bears a high character.

53. You cannot classify the Lancashire coals in any way so as to distinguish between those that are combustible spontaneously and those that are not?—No, but certain kinds, such as Crow Orchard coal, are thought to be more liable to spontaneous combustion, and Liverpool underwriters who discriminate between the different kinds of coal do not care to insure them.

54. You cannot distinguish as between dry coal and bituminous coal, that is to say, coking coal and non-coking coal?—No, I do not find that Liverpool underwriters go into that question, though bituminous coals are avoided by some.

55. That is to say, coking coals?—Those that cake. In west coast voyages there is the largest increase in the number of casualties in the second period as compared with those in the first period and this increase proceeds chiefly from coal shipped at Liverpool and from South Wales ports. In the Liverpool sailings to the west coast the greater number of cases of heating were in cargoes of Lancashire coal, while in the South Wales sailings, but with one exception, all were in cargoes of bituminous coal shipped, I believe, for smelting purposes.

56. (*Chairman.*) You have given us an analysis of casualties with reference to the districts from which the coal was shipped, can you tell us out of so many ships carrying north country coal, for instance, what percentage of casualties there would be, and out of so many ships carrying Welsh coal what percentage of casualties there would be?—Yes. Taking the different districts, the east coast ports, for instance, I could tell you, out of the number of casualities, what coal in particular cases was shipped by one shipper and what by another shipper. I could give the names of the vessels.

57. Could you analyse the return and give those statistics?—Yes; I will furnish that to the Commission, but I could not give the percentage of casualities on the shipments of any class of coal. I can only say in how many cases heating occurred with Scotch coal or Lancashire coal, and so on.

58. With reference to the class of shipping in which these casualties occur, I think you have some information to give the Commission?—Yes. In the first period in which the casualties which I have mentioned occurred, the average tonnage of the vessels was less than in the second period. Only one third of the vessels in the first period are over 1,000 tons, while in the second more than half of them are above that tonnage. In the second period there are eight vessels above 1,500 tons, and only two in the first period. To estimate how far the greater size of the ships explains the larger number of cases in the second period it would be necessary to ascertain if the average tonnage of the vessels which sailed during the second period is much greater than in the first period. Those particulars I have not had an opportunity of getting for the whole of the districts, but I have the particulars for Liverpool.

59. Have you distinguished between sailing ships and steamers?—I have taken steamers when they have taken a cargo of coals, but not when they have taken coals for merely bunker use.

60. Have you distinguished in the 1,133 and the 1,240 how many were steamers and how many were sailing ships?—No, but I can do so hereafter, so far as Liverpool is concerned. Nearly the whole of those vessels whose cargoes heated were first-class vessels, vessels holding a high class in the register. I may mention that from Liverpool for the eight months to the 31st of August 1874 I find that the number of sailings of iron vessels to wooden ones bore the proportion of about one to three; of the whole number of sailings about a fourth were iron vessels, but taking voyages to the west coast of America the iron vessels were about 75 per cent. of the whole number of sailings. A much greater proportion of iron vessels seem to have gone to the west coast of America. Among the vessels before referred to whose cargoes heated, the proportion of iron to wood is greater in the second period than it is in the first. Seven out of the 24 iron vessels were built during the second period. In connexion with the character of the ships to which casualty occurred, the depth of hold is a point worth noting as influencing the breakage of the coal. The depth of hold in the second period averages about 16 inches more than in the first period. But the mere depth of hold without taking into account the number of laid decks will only imperfectly indicate the mass of the cargo. The "Glory of the Seas" has by far the greatest depth of hold but she has three laid decks, so that the coal could be turned over when necessary. The captain also reports that his being able to turn over the coal and expose it to the cooling effect of the air, enabled him to bring his ship to San Francisco.

61. With regard to the number of days that elapsed in the cases of the casualties to which you have referred between the period of the sailing of the ship and the first indication of heating, have you any information to give us?—In a table which I have here, the 73 cases to which I have already referred are arranged to show the number of days that elapsed between the sailing of the ship and the first indication of heating. The earliest case occurred when the vessel was between 10 and 20 days out, in the latest the fire occurred nearly 200 days after sailing, and when the vessel had been in port about nine days. Indeed, the number of cases which occur after the arrival of the vessel in port shows that extra care should be taken at this time. It has been suggested that the heating is caused by the access of air when the hatches are taken off, and the discharge commenced, but in the case of the "Merom" the fire was in the fore compartment where the discharge had not begun. In another case, the "Respigadera," I am informed that until her arrival in San Francisco there was no sign of heat in the coal.

62. Can you give us the number of cases in which casualties occurred after leaving port within the first month, and the number in which they occurred in

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the second month, and so on?—Yes. In 1873, I have found no cases until after the fortieth day, between 40 and 50 days out, there was one case; between 50 and 60 days out, two cases; between 60 and 70 days out, three cases; between 70 and 80 days out, three cases; between 80 and 90 days out, three cases; between 90 and 100 days out, one case; between 100 and 110 days out, four cases; between 110 and 120 days out, two cases; between 120 and 130 days out, one case; between 130 and 140, one case; none between 140 and 190 days out; one case between 190 and 200 days, making 22 cases; and then there is the case of the Arracan, where I have not been able to get the time.

63. That exhausts the 23 cases of the first period?—Yes.

64. Can you give the corresponding particulars for 1874?—The first case occurred between 10 and 20 days out; the next case between 30 and 40 days out; the next nine cases between 50 and 60 days; the next six cases between 60 and 70 days; the next two cases between the 70th and 80th day; the next four cases between the 80th and 90th day; the next six between the 90th and 100th days; the next six between the 100th and 110th day; then there were two cases between the 110th and 120th day; one case between the 120th and 130th day; three cases between the 130th and 140th day; one case between the 140th and 150th day; and four cases between the 150th and 160th day, making 46 cases. Then there were five cases in which I have not been able to obtain the exact period. The coal carried in those cases being: Tyne coal by the "Unione," Bower's West Hartley by the "Maassluis," Powell Duffryn steam coal by the "Lady Louisa," and Brancker's Orrell coal by the "Victorine." I may remark that this table affords no indication that spontaneous combustion is more quickly developed in coal from one district than in that from another, and with different kinds of coal, the greatest number of heatings occur about the 60th day after leaving port.

65. Have you any statistics bearing on the question of temperature?—The tables I have here show very decidedly that the summer sailings are the most dangerous, that is to say, those for the months of June, July, and August. In 1873 the largest percentage of cases occurs in the August sailings, in 1874 it occurs in those for July. If the different districts be compared in this way, the following is the result: from the east coast district in 1873 the greatest number of casualties occurred in the sailings in June, and in 1874 in July. From the South Wales district the greatest number of casualties in 1873 occurred in June, and in 1874 in July. In the Liverpool district in 1873, there were no fires, in 1874 the greatest number of casualties occurred in June. In the Scotch district in 1873 the greatest number were in August, and in 1874, July.

66. These are the dates of sailing?—Yes.

67. Have you any percentages as to the dates of the casualties?—The greatest number of casualties have occurred in September and October.

68. Must not your statement be qualified by this, that you do not give any statistics as to the ships which sailed in the last quarter of the year?—Yes.

69. So that the comparison is only between the sailings of the first nine months of the year?—Yes. My statistics refer to these periods, but I have a table which shows graphically the period of the year over which a much greater number of voyages in which casualty has occurred have extended, and that table shows that these voyages have occurred during the hot period of the year. That table includes 128 cases. It embraces a period from 1868 to 1874, and about a half of the whole number of cases occurred in 1874. I will furnish the Commissioners with that table. There are two points worth noting while considering the effect of temperature, that is when and where the first signs of heating were discovered. In selecting those voyages only which extend beyond the Equator there is necessarily a high temperature for a large portion of each voyage. It will be found in each year that the greatest number of heatings occurs in September and October. As regards geographical position in the first period, out of 23 cases 11 occurred in the Atlantic, and 12 to the east of the Cape of Good Hope. In the second period out of 50 cases, 30 occurred in the Atlantic, 14 to the east of the Cape of Good Hope, and six on the West Coast of America. If we compare these with the total number of selected vessels which sailed into the three geographical divisions thus indicated, to the South Atlantic division must be added the sailings east of the Cape of Good Hope and west of Cape Horn (excluding the Indian sailings by way of the canal). These selected sailings into the South Atlantic are about 1,129 for the first and 1,237 for the second period. Comparing with these numbers the respective Atlantic casualties, the percentages are for 1873, 0·97 per cent.; for 1874 2·43 per cent., an increase of more than 150 per cent. If the sailings for the Indian Ocean are compared in the same way the resulting percentages are 2·41 and 2·56 respectively, or an increase of only about six per cent. for the second period. For the West Coast, in the first period, there were no casualties out of 235 sailings, while, in the second period, out of 283 sailings there were six, or 2·11 per cent. This shows the relative increase for each of the three divisions.

70. That is with reference to the voyage?—Not as regards whether it was a West Coast voyage or an East India voyage, but showing the division where the casualties have occurred, and the percentage is on the number of vessels which sailed from the United Kingdom *into* that division.

71. Have you also statistics with reference to the voyage?—Yes; by comparing for each period the selected sailings for the three divisions with the casualties respectively belonging to these sailings, we obtain the following results:—For South Atlantic voyages in the first period there are four heatings in 399 voyages, or about one per cent.; in the second period there are three heatings in 410 voyages, or about three-quarters per cent., or a slight decrease in the danger on these voyages. For Indian voyages in the first period there are 14 heatings in 499 voyages, or 2·81 per cent.; in the second period there are 23 heatings on 546 voyages, or 4·21 per cent., an increase of about 50 per cent. For West Coast voyages in the first period there are five heatings in 235 voyages, or 2·13 per cent.; in the second period there are 24 heatings on 284 voyages or 8·44 per cent., an increase of nearly 300 per cent. This shows a remarkably large increase in the number of cases of spontaneous combustion in coal shipped to the West Coast ports.

72. Have you any explanation to give of that?—I have separated them into districts to show where the coal came from, and the details are as follows: From the east coast ports in the first period there was one casualty out of 21 sailings, or about five per cent.; in the second period there were three out of 62 sailings, or about five per cent., the same as before. From the South Wales ports in the first period there were four casualties out of 122 sailings, or 3·28 per cent.; in the second period there were seven out of 101 sailings, or an increase of more than 100 per cent. From the Mersey in the first period there were no casualties out of about 71 sailings; in the second period there were 13 out of 99 sailings, or over 13 per cent. In the second period there were 12 cases out of about 82 sailings from the Liverpool side of the Mersey, while there was only one out of 17 sailings from the Birkenhead side. From Scotch ports there were no casualties out of 21 sailings in the first period, and in the second period there was one out of 22 sailings.

73. These all refer to voyages to the West Coast?—These all refer to voyages to the West Coast. The great increase, therefore, in the percentage of casualties in West Coast voyages occurs in coal shipped at Liverpool and the South Wales ports. I may repeat what I said before that 12 out of the 13 cases of heating in these sailings from the Mersey were in cargoes of

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Lancashire coal, while those in South Wales cargoes were generally in bituminous coal.

74. Having given us those particulars as to the voyage, as to the class of coal, as to the place of shipment, and as to the month of the year in which the sailings and the casualties have taken place, have you any statistics to give as to the mode of shipment?—In Liverpool coals are shipped both by hand and by tip. On the Birkenhead side the shipments are by shoots, and on the Liverpool side by a crane that carries the wagon over the vessel; the truck is lowered and then tipped, the coal falling down the whole depth of the hold. In some cases the crane also takes boxes from a truck. There are three boxes generally on a truck, and those boxes are lowered by means of the crane into the hold so that the coal need not get so broken when it is unloaded from the box as when it is tipped from the wagon. The hand loading is by basket and by barrow.

75. Have you any statistics as to the apparent effect of different kinds of loading?—No, but comparing the loading by crane and the loading by hand I have the following: Of the vessels which sailed from the Mersey in the nine months, January to September 1873, 65 out of 189 were loaded by tip, or 34 per cent. of the whole. In the same period for 1874 88 out of 242, or about 36 per cent., showing a very slight increase in the percentage of shipments by tip during the second period.

76. How does that bear upon the result as to casualties?—I have given here the total number shipped by tip, the shipment by tip on the Liverpool side, and the shipments by tip on the Birkenhead side; if we separate the shipments by tip on the Liverpool side from the shipments by tip on the Birkenhead side, the Liverpool side shows an increase in the number of shipments by tip in 1874 of over 75 per cent. which is a significant fact when we remember that the great increase of fires has been in the Lancashire coal shipped on the Liverpool side. The conclusion, therefore, that I would draw is that the shipment of coal by tip breaks the coal very much and increases the danger.

77. (*Mr. Vivian.*) As to the Birkenhead shipments can you tell us what description of coal it was, whether it was bituminous coal or not?—The fires have occurred in North Wales steam coal in each case. There were only three cases; the first was a North Wales steam coal, the particular colliery not named; then there was main coal from the Tryddyn Colliery, Coed-Tallon, and a mixed cargo of Westminster, Brymbo, and Oak pits; that was in the 1874 period. There were no cases of heating in cargoes of coal shipped from Liverpool or Birkenhead in the 1873 period.

78. Can you give us the description of coal shipped from the South Wales ports?—There were ten cases of heating in the 1874 period, one in each of the following: Powel Duffryn steam coal, Insoles' No. 3 Rhondda, Insoles' Merthyr steam coal, Hood's Merthyr steam coal, and six in No. 3 Rhondda coal, which I believe is a smelting coal.

79. Were those to the West Coast of America?—These shipments from the South Wales ports were not entirely to the West Coast; there were three to the East, one to Shanghai, and two to Singapore. In the 1873 period there were seven cases of heating from the South Wales ports as follows: two in Rhondda bituminous coal, one in steam coal (colliery not named), two in Broad Oak bituminous coal, one in Through-and-through bituminous coal, and one in Aber smelting coal.

80. Where was that shipped?—At Porthcawl.

81. Where was the Through-and-through coal shipped?—From Newport.

82. Does that exhaust the whole of the South Wales coal?—Yes, I have given you the whole of the names.

83. Can you give us the same particulars with regard to the Scotch ports?—Beginning with the 1873 period when three cases of casualities occurred, the descriptions of coal were as follows: Wishaw Ell coal supposed to be from the Larkhall district, Townhill steam coal from Fife, and Ell coal from the Wishaw district. In 1874 there were nine cases, viz.: Steam coal, Ell coal, splint coal, collieries not known, Russell's main coal. Then there was a cargo containing four different kinds of coal, all splint coal from these collieries, Woodside field, Hell field, Lougler field, and Skellyton field. Then Watson's Hartley, Auldton Ell coal, Auldton steam coal, and Splint steam coal, colliery not known.

84. Those were shipped from what port?—Some from the Clyde and some from Dundee; the Auldton Ell coal and the Auldton steam coal were shipped from Dundee, and one splint steam coal from Leith, all the other coals being shipped from the Clyde. That was in 1874, and in 1873 the two shipments of Wishaw Ell coal and the Townhill steam coal were shipped from Dundee.

85. Can you give us the same particulars with regard to ports on the east coast of England?—In 1873 out of 13 cases, there was a shipment of each of the following: Harton Wallsend house coal, Davison's West Hartley steam coal, Howard's West Hartley steam coal, Cowpen Hartley steam coal, Featherstone's main second-class large steam coal, West Yorkshire, Featherstone's West Riding steam coal, Yorkshire Hartley steam coal, and three shipments of which I do not know the particulars. Then shipped from London there was Newcastle West Hartley coal in two cases. In the year 1874 there were fifteen cases, viz.: Of Cowpen Hartley, three, of Howard's West Hartley, one cargo of a mixture of both of these, one of Bower's West Hartley, one of Carr's Hartley, one of Ravensworth West Hartley, another of coal described as West Hartley main steam, one of coal Bedside West Hartley, one of Davison's West Hartley steam coal, and one of Hastings Hartley coal; one cargo I have been unable to trace, and this makes the whole for 1874.

86. (*Mr. Cory.*) You have given the number of casualties and the different kinds of coal, can you give also the number of shipments of different kinds of coal?—No. I have only had the number of shipments in 1873 and 1874 from two collieries sent to me.

87. The number of casualties does not very clearly show the comparative liability of coal to spontaneous combustion?—No, unless you know the number of shipments of a particular kind of coal you can scarcely form an idea whether it was a dangerous coal, it might be that that coal was more generally shipped.

88. (*Mr. Fenwick.*) Do I rightly understand you to say that more accidents occurred in iron vessels laden with coals than in wooden vessels?—In the second period there was a greater proportion of iron vessels in the casualties, but it is difficult to say whether it was on account of there being more iron vessels, there was a greater proportion of casualties in the second period. It is noteworthy that out of the casualties in 1874 a large proportion occurred in new ships.

89. Do you give any particulars as regards coal shipped in wooden vessels as compared with coal shipped in iron vessels?—Only with respect to those vessels to which casualty has occurred. In a tabular form I have the particulars of each vessel as well as the kind of coal, on one page the particulars of each vessel and her cargo, giving the names of the vessels, the nationality, the year when built, the class, whether iron or wood, net registered tonnage, depth of hold, number of decks and if laid, tons of coal, description of coal, name of fitters or shippers, and kind of ventilation. Then on the other side are particulars of the voyage and the casualties, giving the date of sailing, the destination, date of the casualty, or when signs of heating were first discovered, the number of days the vessels were out, where the casualty occurred, the nature of the casualty, and some few remarks.

90. (*Mr. Icely.*) Have you any accurate information with regard to the mode of ventilation adopted in the cases of those vessels in which casualties occurred?—Yes, for a number of cases.

91. Have you had any sketches made of the ventilation adopted?—In some cases I have had sketches

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made, and I should be pleased to put them in. It is a very difficult matter to say whether a vessel is sufficiently ventilated or not. She may have left the United Kingdom with good arrangements for ventilation, or been prepared for a certain amount of ventilation, and that ventilation may have been crushed in by carelessness in loading. The underwriters surveyors at Liverpool have recommended a certain system of ventilation, and they would be able to give the Commission very detailed information as to the mode they recommend. These surveyors go round the docks and give information amongst other things to the underwriters respecting the mode of ventilation adopted.

92. (*Mr. Young.*) You said if I remember rightly that of the whole number of sailings in the two years one third of the vessels were of iron and two-thirds of wood?—I spoke merely of sailings from Liverpool for the second period when I said that the number of iron vessels which sailed was a third of the number of wooden.

93. You did not tell us how many iron vessels were burnt or encountered disaster?—Eight on the first period and sixteen in the second. Four out of the sixteen Liverpool casualties were in iron vessels.

94. You said that the sailings in June, July, and August were the most dangerous?—We have had the greatest number of casualties in the sailings for those months.

95. You have not gone into a comparison of the number of sailings in those months with the number of casualties?—Yes, I have the date but have not given the percentages.

96. You have taken into account that there are a greater number of vessels actually sailing in those months as well as a greater number of casualties?—I have a table showing for each month of the two periods the number of sailings, and the number of casualties which have been reported out of those sailings.

97. From that you would come to the conclusion that June, July, and August are the most dangerous months?—Yes, the greatest per centage of casualties have occurred in the sailings for those months.

98. I understand that that conclusion arises from a comparison of the whole number of vessels that have sailed, with the whole number of casualties?—Yes, after that I have been able to ascertain.

99. (*Chairman.*) Crossing the Equator?—Yes.

100. (*Mr. Young.*) Throughout your statistics, do I understand that you have as far as possible got the particulars of every vessel that has sailed as well as of every vessel that has had a casualty?—Yes, as far as possible; the per-centage of casualties on sailings of course is the information that is desired. The sailings from most of the coal ports have been obtained from Brown's monthly export lists. In these lists, when more than one vessel has sailed during the month from either port for the same voyage, only the aggregate of their cargoes is given and not the number of tons of cargo belonging to each vessel. In the tables, each vessel is included if the aggregate tonnage of the cargo gives an average of more than 500 tons, and unless the average amounts to 500 tons the vessels are excluded. The tables are thus subject to any slight error which this may occasion.

101. (*Mr. Abel.*) Do your statistics include as casualties only those cases in which important damages have been recorded?—Any case of heating that I have heard of I have included as a casualty.

102. Any case of heating that has come to your notice as having given rise to damage?—There has been scarcely any case of heating that I have had an opportunity of examining, but that there has been some damage more or less; in fact some pecuniary less

103. May we look upon these cases of heating, which are classed as casualties, as giving something like a complete return of the cases of heating and fire that have occurred?—As far as I have been able to ascertain, I think all the published cases, and, I do not know what proportion, but I should expect a large proportion, of those cases that have not been published, perhaps more particularly as far as Liverpool is concerned.

104. (*Chairman.*) Would the cases which you have included in your figures be those in which a claim has been made upon the underwriters?—No, I have not been governed by that; some are cases in which a great amount of heating in the coal has been observed, and where I am unaware of any claim having been made.

105. (*Mr. Abel.*) You have included casualties where there was heating and fire and you have included also cases of heating without fire which have given rise to damage?—Yes, cases where jettison has resulted, where a man has been so alarmed about the heating that he has jettisoned the cargo. I have the details of each case of damage, and extracts from the masters' protests in a great number of cases giving particulars as to what efforts were used by the captain to extinguish the fire or to prevent the heating of the coal, showing that some captains showed great promptness and energy in preventing the extension of the evil.

106. I suppose you have not any data in your statistics which give the relation between the cases of casualties and the price of coal at the time of shipment?—No; where there is a readier market for coal you find that there is then an incentive to send coal for shipment that would not otherwise be sent. I have here some remarks from our agent at Marseilles that rather bears upon that point. In the latter part of the year 1874 there were a great number of casualties in the shipment of lignite from Marseilles for the Mediterranean ports and the agent's remarks in connexion with this was, "That increased demand has "has induced sellers to ship coals immediately after "their arrival from the mines, that is, with a certain "amount of dampness," showing that if they had not had this increased demand the coal would have probably been left at the coal pits some time before shipment. Inquiry was made of our agent with a view to discover if there were any transhipments of English coal from Marseilles, whether these fires had been in English coal. As far as he could ascertain they have been entirely confined to coal from the south of France.

107. I gather that you establish a decided relation between the breaking up of the coal and the frequency of the casualties in the coal shipped?—The breakage increases greatly the surface; for instance, if coal is tipped and much broken, the surface is greatly increased and there is a greater amount of decomposition going on in a limited space.

108. As a fact your statistics show that where the coal was either shipped by tipping or was "through and through" coal, coal containing a considerable proportion of slack, the number of cases of heating was increased?—I think that would be the deduction to be drawn from the statistics I have here, that the smaller the coal the more frequent the casualties.

109. (*Mr. Ferguson.*) As regards the 1,240 sailings in 1874 and the 1,133 sailings in 1873, have you taken any notice of other casualties that happened to those ships other than spontaneous combustion of coal?—No, but there have been cases of stranding.

110. You do not take those casualties into account in your statistics?—No, I have only taken missing vessels; I have, however, noted that in coal laden vessels on these voyages casualties, otherwise than from heating, are rare.

111. Of several vessels missing during that time it might be fair to conclude that some of those vessels might be lost from other causes than spontaneous combustion of coal?—Yes, in the cases I have here of missing ships I have endeavoured to form some conclusion as to whether they were lost by perils of the sea apart from fire, or from fire.

112. (*Mr. Duncan.*) Have you taken any notice whether the coals were single or double screened?—When I have had any information given me as to that point, I have taken note of it, but I have not asked

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for it specially; if I have had it, it has been given me gratuitously without its being asked for.

113. There is nothing in your statistics which would show whether or not coals double-screened would be less likely to take fire than coals single-screened?—No.

114. You would consider that when coals were carried by the crane over the hatchway and allowed to drop into the hold a greater quantity of small coal would be put on board than if the coal were put on board by barrows?—Yes, the coal would be liable to break more. Some coals are very friable. In "Stevens on Stowage" it is stated that in South Wales it is the practice to dig out a quantity of small coal from the hatch after the vessel has completed her loading, and to replace it by larger coal, showing that there is a feeling that danger may arise from too large a surface of small coal.

115. If they handle coals as I have seen them handle them in Liverpool in the case of the Peninsular and Oriental ships, where the coals are put into barrows, which barrows are lowered into the hold and there emptied, the coal is prevented from becoming broken?—This Company is very particular I believe about the shipping of its coal, and a special agent is appointed to inspect the shipping of the coal, not so much with a view to prevent fire as to prevent the coal becoming broken.

116. Have you been able to make any distinction between ships loaded in that way and ships loaded in the ordinary way by dropping the coals in from the tip?—Yes, and I have a return given me by Mr. Hughes of Liverpool, showing the number of vessels in which he has shipped North Wales coal by hand, showing that they have all arrived safely.

117. Your papers when put before the Commission would give information that would indicate that where coal is shipped in this way by barrow, the coal is not broken so much, the effect of that being to lessen the danger?—Yes.

118. I suppose you are aware that there is a considerable difference between the construction of the new ships and those constructed formerly, that difference having a tendency to affect their ventilation, that is to say, that the stringer plates are brought home to the shell of the ship in place of being attached to the frames as before. Whereas formerly there used to be an opening, now the stringer plate is secured by the angle iron to the shell of the ship and therefore to that extent the amount of space for ventilation is diminished?—The two periods that I have selected being only 1873 and 1874, I do not know that there would be that difference, between 1873 and 1874 in the style of building vessels.

119. You stated that there were a great number of new ships in 1874; the mode of construction to which I have referred of bringing home the stringer plates to the shell of the ship is a mode of construction that has been introduced within the last few years?—Anything that would tend to shut up the coal would naturally conduce to the danger of combustion.

120. That is one of the things that might have some effect in causing coal carried in new iron vessels to burn more readily than in the case of old vessels?—Probably. There is another point with regard to new vessels. It has been stated to me that when a shipowner has a new ship and he wants to load her with coal he asks the builder on what day she will be ready—the builder is always ready to name a day, and replies, "You may depend on having the vessel ready by such a day;" the owner says, "Well if so I will order the coals round." The shipbuilder declares that the vessel will be quite ready by that time and the owner orders the coals. Instead of being ready by that time the vessel may be days and weeks before she is quite ready, and the coal is all the while lying in the trucks exposed to bad weather, and I heard of one case in which coal brought down in that way and kept waiting for the vessel was exposed to bad weather so that the small coal became quite "pasty;" that was the term used.

121. (*Mr. Vivian.*) Did that coal fire?—It went merely from one of the east coast ports to London. I made inquiry about it, but could not find that it heated; it was a short voyage.

122. (*Mr. Duncan.*) You have stated that the coals appear to fire most readily that are shipped in the driest months of the year?—Dampness being one of the elements in spontaneous combustion I made inquiries to see how far the period in 1874 may have been damper than the period in 1873, and I found this to be the result. Taking in each year July, August, and September, when most of the coal cargoes affected by fire were shipped, there appears to have been no appreciable difference in the amount of rainfall in North and South Wales during these two periods, while in Lancashire, and Yorkshire, Northumberland, and Scotland those months were decidedly drier in 1874 than in 1873.

123. So that according to that it would seem that exposing them in the waggons does not do them any harm?—Not for these periods, so far as rainfall is concerned, but the coal may have been worked damper in 1874 That is another point; the surface seams in Lancashire (in this I am only speaking from hearsay) are damper than the deeper seams, but I have not heard that they were more worked in 1874 than in 1873. With regard to the coal from the seams in Lancashire I heard that some old seams that had been out of use for some time were reworked in 1874.

124. And those workings were all the time standing full of water?—I have no information upon that point.

125. You would not infer that there would be more coals brought in June, July, and August than in other months of the year?—No, not as a rule. In 1873 more vessels sailed in the previous three months.

126. (*Chairman.*) May the Commission take it as the upshot of your evidence that the casualties from spontaneous combustion of coal afloat are on the increase?—There is a great increase in 1874. As regards a comparison between 1873 and 1872, I could not give the result, as to the per-centage of casualties on voyages, but only approximately from a table I have here as to the number of casualties; I think I mentioned before that the number of casualties from 1868 up to the end of 1873 were 65, while the number in 1874 alone there were 64 cases, showing that for seven years we have the same number as in 1874.

127. From the knowledge which you have derived from your connexion with the Underwriters' Association of Liverpool, would you say generally that casualties were on the increase?—No, I should say not; there have been scares; as far back as 1867 there was a great deal of talk about spontaneous combustion of coal, and underwriters gave some particular attention to it, but since that I think premiums, so far as I have been able to judge (I have only limited experience in that matter), have not increased till last year.

128. During a series of years, up till last year, premiums were pretty steady?—As far as I know.

129. In this year they have risen?—They have risen from 25 to 75 per cent.

130. So far as the figures you have produced may be relied on, they have risen with justice; they are in consonance with the actual results?—Yes, but when the underwriters hear of a great number of fires occurring they are unable to tell, without going into a calculation of this kind, what the per-centage would be on the sailings, so as to estimate the risk, and therefore each one is left to himself to form an idea what would be a safe premium.

131. The per-centage on the sailings of 1874 is very much higher than the per-centage on the sailings of 1873?—100 per cent.

132. That is understood and acted on by the underwriters?—Yes.

133. They have not yet got before them those details which you have been working out, still the general results are tolerably well known?—I do not know what effect those statistics may have in altering

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the rates of premium. I do not know that they will have any effect, but each underwriter has been forming his opinion during this period, and I understand that generally the rates have somewhat declined of late; they consider that coals are safer now than they were in 1874.

134. Looking at the fact that for a good many years before 1873, so far as you can judge, there was no seriously increased per-centage of casualty, and that that per-centage seems to have suddenly increased in 1873 and 1874 and is now going down again, does not it look as if the proportion of casualties had a good deal to do with the increased demand and to the consequent want of care in the selection of coal?—Yes, I think that is a very natural deduction to make.

135. So far as you can judge, the casualties are not so much the result of carelessness on board, or in the appliances of the ship, as of want care in the selection of the coal and an absence of proper precautions in the shipment of the coal?—The information I have obtained appears to lead step by step to the conclusion that the increased number of casualties from spontaneous combustion proceeds from the condition in which the coal is shipped, and the coals shipped in 1874 were most probably not so free from pyrites and small coal as in the case of the coals shipped in the preceding years.

136. The very large increase in the demand for coal, compared with the supply in 1874, would account for coal being shipped that would be liable to take fire?—The coal agents found a market for the inferior coal, they were able to sell and get rid of.

137. Have you considered at all whether there is any connection between the increased amount of casualty, and the increased want of control over the men who are concerned in handling the coal?—No, I have only gone into the labour question as far as this, that for the last two years there has been a great increase in the cost of labour, and consequently there has been an inducement to curtail, as far as possible, all labour beyond that required to get and deliver the coal. The Coal Mines Regulation Act which came into force on January 1st, 1873, has also made one source of cheap labour less available. By this Act children are not allowed to work at the mine, except under certain restrictions, which have had the effect of gradually removing a great many of those who were formerly employed in cleaning and selecting the coal, and the remainder of the children, I am told, have thus been able to secure an increase in their wages of nearly 100 per cent. And then with regard to demand, there has also been a great demand for coal and a ready sale has been found for inferior and unclean sorts which would not formerly have been marketable.

138. That again is an element on shore and not afloat?—Yes.

139. Have you considered whether this also may not be an element, the diminished control over the men in the collieries, the men sending up coal which they ought not to send up and ought to throw aside?—I have been told this, that formerly men were fined for sending up bad coal, but that they are not fined for doing so now. The pitmen, it is stated, would have been fined for sending up so much bad stuff, and the stony and metallic part of the coal which did come up would be carefully separated at the pit's mouth. Now, the coal agent, in order to meet the demand, appears to accept anything passed to him.

140. So, summing it up, you, whose special function it is more to observe what happens afloat than what happens ashore, and who would be the first to mark if there was anything serious going wrong afloat, do not attribute either to the construction of the ships or to the management of the coal ships, or to the selection of the coal ships, the increased proportion of casualties, but you rather attribute it to what occurs before the coal is shipped?—Yes, greatly. There may be some other causes that help to make the increase larger, but I think the great cause for the increase arises from the condition of the coal before it goes on board.

141. A question was put to you as to the construction of coal ships, and you said on one point the more modern construction may lead to greater danger than before, by its interference with the ventilation?—I was not aware of the fact of my own knowledge, till Mr. Duncan mentioned it, but I think that would bear on the ventilation of the cargo.

142. Do you think that the class of ship that is being employed for carrying coal on these voyages is a worse class or a better class than it used to be?—I have not very great experience in the matter, but I should fancy that other freights being lower a better class of vessel is employed in the shipment of coal on long voyages than formerly.

143. The popular impression is that any ship would do for coal, but so far as those voyages you have been analysing are concerned you do not think that impression is borne out by the facts in recent years?—No, the casualties have been in high class ships. I am told as regards a number of American vessels that go from Liverpool which used to carry coal, the owners in America are sending them now with salt in preference to coal on account of the increase in the premium.

144. With regard to the owners of the ships which are used in the coal trade for those longer voyages, speaking generally you see no foundation for any imputation that they are less careful than they used to be in the selection and in the handling of those ships?—I have no reason at all to suppose so from what I know.

145. You attribute the increased proportion of casualties to what occurs on shore not to what occurs afloat?—Yes.

146. You have analysed the voyages of the larger ships on the longer voyages crossing the Equator, have you any general facts as to shorter voyages?—I prepared some preliminary statistics taking in North Atlantic voyages as well as South Atlantic voyages, and I limited it at first to all vessels taking above 100 tons of coal. I then prepared another series of statistics giving vessels which had taken 250 tons of coal and upwards.

147. Could you give us the general impression in your own mind of the result?—My impression from those statistics was that a greater number of casualties occurred in voyages across the Equator, and for that reason in subsequent investigations I limited myself to those longer voyages.

148. Is the general impression on your mind that casualties are on the increase on the shorter voyages?—I am not able without reference to say, but my opinion is from recollection that they would show about the same great increase in 1874 over 1873.

149. You think that the longer voyages are a good specimen of the whole trade?—Yes.

150. What would you call the average voyage of a sailing ship to New York for instance?—About 30 days.

151. And to the West Indies?—To the West Indies, say St. Thomas you might put on five or six days; where they are going south they get finer weather sometimes, the length of the voyage also depends on the class of ship.

152. They come within the period in which a considerable number of casualties occur according to your statistics?—Yes.

153. So far as you have carried your inquiry you have not seen any reason to discriminate between the longer voyages and the shorter voyages?—Time is an element, but I think the same thing which would operate to give the increase on the longer voyages would operate to give the increase on the shorter voyages, that is the condition of the coal.

154. Have the premiums risen in equal proportion?—Yes, irrespective of the voyage, that is, taking it apart from its character as a marine risk.

155. The premiums have risen in the same pro-

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portion for the short voyages as for the long voyages?—Yes.

156. As to the mere coasting trade, do many casualties from fire, as far as you know, occur in the colliery coasting trade?—I am not prepared to give any information upon that.

157. (*Mr. Vivian.*) You were asked as to the shipping of coal in barrows, is not it a much longer process to ship coal in barrows than in the ordinary way by tips?—It takes considerably longer.

158. Is not it the case that vessels have frequently only a certain number of days allowed them to load?—Yes, a vessel going under the tips at Liverpool, that is to say, at the cranes, can load, I am told, as much as 800 tons a day.

159. How many tons a day do you think a vessel could load with barrows?—A vessel of 1,200 tons would take I believe 10 or 12 days.

160. What number of days is a vessel usually allowed for loading?—I have no information upon that point.

161. It would take, perhaps, very nearly five or six times as long to load by barrows?—I should think that would be about the time.

162. She would be occupying dock space all that time and incurring other expenses?—Yes. With respect to shipments in South Wales, I was told the other day by a shipper who had had some experience in shipping coals there, that the shipments were sometimes very hurried, and occasionally it was to the advantage of the coal agent that the shipments should be hurried. For a consideration a coal shipper will undertake to load night and day, so that a ship may go off on a tide. I was told that it would be from 50*l.* to 100*l.* more in the coal shipper's pocket by shipping the coal off quickly, and I think Mr. Duncan knows a case where 300*l.* has been paid to hurry the vessel forward.

163. That would be inconsistent with shipping the coals in small quantities by barrows, would it not?—Yes, it would have to be done by tip.

164. Are you aware that there are some drops in South Wales in which boxes are used?—No, I have no information except from hearsay with regard to South Wales. On the Liverpool side they have boxes lowered into the hold of the vessel.

165. Do you know at all how the coal has turned out which has been shipped by boxes?—No, the returns I have been favoured with only give the number of vessels loaded under the crane; the crane may lower the wagon, which is then tipped, and the coal may fall down the whole depth of the hold, or it may have a box from a truck slung to it and this box can go into the vessel. You can only say that it has been loaded from the crane, or loaded from the high level, as it is termed.

166. Frequently a vessel is loaded partly in one way and partly in another, is it not?—I do not know.

167. Is it not the case that sometimes a column of coal is formed in the hatch by boxes, and subsequently the coal is tipped from the wagons upon that?—Those that are careful might possibly make arrangements of that sort.

168. The wagon itself is lowered just over the hatch?—Yes, as near as can be.

169. It is quite as much to the coal owner's interest to preserve the coal from breakage as it is to the interest of the person who purchases. I presume the character of his coal depends on its size?—Of course those coal agents who have a name to keep up are desirous that their shipments should turn out satisfactorily.

The witness withdrew.

Captain J. Scurr.

Captain John Scurr examined.

170. (*Chairman.*) You command one of the ships of Mr. Ferguson, who is a member of this Commission?—Yes, the "Brenhilda."

171. You have carried several cargoes of coal?—Yes, two as master, but I have carried several half cargoes, and at one time when I was a seaman we carried an entire cargo.

172. The last voyage of the "Brenhilda" was your third voyage, was it not?—Yes, the second as master with an entire coal cargo.

173. On the previous occasions had you any trouble with your coal?—None whatever.

174. On the last occasion had you some trouble?—Yes.

175. Will you explain to the Commission what happened?—I found the coals got heated very much in the fore hatchway. The ship is wonderfully well ventilated; it has all the new improvements in ventilation; there are three large ventilators, one in each hatchway, coming from the keelson right up, wooden ventilators with holes pierced right through at intervals.

176. Your voyage was from where to where?—Glasgow to Calcutta.

177. When did the "Brenhilda" sail?—She sailed from Glasgow on the 23rd June 1874.

178. Where were you when you first experienced that heating?—Between the Tropics and the Equator.

179. Between the northern Tropic and the Equator?—Yes.

180. When you were between the northern Tropic and the Equator you first noticed this heating?—Yes.

181. About how long had you been out?—I should say it was 40 days. I had a long passage.

182. What did you first observe?—I observed a heating in the coals, and a strong smell of gas coming out.

183. In what part of the ship?—The forehatch.

184. What did you do?—I turned the coals over. I took the heated coals out, and endeavoured as well as I could to get to the cool coal. I took 20 tons of coals on deck to let them cool and put them in the fore peak.

185. How long did that take?—I daresay three days.

186. After that had you any further trouble?—I looked at them from time to time and turned them over.

187. Was there any coal between decks?—Yes; there was coal between decks as well as in the hold, but there was no heating between decks. It was all down in one place in the forehatch. The heating was just under the forehatch, between the afterpart of the forehatch and the foremast. We turned the coal over till we got to the cool coal.

188. And you replaced it in about three days?—Yes.

189. After that before you got to Calcutta did you notice any heating again?—No.

190. Did you take any precautions after you observed that heating?—Yes, we went and turned the coal over from time to time.

191. Between decks the coal never heated?—No.

192. What arrangements had you for ventilation?—We had three ventilators; we had one ventilator in the forehatch, and one in each of the other hatches.

193. What was the depth of the ventilator in the forehatch?—It went right to the keelson; that would be about 22 feet.

194. What did it ventilate into?—Into the hatch.

195. When the hatch was down what did it ventilate into?—The other ventilators would then carry off any foul air.

196. Supposing there was smoke or gas escaping, how would it get from the ventilator in the fore hatch to the other ventilators; I mean when the hatches were down how would any ventilation take place through the coal?—The coal was 3 feet from the 'tween decks. The ship was not full of coals; she was empty from the fore part of the fore hatch. The coal did not reach within 3 feet of the upper deck.

197. At all times and in all kinds of weather,

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whether heating was going on in the coals or not, would the ventilators reach the outer air?—Yes; we always kept the permanent ventilators open in any kind of weather.

198. (*Mr. Young.*) Had they the cowls on?—No; they are a sort of skylight, they are sloping. The masts are ventilators as well, they are iron masts.

199. (*Chairman.*) You reached Calcutta without further accident?—Yes.

201. Had you any trouble with your coal there?—Yes.

200. Will you describe what happened?—We discharged some of our cargo from the main and after hatches, and on the Saturday we found a dense smoke coming up. That was as soon as we got well down in the main hatch and the after hatch. A great draught seemed to come from aft, the wind being from north-east, causing a greater draught, and the coal seemed to ignite.

202. Will you describe what happened; what were the first symptoms of anything being wrong?—After discharging about 500 tons, the draught got into the ship and we noticed a strong smell of gas, we could not go into the fore hatch. The moment I found it out I got boats alongside, and got a lot of coolies to come on board, and I put canvas on the coolies' feet and set them to work on the coals. I put the steam donkey pump and the force pump down into the hold, keeping them constantly at work, and I kept the hose down the ventilator. I found that there was still a smoke, and that what I was doing was not sufficient to put out the fire, and I went to the port conservator and got him to let me have the steam float alongside, in case of further mishap, on account of being among a number of ships.

203. Was much water sent into the ship?—Four feet of water in about half an hour.

204. Then you got the coal out?—Yes.

205. In what state did it come out?—I should think 5 tons of this coal was taken out (*producing a specimen*) 10 feet below 'tween decks about 6 feet from the keel. It seemed to be in the middle of the fore hatch.

206. You took this coal in at Glasgow?—Yes.

207. How would you describe that coal from its look?—As half cinder, resembling coke.

208. Quite light?—Yes; there were 3 or 4 tons of that.

209. It had been in a state of half combustion long enough to reduce its weight to very little more than coke?—Yes.

210. Is it your impression that from the time when you had the indications of fire, near the Equator till you reached Calcutta, that coal had been burning?—I fancy so, on account of the heat being still in the same place; it was only 5 feet from where I found the previous heat, this was in the centre of the fore hatch, the other was between the fore hatch and the foremast.

211. What in your opininion saved you from worse disaster, was it the goodness of your ventilators?—I cannot fancy that because on previous occasions I had no trial of ventilators at all. It is a guess of mine, but I think the cause of the casualty must have been in the coals, because in some of the pieces I took up there was a stronger heat in the middle of the pieces of coal when I broke them than on the outside. I am speaking of pieces of 70 lbs. or 80 lbs.

212. Would it be your impression that it was the impossibility of any air getting to the coal that saved you from the like casualty on former occasions?—I would not like to say.

213. To go back to the coal itself, you say you sailed from Glasgow, do you know where the coal came from?—It was called Watson's coal in the bill of lading, that is all I know about it; I could not say from what colliery it came.

214. Do you know anything about the time it was in reaching you from the colliery?—It was a very short time.

215. Was it run down to the ship by rail?—Yes, the coals were taken in in four days.

216. They were put in at once?—Yes.

217. The coal was not standing a long time before you took it?—No.

218. The trucks were run straight to the ship?—Yes.

219. Was there a large proportion of small coal?—No, it was principally large.

220. Was it well picked as usual?—Yes.

221. It was not worse coal than you had had on former voyages?—No; on the former voyages I took 600 tons of small coal with large coal for Bombay.

222. What you were taking this time professed to be large coal?—Yes.

223. You knew nothing about it except that it went on board very quickly after it came from the pits?—Yes.

224. How was it put into the vessel?—By a steam crane which lifts the trucks up and tips them into the ship, we would not let them drop the coal far down.

225. It was carried by a crane and tipped. How high would the trucks be when they were tipped?—I should think 4 feet from the deck.

226. Did it seem to break very much in falling?—No, very little.

227. Did you notice whether it was damp at all?—It was perfectly dry when it came in; it was beautiful weather at the time, there was not a shower of rain.

228. It was at midsummer?—Yes.

229. Had you very dry weather going out?—I always tried to keep the coals dry, I would not let any wet get to them.

230. Had you any bad weather?—No, it was middling good.

231. Was it very close between the tropics?—It was very warm.

232. Was the air very highly charged with moisture?—Yes.

233. Were you hanging about in the tropics a long time?—Yes, I should think it was three weeks before we got the south-easterly trade. It was indifferent weather, the winds were contrary.

234. (*Mr. Vivian.*) Is the "Brenhilda" a sailing ship?—Yes.

235. (*Mr. Duncan.*) Do you know whether or not the coals which took fire in the forehatch were the same description of coals as those in the other part of the vessel?—They were the same, I believe.

236. Do you know of any reason why the coals in the forehatch should take fire any more than the coals in the after-hatch?—No, I do not know why they should; the coals which took fire seemed to be more charged with gas, they seemed to be a slatier coal, when you broke a piece it broke clean like a slate.

237. Did they look like the coals from which they get oil?—They had not any of that appearance. They seemed a slatier kind of coal.

238. You do not think that any water went down the fore-hatch by accident or otherwise?—No, I took great care of that. I always endeavoured to keep the coals quite dry.

239. At the time that you put out the fire in the fore-hatch when you were at Calcutta, by the means which you adopted, did you find the other coals quite cool in the lower hold?—Yes, all except in that one spot. They were a little warmer under the after-'tween-decks, but I attribute that to the want of circulation of the air on account of the weather. It was raining heavily at the time I was in Calcutta, that is to say, on the Saturday and Sunday.

240. (*Mr. Ferguson.*) Were there any other vessels loading coal at the time you were loading?—Yes, the "County of Perth" was loading.

241. Was the "Blair Athol" loading?—No.

242. Was she after you?—Yes, the "County of Perth" took all her coal over at Stob Cross by barrow.

243. Was that the same coal?—Yes, I believe so.

244. She loaded by barrow while you loaded by tip?—Yes.

245. Are you aware that the "County of Perth"

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was burnt ?—Yes, she took fire near Calcutta about 100 miles from Sand Head.

246. The "Blair Athol" took fire at Bombay ?—Yes.

247. She was loaded just after you ?—Yes.

248. With what kind of coal ?—I fancy Watson's Hartley.

249. During the time you were loading was there any rain at all ?—No.

250. (*Mr. Abel.*) When you first found that this heating had taken place in the coal you went down as far as the heated portion went ?—Yes.

251. Did you take out all the heated coals ?—Yes, and turned them over.

252. You went down to the perfectly cool coal ?—Yes.

253. Till you could feel no sign of heat whatever ?—Yes.

254. On both sides ?—Yes.

255. When the coal had got cool again did you put it back in the same place ?—We lifted 15 or 20 tons and filled the coal hole up with them. I thought that was the best thing to do. We kept it for the ship's use. There was 20 tons on the bill of lading for the ship's use, so I did not put it back as I thought it would be a good opportunity to use it.

256. Was other coal shifted into the place from which you had taken out that coal ?—It was just trimmed, and we left a vacant space in case of any more turning up.

257. I understand you to say that on the second occasion when the fire was discovered it was about the same place in the ship ?—A little before the same place, about 5 feet before.

258. But at the time when you first observed this heating you examined the coal generally ?—Yes, from time to time.

259. And you did not discover any other sign of heating ?—No.

260. Did you shift the coal at all after that from time to time during the voyage ?—No.

261. You did not assist the ventilation in any way ?—No. I did not think the ship required it, because she was so well ventilated all round. The air could get all round the lower hold, and also 'tween decks.

262. I thought you said that it was only in unloading the coal after you had got down to a certain point when the air began to get to the coal that you noticed the fire ?—Yes, that was in Calcutta.

263. Had anything been done between the two periods to stop the ventilation ?—On the second occasion there was a wind from the north-east, which caused a great current of air to go through.

264. That would look as if there was very little ventilation through the coal ?—As regards the body of coal I could not say. In the lower hold she was not sufficiently full in the wings or in the 'tween decks to stop ventilation.

265. So that the air circulated round the coal, but did not seem to penetrate through it ?—Not through the body of the coal.

266. What time elapsed between the discovery of the heating, and when you afterwards unloaded ?—Eleven weeks.

267. And you think there was no heating or fire at the time you replaced the coal, when you filled up the space after the heat was discovered ?—I did not fill up the space; I left it open.

268. Then there was increased ventilation to that extent at that part ?—Yes. At the time of the heating of the coal in Calcutta the principal rush of the smoke and the gas was up the ventilator.

269. Have you any reason to believe that one part of the ship was better ventilated than the other ?—None whatever; it was ventilated right through.

270. Was the size of the coal much the same throughout the cargo ?—The same.

271. There was very little more broken, or no more broken, in this particular lot than the other ?—It was about the same.

272. You think the character of the coal was somewhat different ?—I think so. It seemed a slatier kind of coal in the fore-hatch; it might have only been a truck or two.

273. (*Mr. Young.*) You say the ventilators were wooden ventilators ?—Yes.

274. Was there any sign of charring upon them when you got them out of the ship ? — None whatever.

275. When the fire occurred you did not find gas and vapour coming from the ventilator itself ?—No.

276. The smoke came from the body of the coal ?—There was a sort of steam came up the ventilator from the water going down.

277. I mean before that ?—It seemed to come right up the fore hatch.

278. There was no draught in the ventilator ?—No.

279. (*Mr. Icely.*) Is this (*pointing to the sketch*) a moveable cowl attached to this ventilator ?—Yes.

280. Did you trim it according to the direction of the wind ?—We always trimmed it off.

281. So as to be a down-cast or an up-cast ?—It was an up-cast for the gas to go up.

282. Were these ventilators in the hatchways always open on the top, or did you only open them occasionally when the weather would permit ?—No they would come up to here (*pointing to the sketch*). This ventilator was placed on the fore part of the hatchway.

283. Did the ventilators come through the hatchways ?—No.

284. Therefore, they were not up-casts except when you took the hatch off ?—That is so.

285. (*Mr. Cory.*) Though they do not come through the hatches they are always open, are they not ? The ventilator is still a ventilator though the hatch is closed ?—Yes.

286. (*Mr. Icely.*) Are you certain that there was a space between the deck here (*pointing to the sketch*) and the surface of the coal ?—I could travel from this coal right along to the fore hatch.

287. And these (*pointing to the sketch*) were constantly open ?—Yes, in whatever weather. In fine weather they were all open. These hatches (*pointing to them*) were all off. There are four sections of hatches between decks. I took the precaution of keeping them all off.

288. (*Mr. Ferguson.*) Had you the wing hatches off as well ?—Yes ; and the mid-ship hatches too. I stood them up against the side of the ship.

289. (*Mr. Icely.*) Was this (*pointing to the sketch*) an up-cast ?—This was a down-cast.

290. Could you get the air down there ;—Yes, when it did not rain. The moment it came on to rain we turned it off.

291. This ventilator with the cowl was used as a down-cast ?—Yes.

292. And this as an up-cast ?—Yes.

293. Were these masts also utilized as up-casts ; are there perforations in the masts as well ?—Yes. Then in our lazarette there was a grating that was always off, which caused a great draught. The rush of air was through here (*pointing to the sketch*).

294. (*Mr. Ferguson.*) Had you your hatches often off ?—Yes ; always when it was not raining.

295. During what portion of your voyage had you them off ?—During half the passage they were only put on at night, but the flap of the booby hatch was always down.

296. (*Mr. Icely.*) How did you ascertain that the cargo was becoming heated ?—You could tell by the heat and smell the moment you went down.

297. You did not take the temperature by the thermometer ?—No, In fact it was a thing I forgot to take with me.

298. Is it the custom to take a thermometer in ships ?—Not that I am aware of.

299. Or to test the temperature in the hold in any way ?—Not that I am aware of.

300. Do you think if the temperature of the cargo were tested at different points throughout the cargo,

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the means of discovering the seat of the fire would be afforded?—I dare say that might be done. It is one of those things that masters and mates, I am sorry to say, do not think of till it breaks out upon them.

301. You found the seat of the fire by walking along the deck and finding that it was hotter in one place than another?—Yes, I found smoke rising there (*pointing to the spot*). We could get right over the coals fore and aft. We could see anything rising in the way of smoke and gas.

302. (*Mr. Fenwick.*) What was the quantity of your cargo?—1,776 tons.

303. Was your vessel built of iron or wood?—She was an iron ship. It was her first voyage.

304. (*Mr. Cory.*) Upon the whole do you think you are better without ventilators or with them?—I think I should be just as well without them, looking at the way in which the ship is ventilated in other respects.

305. (*Mr. Vivian.*) Have you often carried cargoes of coal?—No, only on two previous occasions. I have several times carried half a cargo of coals.

306. On the previous occasions did you have ventilators?—None whatever.

307. Was the coal which you carried on the former occasions the same description of coal?—No, it was from the north of England.

308. If you had to load this same description of coal would you prefer to have ventilators or to have no ventilators?—I should leave that to my owners.

309. What would be your own judgment about it?—I am not able to give a confident opinion upon it. But speaking for myself, I would as soon go to sea with a ship that was not furnished with ventilators as with a ship that had them, provided then a stream of air got to the coals, or if the coals had been loaded out of barges.

310. That is to say, if they were put on board gently?—Yes, if they were taken from a craft alongside.

311. And lowered down into the hold?—Yes.

312. So that they would not become too dense in the ship?—Yes, I fancy myself from what I have heard that the heating of the coals is due to the coals coming direct from the pits into the trucks?—A sufficient time is not allowed for the gas to escape from them as used to be the case a few years back.

313. Will you give us the exact history of the occurrence of the fire at Calcutta. When did you commence to discharge?—On the Tuesday I broke bulk, and on the Saturday evening following we observed the smoke.

314. From the Tuesday to the Saturday the hatches were off and the cargo was ~~being~~ discharged?—Yes.

315. Was it being discharged at all the hatches at once?—No, from the main and after hatches. I was discharging from the after-hatch by coolies, and from the main hatch by the steam winch. The fore hatch was untouched.

316. That went on during the period between the Tuesday and the Saturday?—Yes.

317. How much coal was taken out between the Tuesday and the Saturday?—I should think 500 tons.

318. A freer circulation of air must have been caused after that quantity of coal was discharged?—We were well down in the main hold then.

319. You were well down in the main hold before the fire occurred?—Yes.

320. And consequently more air must have passed down?—Yes.

321. Then on the Saturday evening a dense smoke occurred?—It was slight on Saturday evening, then it increased on the Sunday.

322. Did you between the Tuesday and the Saturday examine the main hatch at all?—No.

323. Then you did not know whether the heat was gradually increasing during that period?—I could not say.

324. Your attention was attracted to the smoke rising?—Yes, on the Saturday evening the night watchman came and reported that there was a strong smell of gas, the fore hatch was off then. When the rain had ceased the the hatch was taken off to allow it to get cool, but he reported that there was a strong smell of fire coming up the fore hatch.

325. Did you smell it?—I went and smelt it.

326. What was the smell like?—A strong gasy smell.

327. Was it anything like ordinary gas?—It was a sulphury smell, a stifling smell.

328. Like a sulphur match?—A very stifling smell.

329. Was it anything like the smell when an ordinary fire-place is smoking?—No, nothing like it; it affected your breath, it was a stifling smothering smell.

330. What occurred after the Saturday evening?—On Sunday I could not do anything. I was going constantly to and fro. I found that the smoke and heat was increasing, and on Monday morning I got the donkey engine and the force pump to work and put down the hose, and as soon as I thought the office of the port conservator would be open, I went to the port conservator and got the steamfloat alongside.

331. By that time what was the condition of things?—It was getting worse and worse.

332. There was more and more smoke coming?—Yes, the steam donkey pump and the force pump were not sufficient to keep it down; they kept it down to a certain pitch, but the floating engine put it out instantly.

333. If you had been at sea you would only have been able to use your donkey engine?—Yes, the donkey pump, the head pump, and the fire engine.

334. Would they, in your judgment, have been sufficient?—Yes, I think so, if I had turned all hands to putting out the fire.

335. You think you would have had pumping power enough to extinguish the fire?—I think so, only as this fire-engine is kept on purpose at Calcutta I thought I might have the use of it. If I had been at sea we would have strained every nerve to put it out ourselves.

336. You said that on breaking some of the pieces of coal which were comparatively cool outside, you found that they were in a state of ignition inside?—There was a strong smoke arose when one of those pieces was broken.

337. You exposed more surface in breaking it?—It did not feel so warm outside as inside; it was only in some of the pieces of coal that we observed that. As the men passed them up, I saw a smoke rising from them, and I broke them on deck.

338. Do you think it possible that that coal had been submitted to the action of water outside, which had cooled the external surface, and had not cooled the inside of the coal?—I don't think it. I was very cautious not to let any wet get to the coals.

339. Are you speaking of when you turned over that coal at sea, or of the occasion when you discharged part of your cargo in Calcutta?—When I turned the coal over at sea I found this smoke.

340. And then you found the blocks of coal were cooler on the outside than in the inside?—Yes, if you struck them they would all fall to a crumble, and there was much heat inside.

341. Was there much small coal?—No.

342. The bulk of it was large coal?—Yes, ordinary sized coal, there were some very large pieces among them.

343. Did you find pieces of 20lbs. weight, for instance, in a state of heating?—No. I tried one or two pieces when the men were passing the coals up, and I let them lie on the deck to cool till I put them down the fore peak.

344. Did you remark any brassy look in the coal?—No. I did not notice that there was a shiny appearance.

345. A black shiny appearance; not a yellow appearance?—There was some of that on the coal too;

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there were gold looking streaks in some of it. I did not notice it in that which I broke, but I saw some of that on the coals like a vein of gold.

346. Was there more of that in the portion of the coal which was on fire in the fore hatch than there was in the other portions ?—I did not take notice of that.

347. (*Chairman*). Do you know iron pyrites when you see it ?—No.

348. Your impression is that the cause of the heating of the coal was its being brought too rapidly from the bottom of the colliery into the ship ?—Yes.

349. The coal you had been accustomed to take before had been exposed to the air longer ?—Yes.

The witness withdrew.

Adjourned to Tuesday next at 12 o'clock.

Tuesday, 8th June 1875.

PRESENT :

THE RIGHT HONOURABLE HUGH C. E. CHILDERS, M.P., IN THE CHAIR.

FREDERICK AUGUSTUS ABEL, Esq., F.R.S.
RICHARD CORY, Esq.
GEORGE DUNCAN, Esq.
JOHN FENWICK, Esq.
JOHN FERGUSON, Esq.
CHARLES ICELY, Esq.
JOHN PERCY, Esq., M.D., F.R.S.
WILLIAM YOUNG, Esq.
J. M. CARMICHAEL, Esq., Secretary.

Mr. CHARLES TULLY examined.

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350. (*Chairman.*) You are a shipowner at Newcastle ?—Yes.

351. I believe you have had some experience of cases of spontaneous combustion or explosion on board your ships ?—I have.

352. Have you a large fleet ? — We have 19 altogether.

353. Are many of them employed in the coal trade ? —The whole of them are employed in the coal trade.

354. And I believe within the last few years you have had more than one instance of spontaneous combustion ?—We have had two vessels burnt at sea.

355. One as far back as 1865 ?—Yes, 10 years ago.

356. And the other more recently ?—The other was in January 1873.

357. Are those the only instances that have occurred in your own fleet ?—They are.

358. Will you give the Commission some information about the first case ; what was the nature of the casualty ?—The ship was loaded with smelting coals from Swansea bound to Valparaiso. She was properly ventilated, because in West Coast charters we have always been bound to ventilate the coals properly, as smelting coals are more likely to take fire than steam coals. When she got off the river Plate the men felt the heat, and it gradually got worse and worse till she burned altogether. That was after she had been 60 days out.

359. Have you any recollection of the manner in which the coal was put on board ?—It was put on board in the usual manner, but I believe it was during wet weather.

360. Was it tipped ?—It was tipped in the usual way at Swansea.

361. Do you remember what the condition of the coal was when it came on board ; was there any objection to it on account of its dampness ?—I cannot remember that.

362. Was there any suspicion that the coal contained pyrites ?—It was just the same as the ordinary smelting coals ; they are very full of gas.

363. Will you describe how the ship was ventilated ? —At the present time they have iron ventilators, but we had wooden ventilators like spouts, which communicate with different parts of the vessel. These ventilators come up to the hatchway.

364. To each hatchway ?—To each hatchway.

365. When the hatches were down did the ventilators act ?—They would only act to a certain extent.

366. So that in bad weather when the hatches were down the ventilation would be imperfect ?—Yes.

367. Do you recollect any other special circumstance connected with the casualty which would tend to indicate the cause of it ?—No ; not any further than the nature of the coals. Smelting coals are very full of gas.

368. Was the ship totally destroyed ?—Yes. The fact was the men could not do anything with her : they had to come out of the cabin at last, they felt the heat so much. The ship took fire gradually, and the men had to leave her.

369. (*Mr. Duncan.*) Had your ventilators any connection the one with the other : had you any trunk below connecting the ventilators ?—Yes ; we had the trunk that we carried the copper ore in on our return voyages.

370. You had a double bottom ?—Yes.

371. Was each ventilator put through that double bottom ?—Yes, the ventilators were put through the double bottom.

372. Did they endeavour to put water down the hold ?—Yes.

373. They poured water down the ventilators, and did everything they could for some time ?—Yes.

374. (*Mr. Ferguson.*) Do you know whether the hatches had been much off during the voyage ?—Not expecting to be asked that question, I did not ascertain that, but I should think that during fine weather they would be off. We generally give instructions to that effect, that whenever it is possible to have the hatches off they are to be off.

375. Had she a permanent ventilator ? — Not through the deck. When the hatches were on there might be a little ventilation, but it would be very imperfect.

376. Was she a wooden or an iron vessel ?—Wooden.

377. What was her size ?—460 tons register ; she would carry about 700 tons.

378. She would have 15 feet depth of hold ?—She would be more than that ; she would be 17 feet I should think.

379. Was she filled with a full load of coals ?—Not quite a full load.

380. (*Dr. Percy.*) Do you know from what colliery the coal came ?—I cannot say.

381. Was it small coal, like slack ?—Yes.

382. There was not much lump in it ?—No, it was smelting coal.

383. Smelting coal is what we should call slack ?—Yes.

384. I suppose it would be difficult to ventilate a mass of slack ?—Yes, very difficult.

385. (*Mr. Abel.*) Have you been in the habit of shipping this particular coal ?—We have been years in the trade, and all the time that we have been in the trade we have been shipping the same class of coal.

386. Have you any reason to believe that there was anything exceptional in this particular cargo of coal ?—No, except that I think the weather at the time was very wet when they were shipped.

387. You are not aware whether the coal had been

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long exposed before shipment?—No, I could not say that.

388. (*Mr. Young.*) What was the name of the ship?—The "Morning Glory."

389. Were there any premonitory symptoms of fire; was there any smell of fire or gas beforehand?—If I remember rightly, the men for some days before felt that there was some fire about the vessel.

390. That is to say, they noticed the heat coming up the ventilators?—Yes.

391. Were the ventilators showing any signs of charring?—Very probably.

392. (*Mr. Icely.*) Were your men in the habit of taking the temperature of the coal by a thermometer? —No, I do not think they were at that time.

393. That has never been the practice?—No, it had not been the practice as far as we were concerned; it may be at present, it was not then.

394. (*Chairman.*) Now as to the second case, the second case was in January 1873?—Yes.

395. What was the ship's name?—"The John."

396. She was on a voyage also from South Wales to Valparaiso?—Yes, Porthcawl to Valparaiso.

397. Will you state what the nature of the coal was?—The same kind, smelting coal.

398. From the same colliery?—No, from another colliery, but it was the same character of coal.

399. Do you know from what colliery it came?— I cannot say exactly.

400. It was shipped at Porthcawl?—Yes.

401. Where did the combustion take place?—Off the Falkland Islands, when she had been 100 days out.

402. In the former case the ship had been about 60 days out?—Yes.

403. Do you remember the circumstances under which the ship was loaded: was it wet weather?— Yes.

404. Was the coal received on board in the usual way?—Yes.

405. Was it as small coal as the other?—It was about the same character of coal; it was smelting coal.

406. How was the "John" ventilated?—In the same way.

407. Was she about the same tonnage as the "Morning Glory"?—She would have about 580 tons on board.

408. She was rather a smaller ship than the other? —Yes.

409. Was the fire suspected some time before it broke out?—Yes, some days before. They felt the heat in the cabin some time before.

410. Do you remember what they did when they first found it out?—They threw water down.

411. Was the ship burned?—Yes, they ran on shore on the Falkland Islands to save their lives.

412. Both became total losses?—Both became total losses.

413. Was there any suspicion of the presence of much iron pyrites in the coal?—Not to my knowledge.

414. In fact the two cases were on all fours?—Yes, just about the same.

415. (*Mr. Ferguson.*) Was she a wooden vessel likewise?—Yes.

416. (*Dr. Percy.*) In 1873 the price of coal was very high, was it not?—Yes, in 1873 and 1874.

417. I suppose it is probable under those conditions that an inferior kind of coal might be supplied?—I cannot say that.

418. There might have been rather more slack than usual?—I cannot say.

419. (*Mr. Fenwick.*) You say she was fitted as a copper ore ship?—Yes, with a platform and trunk, so that when the ship was coal-laden the platform would be down, but the trunk would be taken away, leaving the uprights.

420. What space was there between the platform and the bottom?—Four feet.

421. (*Chairman.*) Have you any suggestion to make to the Commissioners on the subject referred to them, which is, what precautions should be taken to prevent the spontaneous combustion or explosion of coal, or what special steps should be taken when the existence of combustion is suspected?—In the case of smelting coals or gas coals much more difficulty arises than in the case of steam coal, but I think as regards small vessels on short voyages it would be a sufficient precaution if for a little time after the vessel was loaded, the hatches were kept off every fine day during the voyage; but in a larger vessel, where there is a greater quantity of coal, and where the voyage is longer, I think she ought to be properly ventilated. I think those vessels which are not fitted with trunks ought to have ventilators on each side of the keelson, and a ventilator up each hatchway and one besides those between the hatchways. A large vessel ought to have ventilators within a foot of the deck at least, and I think she ought to have screw ventilators some height above the decks, so that the water would not go down.

422. Those are all the precautions that you think would be necessary?—My own idea is that the heating of the coal is attributable to a great extent, to the coals being shipped fresh from the colliery, they come direct out of the mine into the vessel and as they fall into the vessel they break and the gas generates about the vessel. But you could not very well remedy that, the only remedy would be to apply precautions on board the vessel at sea. In former years the coals used to be allowed to lie in heaps some little time and the gas got away before they were shipped; but now they come, one may say, red-hot out of the mine.

423. Do you think they will continue to come, as you describe it, red-hot out of the mine, even when the price falls as it is falling?—Yes; because they will not work them in advance of the demand for them.

424. Is it your opinion that the intervention of Government, by means of a special inspection of coal-laden ships, would be of any advantage?—I think if there was a general law laid down providing for certain precautions, it would be acceptable to shipowners and it would be observed by them. All the owners that I have come near are anxious to save their vessels and they would be very glad to ventilate them. In fact, most of the merchants have the provision inserted in their charters, that the ship is to be ventilated.

425. You think if the law provided that certain precautions should be taken, the anxiety on the part of owners to save their own property would be quite sufficient an inducement for those precautions to be taken, and it would not be necessary to have an army of inspectors to inspect coal-laden ships?—Not at all.

426. What would you prescribe by law as to the ventilation of coal-laden ships?—The difficulty would be this, that each different vessel might require to a certain extent a different kind of ventilation. For instance in an iron vessel you might have permanent ventilators in different parts of the vessel, such as the stanchions and the timber heads might all be ventilated; but in a wooden vessel you would have to depend on screw ventilators on deck.

427. Could you formulate those requirements in any way, so that they could be expressed in a law, or in regulations having the effect of law?—I think it would be quite possible.

428. Do you think the Board of Trade could be empowered to make regulations?—Yes

429. How would they be enforced?—There is the difficulty; unless there were some person to see the vessels when they went away, I do not see how the regulation could be enforced.

430. You have not perhaps thought that point out? No. Before I left Newcastle I was told there was an old captain who had saved two or three vessels after a fire had taken place. He did it in this way (*producing diagram*). He had a number of wooden cones made, which he put on the top of one another as he dug down to the heated coals. He would put down one of these cones, in the inside of which there would be room for a man to work the coals, and then when he had got down to a certain depth he would put another cone on the top. He saved three vessels by that means. The cones would be 4 feet high by 4¼

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feet wide; there would be just room enough in them for a man to work. One man could not work in them for very long; he kept changing the men and kept working down, making a sort of shaft as it were.

431. Did he keep a stock of those on board?—Yes, he saved three vessels in that way, the "Cambria," the "Manfred," and the "Druid." They all took fire by the heating of the coals, and he saved them in that way.

432. (*Mr. Duncan.*) If those cones were only four feet square he could manufacture them out of inch and a half boards?—He must have made them on board.

433. (*Chairman.*) He did not take them with him?—No, he did not anticipate the fire.

434. (*Mr. Young.*) Did he make them after the fire began?—I cannot answer that question; he got them right down to the spot where the fire was. He told me that from the working of the cargo in the ship there was very little labour after the first one had been got down; it was just a matter of getting the first one down.

435. (*Chairman.*) What depth would the cones in that diagram represent?—About 18 feet.

436. (*Mr. Cory.*) That diagram is not intended to represent the precise number that would be required?—No.

437. (*Mr. Duncan.*) You say that for the purpose of ventilating the upper part of the vessel below the upper deck you would have screw ventilators?—Yes.

438. Have you ever seen the ventilator which is called McKinnel's ventilator, which has an up and a down shaft?—No, not to my knowledge.

439. A ventilator which has an outer and an inner jacket?—No, I have not seen it.

440. Such a ventilator as that which would constantly act would be better than your proposal?—Yes.

441. (*Mr. Ferguson.*) You have given us the particulars of two casualties which have occurred out of the whole number of coal-laden vessels which you have sent away during the time you have been a ship owner; what has been the total number of coal-laden vessels you have sent away?—Very many; we send between 400 and 500 cargoes away every year.

442. The proportion of burnt vessels to the total number of vessels you have sent away would be very small?—Very small indeed; I could not say how many years we have been shipping coal, but a great many years, and we only have had those two cases. That sort of casualty has mostly taken place of late years in the case of very large vessels bound to India, and it has taken place more with steam coals than with other coals. We have a fire-engine on board all the vessels, that is to say, one of those small engines they wash the decks with, and in case of fire they put the hose over the side of the vessel, and pump the water.

443. Is it not a steam pump?—No, a hand pump.

444. (*Dr. Percy.*) You spoke of the occurrence of spontaneous combustion; have you had any experience of explosion from gas?—No; I have known two or three instances on the Tyne, where after the gas coals had been loaded and the hatches had been put down, immediately the vessel blew up altogether, that is to say within two or three hours afterwards. In those cases the vessel had not begun her voyage, but the hatches had been put down, and the coals being full of gas, they caused an explosion.

445. Did I rightly understand you to express an opinion that steam coals were more liable to fire than other coal?—No, smelting coals are more liable to fire, but of late years the steam coals have been shipped in large vessels that they have taken fire more than the ordinary smelting coals which have been shipped in smaller vessels.

446. What particular kind of steam coal do you refer to, Welsh coal or north country coal?—Some of the north country; my experience is more in connection with the north country coal.

447. Do you know of any case of spontaneous combustion in the case of Welsh steam coals?—I should think there have been such cases; those vessels to which I allude carry between 2,000 and 3,000 tons, it is a great quantity of coal to be put in one vessel.

448. (*Mr. Fenwick.*) Do you ventilate all your vessels?—Yes, that is to say all vessels going any distance, not if they are only going to Spain or Portugal.

449. Your impression is that ventilation is very important?—For a long voyage.

450. (*Mr. Cory.*) I presume with respect to ships employed in coasting voyages it would be quite unnecessary to ventilate them?—Yes, they are only three or four days out.

451. Are you aware of any fire having taken place in those cases?—I never heard of one. I have heard of them being blown up after being loaded. Being small vessels there is not the same quantity of gas generated in the vessel.

452. (*Mr. Duncan.*) You have had great experience in shipping coals; you are a coal exporter?—Yes.

453. What is your experience with reference to the coals that are shipped in the present day, as compared with the coals which used to be shipped in former years, say ten or a dozen years ago, are they as well screened now, and in as good order for shipment as they used to be?—Better, I think. The screens are better now than ever they were, and the mode of shipment is better. In former years they lowered the coals down in wagons, and they knocked out the end of the wagon when perhaps it was 10 feet above the vessel. Of course all the coals went down, breaking as they went. Now they have spouts. The coals shipped by spouts certainly do break in large vessels, on account of the depth of the hold, but the spout itself comes down close to the hatchway of the vessel. They do not load the coal by wagons as they used to do.

454. Where are those coals screened?—At the pit before they come down to the spout.

455. How are they screened?—They have a large metal screen, and as the coals come out of the pit they are put through the screen, and the small coal goes one way and the large goes another.

456. Is it not the case that the coals come up in the truck from underground, and are then run on a railway to the edge of the staging to the screen upon which they are tipped?—Yes.

457. And the coal when screened goes direct into the wagons that go to the ship?—There are two wagons; one of the wagons receives the small coal, and the other wagon receives the large coal.

458. The one that receives the large coal stands at the end of the screen, and the one that receives the small coal stands under the screen?—Yes.

459. If there is a considerable slope do not you think it quite possible that some of the unscreened coal as it runs down may get mixed with the screened coal?—No.

460. In the rush down from the top of that screen to the bottom of it, might not a good deal of dust get into the large coal?—No, not much.

461. You are prepared to say that according to your experience coals are as well screened now as formerly?—Double screened coals which are the best steam coals are free from small when they are sent from the pit, of course they break afterwards.

462. Have you any reason to believe that on account of the high price that has been paid for coal, there may have been a temptation to the coal owners to ship a lower class of coal?—They could not do so, because their pit only produces a certain class of coal. For instance, north of the Tyne we have our steam coal pits, and those pits only produced the steam coals; we have other pits that produce gas coals.

463. In some of those pits many different seams, in the same pit.

464. Would you consider house coals as safe to ship as steam coals?—Quite as safe.

465. Would they be as a rule as well screened?—I think so.

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466. Supposing when a vessel was being loaded with steam coals they had not the necessary quantity of steam coals, and they made up the quantity with ten or a dozen tons of house coals, would that have an injurious effect upon that cargo?—No; it would not produce the steam when it got to its destination; but, so far as regards safety in carriage, I think it would be as safe. It would soon be detected that it was not steam coal, and the shipper would not get another order.

467. (*Chairman.*) Can you give the Commission the number of ships and the tonnage of the coal you have sent out?—We ship between 400 and 500 cargoes a year. I should say on the average they would be 300 to 400 tons.

468. (*Mr. Icely.*) Is it the custom to shelter the coal before shipment?—No, they could not do that; it comes down direct from the pit; if it is wet weather it receives the wet, and if it is snowy weather it receives the snow.

469. (*Mr. Young.*) I understand you to say that both those vessels were built to carry ore home; do you think the platforms and other things in ore ships tend to facilitate combustion, or to stop it?—I think so, they tend to prevent it. I think that has been proved by the very few copper ore ships that have been burnt.

470. The platforms are all made of wood?—Yes.

471. If there were combustion at all the wood would assist it?—With the platform above, and the ceiling underneath, there is always a current of air.

The witness withdrew.

Mr. James Finlay examined.

Mr. J. Finlay

472. (*Chairman.*) You have been master of a ship for some time?—Yes, from 1852.

473. And you have been carrying coal in the ships of which you have been master?—Yes, constantly.

474. I believe you have had no case of combustion since you were master of a ship?—No.

475. When you were chief mate you had a case, I believe?—I had one case, in 1851.

476. Will you describe to the Commissioners the particulars of that case?—The ship was the "Alfred" of Belfast. She was loaded in London. The coals were brought from the Tyne in colliers. She was 1,073 tons register, and we only took 1,050 tons of coal.

477. You were only half full?—About half full, a little more. The coals were shipped on owner's account, bought in the London market. I noticed the coals were small sized. I noticed they were weighed into the ship in drafts of 2 cwt. at a time by a sworn meter. She was bound for Valparaiso. We got on all right till we were off Conception, within a few days' sail of Valparaiso, when one morning, at daybreak, on opening the hatches we found a strong smell of coal gas; that was the first intimation of there being anything wrong. Soon after that smoke began to ooze up through the coals. We dug a large pit in the coals where the smoke was coming up the most, as deep down as we could, as long as the men could stand, for the smell of the gas made them sick. We then cut a hole in the upper deck over that, and poured down water and filled the pit quite full till it overflowed. The water did not seem to penetrate the coals much, as it overflowed and ran down by the ship's side. It was all of no avail, the smoke and heat increased in the hold, fire broke out, and about 60 hours after we first discovered that there was anything wrong we were driven out of her. The ship was then like a volcano.

478. The ship was lost?—The ship was lost.

479. Was the coal steam coal?—I could not say.

480. You described it as being small coal?—I noticed that it was small-sized coal.

481. It had not been screened I suppose?—I do not know.

482. Do you know how the ship was ventilated?—There was no ventilation whatever through the coal. We kept the hatches open in fine weather. We kept them on at night, and opened them in the day.

483. She was not a regular coal-carrying ship?—No.

484. Was it the first time she had carried coals?—Yes.

485. She was an ordinary cargo ship, not fitted for carrying coal, in fact?—Yes.

486. It was a case of combustion in an unventilated ship?—Yes, that ship had no 'tween decks. I consider 'tween decks a great help to a ship in the way of ventilation.

487. Have you had much experience of coal carrying since?—Yes; I have carried 15 cargoes to the East Indies and China, some of them very large, over 1,800 tons.

488. Within the last few years?—Within the last 20 years.

489. Have those ships been generally fitted for the carrying of coal?—Yes, they were ventilated.

490. How were they ventilated?—Wooden ventilators ran fore and aft through the bulk of the coals in the lower hold; 'tween decks they were not required as there is only a limited thickness there. This is a model of the ventilators which are put through the lower hold (*producing it*).

491. Those were put horizontally?—Yes, fore and aft, on each side of the stanchions. Suppose this is the middle of the ship, on each side there is a line of these ventilators fore and aft.

492. (*Mr. Duncan.*) At the bottom?—No, in the middle. In the ships I have commanded the lower hold would be about 16 feet in depth, and the 'tween deck about 7 or 8. The ventilators ran through the centre of the lower hold.

493. (*Chairman.*) Have they all been fitted in that particular way?—In that particular way.

494. Is that plan of ventilation your own invention?—No.

495. Is it a common plan?—It is a common plan adopted in vessels loading at Birkenhead.

496. What upright ventilators had you besides?—We had one in the main hatch. We connected the two ventilators in the main hatch, and put one upright in the after part of the main hatch.

497. Was there nothing up the fore hatch?—We kept the ends of the ship clear right down to the keelson, and these fore and aft ventilators projected through both at the fore end and the after end.

498. If the hatches were down what ventilation would the ship have then?—We had always some scuttles down through the forecastle and down aft, and four cowl-headed ventilators through the poop and forecastle open all weathers.

499. All ventilating upwards?—Yes.

500. Nothing ventilating downwards?—No; except that some of the mouths of the ventilators were generally turned to the wind, and the others from it.

501. So that some would ventilate downwards, and the others upwards?—Yes, according to the wind.

502. According to your experience that method is a very satisfactory one?—Yes.

503. Have you carried coal of all kinds—north country coal and South Wales coal, bituminous coal and non-bituminous coal?—The principal coals which I have carried were North Wales coals, Brymbo coal. We shipped one cargo of Merthyr steam coal at Birkenhead.

504. Have you ever had any suspicion of heat in any coal which you have shipped?—No.

505. Do you attribute your immunity in any degree to satisfactory ventilation?—Entirely, and keeping the hatches open.

506. You say that that system is common in Birkenhead ships; do you mean that it is adopted in all ships sailing from Birkenhead?—I cannot say that it is universally adopted. I know that a good many have it.

507. Do several different owners apply the same

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plan?—Yes, I believe so. The firm by whom I am employed have a great many coal-laden ships, and in those ships this plan has been invariably adopted, instructions being given to keep the hatches open.

508. (*Mr. Duncan.*) As to that cargo which you took from London, was it small coal like what you would call smithy coal?—No, it was ordinary coal.

509. Such coal as you see discharged from vessels in London for steam purposes?—Yes.

510. It did not appear to have been very well screened, but it was steam coal?—Yes.

511. It was not small coal?—No, not small coal entirely, there was some large among it.

512. The two handlings of the coals, first in the shipment on board the brig, and then on board your vessel, would have a tendency to break the coals; would not the distance which they would have to fall break them a great deal?—The depth they would have to fall in the ship's hold would be gradually decreasing, there would be a very small fall towards the end of the shipment. The coals were wheeled away from the hatchways in barrows.

513. Was she a colonial-built ship?—Yes, a St. John's built ship.

514. From the way in which she was loaded the whole weight of the coal would go down upon that which was at the bottom, and make it closer?—Yes.

515. Do not you think it would be an improvement in the case of those vessels which you say are ventilated in the way you have mentioned, if you had one shaft coming up through the quarter hatch, and another shaft coming up through the fore hatch, with which shafts the horizontal boxes which you say you placed fore and aft among the coals could communicate?—Yes, no doubt it would.

516. (*Chairman.*) That is a very cheap method of ventilation, is not it?—Very cheap.

517. What would it cost in the case of a ship carrying 1,200 tons?—The ship in which I sailed last was a ship 1,300 tons, and I reckoned that it cost about 13*l.* for the material.

518. The original cost?—Yes; the ship's carpenter made it on board as it was required, and saw the ventilators properly placed.

519. (*Mr. Ferguson.*) Do you know whether any of the other vessels which have from time to time loaded at Birkenhead, and which have adopted that plan of ventilation have taken fire?—I do not know of any; the employers for whom I sailed had at one time over a dozen ships, and they were nearly always carrying out coals to the East, and I never heard of a case of combustion in any one of them.

520. Have you heard of any casualty occurring in the case of any of the vessels which loaded at Birkenhead with the same coal, and which adopted the same method of ventilation?—No; I never heard of any casualty having occurred in the case of any one of those vessels in which that plan of ventilation had been adopted.

521. (*Dr. Percy.*) How long has this system of ventilation been in operation?—For over 20 years to my knowledge.

522. (*Mr. Abel.*) Are you in the habit of examining the cargo periodically, or the ventilators, to ascertain whether the coal remains cool?—I was in the habit of visiting the hold each day.

523. Is that a rule laid down by you on board ship?—No, not a stringent rule, but I made it part of my duty to go over every part of the ship every day.

524. You do not keep a record of having inspected the cargo, and seen whether there is any heating?—No.

525. (*Chairman.*) Do not you mention it in the log?—No.

526. (*Mr. Abel.*) Have you ever known a case of explosion on board a coal-laden ship?—No.

527. (*Mr. Young.*) Most of the cargoes you have carried have been from Birkenhead?—Yes.

528. You are in the employment of a Liverpool firm, I believe?—I was till I left off going to sea in 1872.

529. What is the name of the firm?—Messrs. J. and T. Sinclair of Belfast and Messrs. W. and P. Sinclair and Co. of Liverpool. I have taken three cargoes from Sunderland.

530. (*Mr. Icely.*) What is the name of the coal you have been in the habit of carrying?—Brymbo. One cargo I took from Birkenhead was Merthyr steam coal. From Sunderland it was Londonderry and Cowpen coal.

531. You have carried all those coals which you have mentioned?—Yes.

532. And never had an accident with them?—No.

533. Are you in the habit of shipping coal in all weathers?—Yes, I have shipped coal in wet weather.

534. You have known a cargo to be wet when shipped?—A little wet.

535. Charged with moisture?—Yes.

536. Still no accident has occurred?—None whatever.

537. (*Mr. Fenwick.*) You must have met with a great many other ships which have taken coals out to India; do you know whether they have all been ventilated?—There is a great diversity of opinion about ventilation; some captains are strongly in favour of ventilation; the majority I have talked with on the subject are in favour of ventilation, others are not.

538. You do not know whether every coal-laden ship that sails for India has been ventilated?—No; the majority are in favour of ventilation, but I have met with some, a few, that do not believe in ventilation.

539. (*Mr. Cory.*) Most of those cargoes which you loaded at Sunderland, and those you shipped at Birkenhead, came direct from the colliery to the ship?—Yes, as far as I know.

540. In the case of the particular cargo which you loaded in London, they had been some time out of the colliery, no doubt?—Yes.

541. They had had a thorough airing?—Yes.

542. Nevertheless, that particular cargo was the one that took fire?—Yes. We did not think there were any particular precautions necessary as the vessel was not taking a full cargo.

543. (*Chairman.*) May I sum up your evidence as expressing this, that provided that system of ventilation was applied to which you have referred, and provided the hold is visited every day, you would feel perfectly safe in carrying coal to any part of the world?—Yes.

544. You believe that those are all the precautions that are necessary to be taken?—Yes; and keeping the hatches off night and day.

545. Whenever the weather allows?—Yes.

The witness withdrew.

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Mr. JOHN PAUL NEWTON examined.

546. (*Chairman.*) You have been for some time commanding a coal-carrying ship have you not?—I have been in several ships carrying coals.

547. For how many years?—Since 1842.

548. How long have you been a captain?—Twenty-two years.

549. Have you made many voyages every year?—Yes. I have not been carrying coals continuously, but from time to time during that time.

550. How many voyages altogether have you made with coal?—I have carried coals from England to the Baltic once or twice, once to the Mediterranean, twice or three times to the Brazils, once to Calcutta, once to Shanghai, once to Japan, and once to Aden.

551. During the 22 years that you have been making voyages as a captain, have you had more than one case of spontaneous combustion?—Only one case.

552. Will you describe to the Commission what the particulars of that case were?—The vessel which was called the "Lochee," was loaded in the Tay, off Tayport, in the month of September, 1864. The weather was very wet and there were gales of wind every day. The coals were brought off in lighters, and the lighters

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in some cases were nearly foundering alongside, so that the coals were thoroughly saturated with both rain water and salt water. The coals were filled in tubs and whipped on board ship out of the lighters.

553. They came on board very wet?—Thoroughly saturated with wet. The ship just had her registered tonnage; she was not nearly full. She had 90,000 cubic feet of spare space. Her registered tonnage was 1,500 tons, and she only had 1,500 tons of coal in her. We sailed on the 17th of September, 1864, for Calcutta. Then on the 4th of January, 1865, about three months afterwards, we discovered a little smoke from the coals. On discovering this smoke I lowered the thermometer so as to get the temperature, which I found was very high, and I considered then that if she was not burning, she would very soon begin to burn. I was then in the Bay of Bengal. I immediately bore up for Ceylon, and when I was in sight of Galle she blew her decks out from end to end. That was a thing I had never heard of before or expected. I never apprehended any danger from explosion. No one could credit the force of the explosion. The decks were literally blown out. All the men were burnt and bruised. She went into flames immediately and we all got out of her as quickly as we could.

554. To what do you attribute that casuality?—I have not the least doubt that it was from the coals having been shipped wet. On the voyage before that, we had in the same ship 2,000 tons, which we shipped in London, and at that time she went perfectly safe. They were loaded then from small vessels, and the coal came from the north of England.

555. What coal was it in this case?—It was coal from the Town Hill Colliery in Fifeshire.

556. What coal is that?—Steam coal.

557. Since that casualty occurred I believe you have carried no coal?—No.

558. (*Mr. Duncan.*) I suppose you have known cases of this Town Hill coal taking fire since that time?—Yes, and before.

559. You have no reason to suppose that those cargoes were loaded in bad weather, or that the coals were saturated with wet?—I was in Dundee when the "Robert Gilroy" was loaded. She was loaded before the "Lochee." The coals which she shipped were very similar to those shipped by the "Lochee."

560. Were they loaded in the roads by lighters?—She took the greater part in in the dock, and finished loading in the river.

561. Was it very wet weather? — It was wet weather. She had a full cargo.

562. Those coals were brought from Fifeshire?—From the Town Hill colliery, the same colliery.

563. You have heard of other cases of this Town Hill coal burning where there was no reason to believe that the coal was saturated with moisture?—Yes; I have heard of several where the vessel has been destroyed having shipped the same coals, but I never took any interest in it till I was burnt out myself.

564. Have you any knowledge whether in the case of any of those ships that were burnt the coals were shipped dry? — In 1847 I went to Aden in the "Alexander," belonging to Dundee, and we took 1,000 tons of coal off Tay port in the month of May in dry weather, and no casualty happened in that case.

565. Were they Town Hill coals?—They were shipped at Tay port. I could not tell which colliery they came from. Those coals in 1847 went perfectly safe to Aden.

566. Do you remember what coal a ship which went out not long ago belonging to Messrs. Gilmore, Rankin, and Strahan was loaded with?—I could not say.

567. (*Mr. Ferguson.*) Previous to the explosion in your vessel how long had the hatches been on?—The hatches were off every day during the voyage. They were never battened down till the ship took fire. We never had occasion to close the hatches on account of sea. We merely put them on to keep out the rain and spray, covered with a tarpaulin, and tacked down in windy weather to prevent it being blown off, but they were never battened down.

568. You had them off nearly every day?—The hatches were lifted off the first thing every morning.

569. Were they off at the time of the explosion?—They were battened down then; that was to let the fire smoulder out, to keep it from spreading.

570. What mode of ventilation did you adopt?—There were several ventilators 12 feet apart, fore and aft, and in the main hatchway we had a ventilator at each corner.

571. Did those ventilators come up above the hatchway, or were they beneath the surface of the hatchway covers when the hatchway covers were on?—They were vertical ventilators made of wood.

572. Did they come up through the hatchway?—Only up to within 2 or 3 feet of the main deck.

573. Had the ship a permanent ventilator?—Yes. The ship was very well ventilated in that way, but I sealed her up tight when there was an indication of heating. This ventilation in the main hatch came up just under the hatch.

574. Was she an iron ship?—No, a wooden ship.

575. (*Mr. Abel.*) How long previous to the explosion had you any indication that there was anything wrong?—It was only 24 hours before she exploded that we had any knowledge of what was going on.

576. Then you found considerable indications of fire?—No, there were no indications of fire whatever till the morning before.

577. Do you remember any other case in which you noticed heating in a cargo of coal?—No. I never saw any heating in a coal cargo before in my experience.

578. (*Mr. Icely.*) You said just now that you lowered the thermometer into the hold when you discovered that the cargo was becoming heated. Had you been in the habit previously of trying the temperature of the cargo?—No; I never tried the temperature before.

579. It was only when you felt that there was heat that you put down the thermometer?—When I saw that the coals had really taken fire, I wanted to ascertain where the fire was. There was no doubt that it was in the main hatch.

The witness withdrew.

Adjourned to Tuesday next at 12 oclock.

Tuesday, June 15th, 1875.

PRESENT:

THE RIGHT HONOURABLE HUGH C. E. CHILDERS, M.P., IN THE CHAIR.

SIR GEORGE ELLIOT, Bart., M.P.
HENRY HUSSEY VIVIAN, Esq., M.P.
FREDERICK AUGUSTUS ABEL, Esq., F.R.S.
RICHARD CORY, Esq.
GEORGE DUNCAN, Esq.
JOHN FENWICK, Esq.
JOHN FERGUSON, Esq.
CHARLES ICELY, Esq.,
JOHN PERCY, Esq., M.D., F.R.S.
WILLIAM YOUNG, Esq.

J. M. CARMICHAEL, Esq., Secretary.

MR. JAMES WRIGHT examined.

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580. (*Chairman.*) You are engineer-in-chief to the Admiralty?—I am.

581. I believe you can give us some information about the cases of spontaneous combustion or explosion of coal which have occurred in Her Majesty's ships for some years past?—The papers generally

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pass through my hands, and I have looked up the records during the last seven years. So far as I can ascertain with any degree of exactness there have only been about five cases of spontaneous combustion, and two cases of explosion in men-of-war which were considered of sufficient importance to be reported during those seven years.

582. That is quite irrespective of ships chartered by the Admiralty to carry coal ?—Quite irrespective of those.

583. Can you give the Commission the particulars of some or all of those cases ?—Beginning in 1868 the first is the case of the "Euphrates" Indian troop ship. When nearing Bombay, shortly before midnight smoke was observed issuing from the starboard bunker. Fortunately there were only about 20 tons of coal in that part of the bunker. The fire was extinguished without any serious results. It was supposed to have been caused by the fact that some of the Spence's cement with which the boiler was covered had fallen off and an undue amount of heat had been communicated from the back of the boilers to the coal in the boxes. The kind of coal is not reported, but I believe it was Welsh coal.

584. That in the opinion of the Department was not a case of spontaneous combustion ?—Not a case of spontaneous combustion.

585. But of communicated heat ?—Yes.

586. What was the next case of which you have any particulars ?—The next case was that of the "Megæra." In February 1870, on her passage from England to Ascension, the temperature in the wing bunkers was discovered to be rising from 130° to 145°, and it was considered necessary to clear part of the bunkers. They did so, and the coal was found to be considerably heated, but no damage of any importance was done. The coal in that case was the Hirwain Aberdare.

587. What was the view of the Department as to the cause of the casualty in that case ?—The bunkers were reported to be well ventilated ; the usual precautions had been taken to prevent overheating. It was supposed that heat had been communicated in that case to the coal, but that was proved not to be the case* as the boilers were well covered, and no steam pipes went through the boxes. It was one of those difficult cases in which you could not give any clear reason for the heating of the coal. The Hirwain Aberdare is supposed to be a hard coal of the nature of anthracite, rather more so than most of the other coals. No satisfactory cause, I think, was ever given.

588. Was there an inquiry ?—Only the reports of the different officers upon the subject ; it was inquired into as well as it could be, but nothing satisfactory came of it as far as I could ascertain.

589. You were then at the Admiralty ?—Yes.

590. You could not arrive at any satisfactory conclusion ?—No.

591. What was the next case ?—The next case was the "Malabar" Indian troop ship. In February 1872, on her passage to Bombay, a strong smell of sulphur was observed by the men working at the boilers, on both sides at the same time. The bunkers were immediately cleared, and all ventilation stopped. The doors in the after end of the stokehold were opened, and the coal taken out as fast as possible. Much of it was found to be smouldering and heated, and only requiring a little air to burst into flame. It was necessary to flood the boxes entirely to put out the fire. The coal had been received from under cover at Malta only 16 days before, and was perfectly dry and apparently well mixed. It consisted of one half Cowpen Hartley received at Malta in April 1871, and one half Fothergills Aberdare received at Malta in September 1871. The test-tubes did not show any sign of the heat before the discovery, and afterwards they never showed more than 110°, which is very little above the usual temperature in the Red Sea.

592. What conclusion did you come to in that case ?—No decided conclusion was come to in that case either.

593. The coal was put on board perfectly dry ?—The coal was put on board perfectly dry, in good condition, and well mixed.

594. There was no reason to believe that the casualty arose from any external cause ?—Not in that case.

595. (*Mr. Vivian.*) That was the coal in the bunkers ?—Yes.

596. (*Chairman.*) What is the next case ?—The next case was that of the "Glasgow" on the East Indian Station. On the 22nd of June 1872 it was discovered that there was a heating of the coals in the starboard aft coal bunker. It was found that the greatest heat was about 6 feet down, and about 4 feet from the side athwart ships. The coal there was found to be at a temperature of about 135°. The coal was received on board perfectly dry, and in fine weather. The coal had been received at Aden, Bombay, and Trincomalee. It had been received on board in small quantities at those places. It consisted of Davis's Merthyr, Davidson's West Hartley, Tyne fine Merthyr, Cowpen Hartley. The greater part of it was Welsh coal ; it would probably be three-fourths Welsh and one-fourth of it North-country. There was no damage done in that case, and no clear reason was assigned for the heating of the coal.

597. What was the next case ?—The next case was the "Rattlesnake" on the coast of Africa. On the 20th of February 1872 smoke was discovered to be issuing from the bunkers at the back of the starboard boilers. On removing about 20 tons some discoloured coal was found. Precautions were taken to stow the coals quite dry. The coal was observed to contain a large quantity of sulphur and iron pyrites ; that was the report of the officer. I suppose it would be sulphide of iron.

598. In that case did you form any judgment as to the cause of the heating of the coals ?—No.

599. What coal was it in that case ?—It was entirely Cowpen Hartley.

600. In the two other cases, the "Malabar" and the "Glasgow," there was also some Cowpen Hartley on board ?—In small quantities.

601. (*Chairman.*) Will you go to the next case ?—The next case was the "Daphne," on October 11th, 1874. She was lying at Trincomalee. One lid of the port aft bunker and one lid of the starboard aft bunkers were blown off, and a flash of flame was observed. Smoke was then seen ascending. In shovelling out the coal a stoker came across a small portion of burning coal. The pumps were then used, and the fire was extinguished. The temperature of the bunkers before the occurrence, close to the seat of the fire, was not beyond the usual average. Starboard the temperature was 92° and port it was 95°. The main steam pipe passes through the port bunkers so that its temperature is rather higher than starboard when the ship is under steam. The coal was two thirds Nixon's Navigation and one third Davison's West Hartley. It was shipped at Aden in a perfectly dry state. It also appeared dry on the 11th of October. It afterwards transpired that the bunker lids on those coal boxes had not been removed for some time previous to the fire, contrary to regulations. It was supposed at first that the casualty was caused by an explosion of gas which had communicated the fire to the coal.

602. Was there any special inquiry in that case ?—There was an inquiry by the Commander-in-Chief.

603. Do you know by whom it was conducted ?—The Commander-in-Chief made the inquiry personally, I think.

604. What was his opinion ?—His opinion was that it was through not ventilating the coal boxes sufficiently.

605. Did you concur in that ?—So far as we could see there did not appear to be any other reason assignable for it. Those are the cases of coal taking fire. The only two cases resulting from explosion of gas occurred in the "Devastation" and the "Minotaur."

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* *See* Admiralty Papers, page 62, which confirm this statement, and show that Mr. McCulloch in his replies, No. 729 and 730, was mistaken.

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In the "Devastation" in June 1874, the ship's cook's assistant very early in the morning entered the bunkers with a naked light and an explosion of gas took place.

606. Was any mischief done?—There was no mischief done except that the man was burnt somewhat about the face and neck; the bunkers lid had been removed two days before, so that the gas must have been generated during the interval.

607. Was anything done to the coal afterwards?—No, the coal was all examined. No increase of temperature was found to have been caused, and there was no appearance of any heating in the coal.

608. What description of coal was that?—Welsh. $\frac{2}{3}$ Fothergill's Aberdare and Insole's Merthyr, I think, and $\frac{1}{3}$ Cowpen Hartley.

609. What was the date at which the coal was received, and how long before that date had it come away from the colliery?—The coal had been shipped at Portland six days before. The bunkers was nearly full; there was no indication of heat or damp; the temperature was 66°, and the explosion was confined to the neighbourhood of the aperture.

610. The "Devastation" was one of the Channel fleet?—She was lying at Portsmouth at the time.

611. The "Devastation," while in the Channel fleet, received nothing but Welsh coal?—It was mostly Welsh, but mixed as before mentioned in the bunker where the explosion occurred.

612. The other case of explosion was the case of the "Minotaur?"—Yes, in June 1869. There then was a slight explosion of gas in the starboard aft bunker opening into the shell compartment. On a light being lowered into the bunker for the purpose of examination it was blown out by the rush of gas, and there was a slight explosion, but no harm done—nobody was injured.

613. Was the coal turned over afterwards?—The coal was examined and it was found that there was no heating; the temperature in the forward bunker was 71°, and in the aft bunkers 68°.

614. What coal was that?—A mixture of several kinds of Welsh and north country received on board at various times during the previous nine months. The last supply was received at Lisbon in May 1869, it was on the top of the other coal, and consisted of Dowlan's Merthyr, Ocean Merthyr, Powell's Duffryn, Hastings Hartley, and Cowpen Hartley; exact proportions not recorded.

615. Those are all the causes of spontaneous combustion or explosion which have occurred in the navy for some time past?—They are all the cases that appear to have been considered worth reporting to the Admiralty within the last seven years.

616. Can you tell the Commission what precautions are taken in the navy to guard against over heating and to detect any rise of temperature?—There is an old standing order that the wood work of all coal boxes is to be lined with sheet iron and the Captains are to see that the coals are never shipped wet. They are to see that they are kept as dry as possible on board, and when a fresh supply is received they are to cause as much of the old coal as possible to be trimmed forward so that it may be used first, and they are to cause the temperature of the coal to be taken through the temperature tubes every watch, and recorded. In all large ships, at least, the coal boxes are placed from 16 to 18 inches from the boilers, and means are taken to create a current of air between the coal boxes and the boilers. The boilers are covered with four thicknesses of boiler felt to prevent radiation of heat. There is a standing order also that the coal boxes shall be ventilated by the bunker lids being removed occasionally. And lately, about a year ago, a general order was issued that in all ships some means of ventilation should be provided for carrying off any gas that might be generated in the coal boxes. The general plan is to lead some light iron pipes from the upper part of the coal boxes to the nearest funnel casing.

617. Have you noticed of late years, say within the last two of three years since the great rise in the price of coal, any increase in the number of cases of spontaneous combustion?—I think not. Of late years there has been no difference. There were rather more cases between 1871 and 1873 than we have had the last two years.

618. To what do you attribute that?—I cannot give any reason for it.

619. At any rate in 1873 and 1874, when, undoubtedly the number of casualties in coal-carrying ships was much greater, there was no corresponding increase in the number of casualties in the navy?—No.

620. (*Mr. Icely.*) There was a case of spontaneous combustion of coal in a store at Jamaica some years ago. Have you the particulars of that case?—That was in September 1872. The coal was stored in a brick or stone-built building, I cannot say which, and it was roofed with corrugated zinc. The coal was Cowpen Hartley, and had been landed in April 1872. The temperature under the store was generally about 97°. The coal was discovered to be hot, and they removed about 240 tons. It was reported by the officer there that the coal appeared to contain a considerable quantity of sulphur and iron. There was no cause assigned for the heating further than that. There was no serious damage done. The coal was slightly charred and injured, but there was no serious damage.

621. Are there any means taken to ascertain the temperature in the Government stores from time to time?—I do not know that any special means are taken to ascertain the temperature in stores on shore; but a system of ventilation is carried out by louvres over the top in some cases, and in other cases by side louvres all along; but as regards the taking of the temperature, I do not think there are any distinct orders upon the subject.

622. Was there any evidence as to how the fire, or the heating of the coal was first discovered in the store at Jamaica?—That is not stated in the report. The report simply says that the coal was discovered to be hot.

623. Do you attribute the immunity which the vessels in the navy appear to enjoy from this tendency to spontaneous combustion of coal to the fact of the temperature being regularly taken, and to those orders of the Admiralty to which you have referred being carried out?—I should think that would be one great reason for it. By reason of the discipline on board a man-of-war, and by the temperature being regularly taken in that way, a fire is prevented from getting ahead to any extent before it is discovered. It is rather strange that in two of the cases which I have mentioned, the thermometer on being lowered in the temperature tubes did not show any unusual rise. The fire or heating seemed to be confined to a very small space. It was first discovered from the smoke or sulphurous fumes coming up the boxes, not from the indications of the thermometer.

624. Have you in any of the cases any information about any rise of temperature having taken place; for instance, is there any statement that the thermometer was lowered at a certain time and gave you a certain number of degrees, and that it was lowered at another period, an hour or so after, and that it showed an increase in the temperature during that period, so as to indicate whether the heating took place rapidly or not?—No, I do not think that in any of these reports that has been recorded, for in most cases as soon as they discovered that the temperature was high they took means immediately to get at the seat of the heat, so that they did not wait to take the temperature subsequently. In all these cases I think they did not wait to record the gradual increase of the temperature.

624*a*. At present those tubes are plain. Do you think if they were perforated they would show indications of heating in the coal more readily?—They would show those indications more readily no doubt, because there would then possibly be a current towards the tube. However, I believe it is a question with some people whether it is right to ventilate coal-boxes much or not.

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625. Are you in favour of the ventilation of coal-boxes?—We are in favour of the ventilation of coal-boxes, so much so, that an order has been given to ventilate coal-boxes in ships of any kind, by means of these tubes leading to the funnel casings.

626. Those are outlets, but are inlets provided, or recommended?—No inlets, except what you get by removing the bunker lids, and opening the trimming doors.

627. Then would the air pass through the body of the coal?—Yes. Then when the coal was taken on board ship on most occasions the trimming doors would be open, which would give ventilation from below. The shoots in front of the trimming doors are generally free from coal, so that generally there would be a current there whenever the bunker lids or doors were open.

628. That is usually the case on board ship?—Yes.

629. An inlet is provided by that means?—Yes.

630. The exit would be through the funnel casing, where an active circulation would be kept up from the heat maintained there?—Yes.

631. (*Mr. Fenwick.*) In all those cases which you have given us, the coal appears to have been mixed coal?—It was mixed coal in most of the cases, but Welsh in the "Euphrates" and "Megæra," and north country in the "Rattlesnake." The report in that case only refers to Cowpen Hartley received at Sierra Leone.

632. It was only Cowpen Hartley in the "Rattlesnake"?—Yes, so I infer from the report.

633. Do the Admiralty take such coal as the market produces, or is the Admiralty in the habit of sending particular descriptions of coal direct to the particular ports?—I am not very well acquainted with the way in which the contracts are made at present; but I understand that Mr. Rowsell, the director of contracts, has been requested to give evidence, and he will be better able to tell you all those particulars than I am. I think the Admiralty contract, in most cases, direct with the mine owner to supply certain quantities of coal for certain stations; and the Admiralty are not at the risk of shipping them, they only receive the coals as they are sent over the ship's side, the coal owner, or the shipowner, taking all the risk; the Admiralty simply receive the coals at the stores. They order a cargo of Cowpen Hartley, or a cargo of Davisons, or a cargo of Welsh coal to be delivered at certain depôts by certain times.

634. (*Mr. Vivian.*) You have given us five cases of spontaneous combustion, namely, the "Megæra," the "Malabar," the "Glasgow," the "Rattlesnake," and the "Daphne"—Cowpen Hartley formed a portion of the cargo in four out of five of those cases—have you investigated the character of the Cowpen Hartley coal at all?—No, I have not. Samples of coal are generally tested in the coal testing boilers in the dockyard, and, speaking from experiments that were made with it, I think as a steam producing coal it is about the best of the Hartley coals.

635. In view of the fact that in four out of the five cases Cowpen Hartley formed a portion of the cargo, you have not thought it necessary to examine it so as to see whether it contained much iron pyrites or not?—No.

636. In two cases it was reported to contain iron pyrites?—That was the statement in the report.

637. (*Mr. Duncan.*) As it is not customary to have in merchants' ships which carry coals these tubes for testing the temperature, perhaps you will be good enough to describe what those tubes are?—They are simply iron tubes fitted in the deck above the coal bunkers, 3 inches in diameter, having a moveable lid, and the tubes are long enough to reach to within about 2 feet of the bottom of the coal bunkers. You take off the lid and lower the thermometer by a string and leave it till you get the correct temperature of the region around. They are fitted at a distance of about 8 feet apart.

638. They have no holes perforated in them?—They have no holes perforated in them.

639. Unless the lower end was left open, they would be apt to get the small coal in them?—Yes; and if perforated round the sides the small coal would get through.

640. Those tubes come up through the deck?—Yes, so that you can lower the thermometer down them.

641. Have you any impression that the mixing of the coals is an injury, that is to say, the combining of different coals?—No, I have no opinion upon that subject. I have not seen anything to call particular attention to it.

642. Some ship owners object to carry mixed coals having the impression that the mixing of coals is calculated to cause spontaneous ignition?—There is at present a standing order at the Admiralty that mixed coals should be used in the navy in proportion of two-thirds Welsh and one-third north country on most of the stations, and that the coals should be mixed on board, that two bags of Welsh and one of north country shall be put in the bunker alternately.

643. The fair inference from that is, that the Admiralty have no idea that mixing has any effect at all in causing spontaneous combustion?—Quite so.

644. (*Mr. Ferguson.*) Were those coal boxes in which the coals took fire nearest to the boilers?—At the back of the boilers, near the boilers, there was a space of about 16 inches in each case between the boilers and the coal box.

645. Was there a fire in the boiler in all the cases?—There was not in the "Devastation," "Minotaur," or "Daphne," I think.

646. It is not usual in merchant vessels to put the bunkers so far away back from the boilers. Six inches is about the distance in the case of old low-pressure boilers, but in the case of high-pressure boilers the distance apart is greater?—In the navy we keep them about the distance apart that I have mentioned, it is partly with the object of getting round the boilers to do any repairs, that distance gives sufficient space for a man to get between the coal box and the boilers. In some small vessels in the navy where the quantity of coal carried is necessarily small and the coal boxes are of small cubical capacity, we have been obliged to put them within about 6 inches of the boilers; in those cases the bulk of the coal is not large, and with the temperature tubes fitted we have had no reason to believe that there was any risk in putting the coal boxes so close to the boilers as 6 inches.

647. From the fact that the coals which took fire were so near the boiler, you would infer that the heat of the boilers affected them would you not?—In some cases the temperature tubes did not show any increase of heat. A little would be due to proximity to the boiler.

648. Are you aware whether, in merchant vessels, coals have become heated too near the boilers?—No, I am not acquainted with any such cases.

649. You said that you thought the navy was more free from cases of spontaneous combustion than merchant vessels?—I think so from the fact of precautions being taken to guard against danger and from the better discipline on board men of war.

650. Do you think the distance of the coal boxes from the boilers would have any share in it?—It would have something to do with it, no doubt.

651. (*Dr. Percy.*) You spoke of one case in which it was believed that fire had been communicated to the coal on account of the removal of the cement of the boiler. What temperature do you think would be communicated to the coal under these conditions?—I could hardly say; the temperature of the boiler itself would be between 260° and 270°, then there would be a space of 16 inches between the back of the boiler and the coal box, and the seat of the fire was about 4 feet in.

652. It was not on the outside that heat would be greatest?—No, but a little way in.

653. Is it the impression at the Admiralty that all coal when wet is more likely to fire than when dry?—That is the strong impression at the Admiralty.

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654. Can you adduce any facts in justification of that conclusion ?—The only case I know of late years is a case of heating of coal that took place about a year ago in the Bermuda coal store.

655. Do you know whether that coal happened to contain much iron pyrites ?—No, the report was that it was small coal taken into store when the atmosphere was moist.

656. No reference was made to the point of its containing iron pyrites ?—No, I think not.

657. Is any really searching examination ever made at the Admiralty with a view of ascertaining the amount of iron pyrites in the coal which they consume, or the manner in which that iron pyrites is diffused through the coal, whether uniformly or in lumps ?—I do not know of any such inquiry having been made.

658. In one case to which you referred you said that the fire was very local in that case. Was any examination made with a view to ascertain whether that part of the coal in which the firing took place contained more than the usual amount of iron pyrites ?—That I think was the case of the "Daphne." In that case the report contained no statement that the coal appeared to contain more than the usual amount of iron pyrites.

659. No examination upon that point was made ?—No special examination was made.

660. (*Mr. Abel.*) According to the regulations, the operation of mixing the coal is to be carried on during the loading of the coal on board ship ?—That is so.

661. The different sorts of coal are shot into the bunkers alternately. A load of one coal is first shot in, and then one load of another coal ?—Yes.

662. Has any note been taken as regards the mechanical condition of the coal in the bunkers in those cases of spontaneous combustion ?—I think not.

663. No record exists as to whether the cargo appeared to be composed of small coal to a greater extent on some occasions than others ?—The only case in which any reference was made to the smallness of the coal was that of the coal store in Bermuda. There the report mentioned that the coal was very small, and that it lay close together ; and it was also stated that it was taken in when the atmosphere was moist.

664. (*Mr. Young.*) You have told us how the Jamaica store was built, do you know whether other stores elsewhere are of a similar description ?—Some of them are built of brick, and others are built of iron.

665. Are they always covered ?—In all cases, I think, they are covered I think all the foreign ones are covered.

666. What depth of coal is generally stored ?—Various depths, from 5 or 6 feet to 12 feet.

667. The coal would not be so deep as in the hold of a large ship ?—I think not. In some cases the coals are stored in building with brick walls and arches, and those would be from 10 to 12 feet deep ; on the top of the arches there would be *louvres*, those arches would be about 10 feet wide and perhaps 12 feet deep running in long bays.

668. So that there would be a thorough draught ?—There would be a good draught. In the coal store in Gibraltar, for instance, the back of the store is part of the rock, then there is an iron roof, and the sides are covered a certain way up, then perhaps there would be 4 or 5 feet comparatively open all round at the upper part of the sides.

669. You know of no case of combustion of coal in a store on shore, except the case at Jamaica ?—I think there was a case at Woolwich about ten years ago, but I have no particulars of that case.

670. (*Chairman.*) Experiments with regard to mixing coal for the use of the navy have been going on for the last 12 or 15 years, more or less, have they not ?—Yes.

671. Was one of the points kept in view in those experiments the liability of coal to fire ?—No, that matter did not arise in the inquiry so far as I know.

672. You said that there was some difference of opinion whether ventilation promoted or prevented spontaneous combustion ?—That is what I gather from people whom I heard discussing the question. Some are of opinion that it is not desirable to have much ventilation, the general opinion, however, is, I think, that it is desirable to have ventilation.

673. The view of the professional officers at the Admiralty is that it is desirable to have ventilation ?—That it is desirable to have ventilation in the coal boxes on board ship.

674. Would you apply *à priori* the same to merchant ships carrying cargoes of coal ?—I think so.

675. So far as the experience derived from what has taken place in the carriage of coal in bunkers in ships in the navy goes you think it points to the desirability of thorough ventilation rather than of no ventilation ?—I think so.

676. But as to coal-carrying ships you have no personal experience ?—I have no personal experience with regard to coal-carrying ships.

677. Can you give us any idea of the quantity of coal that is carried in the bunkers of a man-of war ?—It varies from perhaps 20 tons to 300 tons, according to circumstances. In large iron vessels the coal bunkers are really water-tight compartments, local conditions regulate the quantity of coal carried in the different compartments. In some cases 200 tons would be carried in one compartment, in others 40, or 50.

678. (*Mr. Duncan.*) In a vessel like the "Euphrates," the compartments would carry more than 1,200 tons, would they not ?—There are a great many compartments in the "Euphrates" ; she would carry 1,200 tons altogether. In the after coal reserves she would carry perhaps 300 tons ; then there would be 100 tons an each side of the engines ; then there would be perhaps 200 tons on each side of the boilers, and then in another reserve, perhaps, she would carry 300 tons.

679. (*Chairman.*) About 300 tons would be the outside carrying capacity of any one of the compartments ?—So far as I recollect at present that would be the outside capacity of any one of the compartments.

680. (*Mr. Vivian.*) In regard to the Bermuda store in which you stated the coals got heated about a year ago, what quantity of coal was in the store ?—I cannot say ; the report does not state the quantity.

681. What description of coal was it ?—Baltimore coal. American coal. It is a small coal, something like our Welsh coal. It was used on the North-American station for mixing with Nova Scotian Pictou coal. Nova Scotian Pictou coal is of a bituminous nature, and it was supposed to burn better with a mixture of this Baltimore coal, but since that casualty orders have been given to discontinue the use of Baltimore coal.

682. (*Chairman.*) You call it Baltimore coal ; that is to say it was shipped at Baltimore ?—Yes.

683. It was a Pennsylvanian coal, was it not ?—I believe so, and shipped at Baltimore.

684. (*Mr. Vivian.*) You have given us seven instances in which casualties have occurred in men of war. During the period that those casualties occurred, how many of Her Majesty's ships were in commission ?—The number of steamships in commission would be 170 on the average during that time.

685. The number of years in which those casualities occurred was about seven ?—Yes, the casualties would be about six-tenths per cent. per annum.

686. Rather more than half per cent. per annum ?—Yes.

687. (*Mr. Icely.*) The seven cases include spontaneous combustions and explosions ?—Yes, about that.

688. In the explosions little harm was done ?—Very little ; in fact there was no material damage done in any case.

689. (*Chairman.*) If you excluded the explosions, it would be half per cent. per annum ?—Yes.

690. Will you give the Commission some information as to the manner in which coal is stored at home

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ports. Is it covered?—In most cases it is covered. At Portsmouth there is no coal store on shore, but there is a large one being built in connexion with the dockyard extension. Some of the coal is stored in the open air in the dockyard, and there are coal hulks and haulabouts that will contain altogether about 10,000 tons. At Devonport there is a store, I do not know the quantity it will contain, but there is an iron building there with openings round the eaves, and there are also coal hulks and haulabouts there.

691. And at Sheerness?—I do not think there is a coal store under cover there, most of the coal however is in hulks.

692. Have you known any instances in the last seven years of combustion in either of those three places?—I do not know of any case of spontaneous combustion having occurred in a coal hulk. With reference to the question, I was asked by Dr. Percy as to whether there was any inspection of coal for iron pyrites. I may mention that in the South Wales district and in the North Country district there is an inspector resident who is supposed to see that the coal shipped is of good quality and free from impurities.

693. (*Dr. Percy.*) All that he does is simply to inspect the coal?—That is all.

694. He does not make any chemical or other examination of the coals?—No.

The witness withdrew.

MR. HENRY JOHNSON McCULLOCH examined:

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696. (*Chairman.*) In what capacity are you employed under the Admiralty?—I am consulting coal engineer to the Admiralty.

697. Have you been long connected with the coal trade?—More than 30 years.

698. Will you tell the Commission in what capacity?—I originally commenced my public life as a mining engineer. Then I was for 17 years consulting coal engineer to the Great Northern Railway, in which capacity I had to buy all their coals. Then I left them and became a viewer and consulting engineer on my own account, having under my charge several collieries in North Wales and other places, till I joined the Government seven years ago. In addition to that, I have been for 20 years, and am now, consulting coal engineer to the Madras Railway Company. I advise as to buying their coals, and inspect them, and ship them. I have sent out nearly 400,000 tons of coal and coke to Madras.

699. You have had 20 years' experience in shipping coal for the Madras railway, and six or seven years experience in shipping coal for the Government?—Yes.

700. Can you give the Commission any information as to cases of ignition of coal which have taken place in your experience since you have been connected with the Madras railway and with the Admiralty?—I have only had two cases of spontaneous combustion in shipping coals for the Madras Railway, and singularly enough they were in 1874. I never had a case before 1874.

701. Have you ever had any cases of ignition in shipping coal for the Government?—In shipping the hundreds and thousands of tons of coal for the Government, I have not known the case of a single ship in which spontaneous combustion has occurred, except one, though there have been two or three cases of spontaneous combustion in some of Her Majesty's ships.

702. That is to say, in the coal ships which have carried to foreign ports the coal which you have purchased for the Government, you have only had one case of ignition?—Only one case, and that was in English waters.

703. Will you state the particulars of those three cases to which you refer, two of them being shipments for the Madras Railway, and the other being a shipment for the Admiralty?—In the case of the shipment for the Admiralty the coal was shipped at Cardiff; there had been contrary winds, and the ship was boxing about, and when she was a little beyond the Scilly Islands she took fire.

704. What was the size of the ship?—She was about 800 tons.

705. How much coal had she on board?—About 800 tons.

706. She was full?—I think she was quite full; she was about 500 tons register.

707. Was any inquiry made into the circumstances of that casualty?—No; the men took to their boats; the ship was burnt, and went down.

708. When was that?—It was in 1872 or 1873.

709. Was no inquiry made afterwards into the circumstances?—I never heard of any particular inquiry; it did not come under my notice particularly, for this reason: we buy all our coals delivered at a port; we contract with colliery owners on this side, and we say, At what price will you deliver at Gibraltar? for instance, and they have to effect their own insurance, and find their own shipping, and everything. If they deliver the coal at that port they are paid for it.

710. So that it did not become the duty of the Admiralty to make any inquiry into the case?—No.

711. Did not the Admiralty take any interest in it with reference to the kind of coal purchased, so that it might be avoided if it was found to be liable to spontaneous combustion?—I do not believe that coal could ignite; the casualty must have been owing to something external to the coal itself.

712. What coal was it?—If I mistake not, it consisted of two kinds of South Wales coal, the exact denomination of which I am unable to give. It was shipped at Cardiff.

713. Now will you give the particulars of the two cases you mentioned of coals shipped for the Madras railway?—The first was the "Numea," which sailed from the Tyne on the 1st of May 1874; she had 1,530 tons of coal filled up. The coal was Cowpen Hartley coal. On the 14th of September (according to the official statement of the captain and the notary), when they were near Madras, they observed smoke coming out of the hatches and they immediately proceeded to endeavour to ascertain the cause of it, and they found that the coals, though not in flame or even red hot, were very hot; they poured water on them, and got into harbour without much damage. I investigated that matter thoroughly myself, and I found the reason of the casualty to be what I should have suspected, namely, that water had got into the hold and the coals had become wet.

714. Water had got into the hold on the voyage?—Yes.

715. The coal was shipped dry?—Yes, in the usual way; I suppose it was perfectly dry when shipped.

716. Was the evidence conclusive that water had got in?—Perfectly conclusive.

717. Was there any iron pyrites in the coal?—It is very free from sulphur; it is one of the best coals in the Tyne. There are four or five of about equal value, but I believe it is the freest from sulphur of any one of them, or as free.

718. Passing to the other case, what was that?—The other case was the "Palmyra," which sailed from Liverpool on the 17th June 1874 with 1,206 tons fully loaded.

719. With what coals?—Pearson and Knowles's Wigan Hartley. On the 17th of September, after having had rather a rough passage, they saw smoke issuing from the hatchways; they took the main hatch off, and discovered that the smoke was coming rapidly up, and to all appearance the coal was on fire. They immediately got the donkey engine to work (according to the report which was vouched for in the usual way), and they poured many tons of water on the fire, and then strove to get the coals out. They got about 18 tons of coals out and mastered the fire.

720. What was supposed to be the cause of that?—In investigating that, I found likewise that they had got a lot of water in.

721. That was the captain's explanation, was it?—

He did not seem to want to explain it that way, but I found it out simply by putting certain questions to him and the mates. I felt perfectly sure that unless some unaccountable action had taken place it must have been from water getting in.

722. There was nothing peculiar about the state of the coal when shipped?—Nothing, I saw it shipped, it was shipped at a high level.

723. How many feet did it fall?—The high level is 30 or 40 feet above the quay side, and then they have a crane on the top; they take hold of the wagon and lower it down to about the level of the bulwarks of the ship, and then they take the end of the wagon out, and the coal goes into the hold; the first fall would be, I should think, probably about 26 feet.

724. Then in all your experiences those are the only three cases of coal taking fire spontaneously which you have been concerned with?—Yes, that is so.

725. How many cases of shipments of coal altogether have you been concerned with for the Government, and for the Madras company, speaking roughly?—Several thousands.

726. Therefore the per-centage of cases with which you are acquainted is very small indeed?—Very small indeed.

727. From your connexion with the Admiralty, do you know of any cases of spontaneous combustion in Her Majesty's ships, the coal for use in which you have been concerned in buying?—Only two cases, the "Rattlesnake" and the "Racoon."

728. Were those cases referred to you for report?—The papers came round to me in the usual way.

*729. What were those cases?—They were very slight, the firing took place in the bunkers. I have no hesitation in saying as I said on the minute, that those cases entirely arose from the heat of the boilers, and perhaps a little water had got in. I forgot the "Megæra," that was another case; that was a plain case where a steam pipe went through the coals.

730. That was a clear case of heating from external causes?—A clear case, and the others were I believe the same. A tremendous heat would be caused, and the slightest moisture would assist the action and cause the coal to break into flame.

731. You stated the method in which the coal was shipped in one particular case for the Madras company, are you well acquainted with the way in which coal is shipped at various ports?—I think I am acquainted with the mode in which coal is shipped in every port of Great Britain.

732. How many different ways of shipping coal are there?—A dozen, or more than that.

733. Will you describe the principal methods of shipment?—The best method of shipment of all is that which is adopted on the river Tyne, and at Blyth, and Sunderlands. There they have two methods. The best one of all I will describe first. The waggon comes on to the top of the drops, which are perhaps 40 or 50 feet above the water, the bottom of the waggon is loose, hanging on a hinge on one side, there being two hasps on the other. There is a long telescope spout, and at the bottom of the spout, or near the middle of it, there is a trap to hold a waggon of coals. When they knock out the bottom of the waggon the coals fall out of the waggon close on to the screen, perhaps within 18 inches of it. The coal falls and runs down the incline of the screen until it meets this trap. Then the ship being empty, or thereabouts, they have a large tub that will hold about two tons of coal, a large iron tub worked by a crane, either by steam, or by hydraulic pressure; they lower that down and put it under the spout. They can, if they like, make the spout reach down to the deck, or close on to the hatchway, but in this case they do not do that; they put the tub under the spout, open the trap, and fill the tub. The tub, when filled, is lowered right down to the bottom of the hold. It has a trap at the bottom, and the man who goes down with it just draws out a pin, and the bottom falls out and delivers its coal without any breakage.

734. That you consider the best method of shipping coals?—Yes.

735. Why?—Because it does not break the coals at all. The larger the coals the greater their value, and the less danger there is from fire. Then, when the coal comes up nearly to the top, they lower the spout down—I said before that the spout was a telescope,—and they run the lower part of the spout on to the deck; then they have a piece of sheet iron close to the deck, forming an acute angle, and the coals as they fall on this piece of sheet iron run down on each side, and the trimmers take them away on each side, right and left.

736. That, you consider is the best method of shipping coal, and spoils it the least?—I do.

737. Will you give us two or three other methods?—Then there is another method in use in those three places, which is by what they call swing drops. That method is adopted in the Tyne and Sunderland, but not in Blyth. They have a drop made with arms, on to which the waggon runs; and there is a counter-balance just to balance the weight of the waggon when the waggon was empty, so that when the coal was out of the waggon the counter-balance would bring the waggon up to bank again. They have a wheel and a break on it, and when a full waggon comes on, it goes down by its own gravity, and to hinder it from going down too quickly there is a man who has a break, and he applies the break sufficiently to let the waggon lower itself down gently till it gets down as far as the swing will allow it in its curve to go. Then, when it gets there, the same process takes place as in the case of the tub, they take out the pins and take out the bottom of the waggon, and the coal drops right into the hold, but it is close on the top.

738. That you consider a safe and effectual method of shipping coals?—I prefer the other, infinitely, but still it is the second-best method.

739. Will you give us one or two more?—In some places, in South Wales and in other places, of course they have nothing but the screen. They have a tip, but that tip is about 8 or 10 feet from the top of the deck; in some instances 6 feet, and all the coal must fall down that distance into the hold.

740. Is that the usual method adopted in South Wales?—Yes.

741. Do you consider that, with regard to the point which this Commission is specially looking into, it is safer and more advantageous to shoot coal according to the first method you have mentioned?—I do, and I will show you why I think so. Small coal falling into a ship becomes compact and dense, and therefore more capable of excluding anything like air. The more crevices that are left between the pieces of coal, the better it is, and the more opportunity there is for the air to circulate around the coal. Of course it is the want of air in my opinion that causes the combustion.

742. You consider that the better the coal is ventilated the less likely is combustion to take place?—Yes.

743. Will you give us some information also about the transport of the coal from the pit to the place of shipment. Has there been of late years much change in point of time in the transit from the pit to the place of shipment, or in any other respect?—Within the last 15 or 20 years there has been a great gain in the time of the transit from the colliery to the staith, but I am not quite so sure whether that has not been counterbalanced by the immense exportation. For instance, 20 or 25 years ago they were all small waggons that were used, what we call chaldron waggons, but of late years we have had 10-ton waggons. The larger the quantity in the waggon, the worse for the coal, in my opinion.

744. Do you think that the employment of large waggons increases the risk of combustion?—It breaks the coal more.

745. The coal is not now so long exposed to the air as it used to be?—Perhaps not.

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* *See* Admiralty Inquiry, page 62, which shows that Mr. McCulloch, in this, and in his next answer, was mistaken.

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746. In that respect is there a gain or a loss?—A gain, I think.

747. It is not so long exposed to moisture?—It is not so long exposed to moisture.

748. Going generally into the question referred to the Commission, what do you conceive to be the principal causes of spontaneous combustion of coal in coal-laden ships?—I look upon it that there is only one thing that can properly account for it. Very often I have seen coals put on board ship in a most lubberly way; that is to say, I have known the sides of the ship to be wet and damp, and sometimes I have even known water to be in the ship, quite apart from any moisture in the coals themselves, and my experience teaches me this, that if coals are even moist, much less damp, the bi-sulphide in the coals (because all coals have it, saving perhaps the hard coal of South Wales, the anthracite coal; except that coal I know of no coal that is free from sulphur) becomes, through its oxidization, turned into sulphide of iron by a natural process, and during that time, naturally, sufficient caloric is generated to cause combustion in the carbon of the coal.

749. Your opinion is that all spontaneous combustion of coal is caused by moisture, and that all coal is liable to spontaneous combustion?—More or less, except anthracite.

750. You do not believe that perfectly dry coal ever takes fire spontaneously?—I do not believe it.

751. Do you think that the number of cases of spontaneous combustion has been greater in the last two years than it was before?—Yes, I believe so.

752. The only two cases you have mentioned as having occurred within your own experience were last year?—They were last year.

753. How do you account for that?—It is partly to be accounted for, I think, by the immense increase in our exports. They have increased marvellously, and of course as the number of ships increase, the number of cases of spontaneous combustion would be likely to increase in the same ratio; but my impression is that there is a great deal of carelessness in the shipping of coal, and that there is no means of ventilation.

754. Do you think that there has been greater carelessness of late years?—I believe so.

755. Have you any evidence of that?—It is only an impression; more ships have taken fire within the last 10 years than perhaps in the century before.

756. You say that there has been greater carelessness, and also less attention to ventilation; do you believe that in a given number of ships there are fewer that are well ventilated than there used to be?—I do not think there are any ships that are ventilated; or if they are, they are so badly ventilated that the ventilation is not worth speaking of.

757. Your impression is that coal-laden ships are not, as a rule, well ventilated?—I think they are not well ventilated. I may say that they are not ventilated at all.

758. Do you say that absolutely as to all of them, or as to the great majority?—The great majority of them come under my own observation, and I have also seen many that I have had nothing to do with. I happened to be in Liverpool some two or three years ago, and I was talking to a shipowner who was there, and expressing my views as to ventilation. He misunderstood what I said to him on the matter evidently, because he went and had his ship ventilated, but instead of making the uprights as I advised him, and the trunk as well, he bored holes in his ventilator, and when the coals went in, the small coals went into the holes, and stopped all the ventilation.

759. What in your opinion are the precautions which should be taken, and which are not taken now, to prevent the spontaneous combustion of coals in coal-laden ships?—I think the remedy is very simple. From the deck I would put in a ventilator, of a size varying according to the size of the ship, in the forward part of the ship, so as always to be open to the breeze. I would carry that down into a large horizontal ventilator below, that would run the whole length of the ship; and at intervals, according to the size of the ship, I would place upright shafts 8 or 10 inches square, with louvres, so that, supposing they were 20 feet apart, if the coals were to heat even from damp of course the heat would always ascend, and it would come into these louvres because they would be all open; it would come right up through the body of the coal, and it would escape, and so keep the coal comparatively cool. This would go through the hatchways into the open air. That would not cost much, and any impression is that it would be a perfect preventative of heating.

760. You have stated already that in respect of ventilation coal-laden ships are deficient; would you impose upon them the necessity of adopting some such system as you recommend, by law?—Yes, and it would save their pockets. Whether the ventilation is made of wood or iron, I do not care. Some may fancy that to make the ventilation of wood would be adding fuel to flame, but I do not think so, because I believe it would keep the ship perfectly free from danger.

761. Have you any suggestions to make as to the best method of extinguishing fire on board coal-laden ships?—I see no other method but the old-fashioned one of water. If you could hermetically seal the bulkheads and the hatchways, we could find a very speedy way of extinguishing it with carbonic acid, but I think it is impossible practically to do so.

762. You mean with the extincteur?—Yes.

763. (*Mr. Vivian.*) What is the form of the waggons which are used on the Tyne, are they not conical at the bottom?—No, as a rule the bottoms are flat.

764. Do not the sides slope to the centre?—Yes.

765. And the ends slope to the centre?—Yes.

766. So that they are conical?—If you turn them upside down they are. When I speak of a conical waggon I mean a waggon in which they have a cone in the bottom.

767. I mean the sides and the ends slope towards the centre?—That is so as regards some of the waggons.

768. All the waggons are so constructed, are they not?—Not half of them.

769. Then how can the bottom clear the axle-trees?—It is in between them.

770. There are two classes of waggons on the Tyne, namely, end-board waggons and bottom-board waggons?—Yes.

771. I understood you to speak entirely of bottom-board waggons?—No; I said they tipped the waggons up at the end as well.

772. I understood that to refer to another species of tipping. Speaking of the Tyne mode of tipping, which you say you believe to be the best, the ends and sides of the waggons fall towards the centre?—Yes.

773. Then those waggons may be called inverted cones?—Yes.

774. You are acquainted with the South Wales coals, no doubt; is it possible to ship South Wales coals with waggons of that character?—Yes.

775. Have you ever seen it done?—No, because there are no waggons of that class in South Wales.

776. The blocks of South Wales coals are very large, are they not?—Yes.

777. Can you state, of your own knowledge, that they would pass through the bottom of those conical waggons?—I think they would.

778. Have you ever known them do so?—No, because there are no conical waggons in South Wales. But I have seen this done; I have seen large blocks of splint coal come out of those waggons, some of them 2 feet long by 15 or 18 inches thick, readily. A piece of coal may stick a little, but the men are prepared with what they call a prod, and they ease the piece of coal, and away it comes.

779. You have no practical experience leading you to say that it is practicable to ship South Wales coals in those conical waggons?—No, because I never saw a conical waggon in South Wales.

780. With regard to swing drops. You have seen

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them only on the Tyne, but are you aware that there are swing drops also in use in South Wales ?—There are some at Swansea now.

781. And have been for a good many years ?—Yes, at Swansea.

782. There are none at Cardiff ?—I think none.

783. At Cardiff end-board waggons are used entirely ?—They are all end-board waggons there.

784. The system is to drop the end of the waggon and let it discharge itself on to the shoot, is it not ?—Yes.

785. Then in what does that differ from the Newcastle system ?—Only in the delivery from the shoot into the hold.

786. In what does it differ from the swing-drop system that you said you have seen in use at Sunderland ?—Somehow or other they do not bring their swing drops in South Wales sufficiently low down.

787. Do not they bring them simply to that angle at which the coal will run down ?—The waggons are not used in this way in the swing drops that you are speaking about.

788. In what way does the swing drop of the Tyne and the Wear differ from the lowering drop used at Cardiff and Swansea ?—The one goes so much lower on to the deck than the other.

789. The end of the shoot goes equally on to the deck in both cases, does it not ?—In the swing drop there is no shoot used at all.

790. Then how do you double screen into the ship ?—I think that is obsolete ; at least, I hope it is. It is obsolete as far as the Admiralty is concerned. I tried to stop that waste of coal (beside the cost of 1*s.* more per ton) very soon after I took office.

791. You disapprove of the system of double screening ?—Decidedly ; it makes the coal worse, and is expensive.

792. The system adopted at Cardiff and other South Wales ports is to have the waggon and the shoot only at such an angle as to enable the coal to run, is it not ?—That is the principal way.

793. So that it is subjected to no more pressure than is necessary to cause it to run ?—Just so.

794. Is there not a check-board across the shoot to prevent its running down too quickly ?—Yes, there is a trap.

795. Are you acquainted with the system of shipping coal at Swansea ?—Yes.

796. Do you know the box system ?—Yes.

797. You did not mention that ?—No ; because the box system is like the tub system, only more clumsy.

798. That is an entirley new system, adopted by the Great Western Railway, and worked by hydraulic power arranged by Sir William Armstrong ?—Yes.

799. Those boxes lower the coal through the hatchways into the hold of ship ?—Some of them do.

800. The bulk of them do ?—Yes.

801. In what way is the Newcastle system better than that system ?—In the first place, the tub in the one case holds so much more than the box ; in the next place, the system is adopted at Swansea to a very limited extent only.

802. You said that the tub held two tons ?—Two or three, or four tons.

803. The limit is the size of the hatchway ?—Yes.

804. The South Wales tubs are as large as the hatchway will take, are they not ?—They are like boxes. The Newcastle tubs are iron tubs.

805. The Swansea box would hold two and a half tons, would they not ?—Yes.

806. That box is lowered through the hatchway into the hold ?—Yes.

807. And the bottom of the box is struck out ?—Yes.

808. Is not that a good system ?—It is a good system, but it is only adopted to a limited extent.

809. Is not there this advantage in that system over the Newcastle system, that it saves one breakage of the coal ? In the Newcastle system the coal has to be discharged from the truck into the shoot, and then from the shoot into the tub ; whereas, in the South Wales system, the coal comes direct from the colliery in the box, and that box is lowered into the hold of the ship, so that there is one breakage less ?—If you look at the coal which is shipped by that method, you will find that it is the soft coals ; none of the best steam coals are shipped from South Wales in that way.

810. Was not the whole of the coal on the Vale of Neath system worked in that way ?—No ; at all events it is not so now ; the tubs are in existence, but they are principally used for this soft coal.

811. Do you find generally that the coal shipped on the Tyne is discharged in better condition than the coal shipped in South Wales ?—No, I would not say that, for the simple reason that the best steam coals in South Wales are so large that they come out larger than north country coals ; but taking the coal all round, they come out with a less per-centage of small in the north country than in South Wales.

812. Do you think it would be desirable that the system of shipping coals adopted in the north of England should be introduced into South Wales ports ?—I do.

813. Do you know the system adopted at the Alexandra Docks at Newport ?—I do not know it. I saw the docks opened, but I did not go round to look at the drops. I believe they are on the same principle as the drops in the Tyne and the Wear.

814. The system adopted at the Alexandra Docks is considered to be the very best system of shipping coals that can be adopted, is it not ?—I believe it is precisely the same in principle as that adopted in the Tyne.

815. You approve of the plan of lowering a tub containing the coal down through the hatchways ?—Yes.

816. Till a column is formed ?—Yes.

817. And then lowering the waggon down to the hatchways and discharging it as gently as possible ?—Yes ? the tub will lower the coal till it comes right up to the hatches, and there will be sufficient room on each side for the coals discharged on to the piece of sheet iron to slip away to each end where the trimmers can get hold of them.

818. You said that all coal contains iron pyrites. Some coal is what they call white ash coal, is it not ?—All the best bituminous steam coal is white ash coal.

819. Is it possible that that can contain iron pyrites ?—In my opinion it does.

820. Supposing it to contain iron pyrites, what becomes of the iron when burnt ?—I daresay chemists would find it somewhere. I am not a chemist, but I have seen certain things take place, and I have drawn my own conclusions from them. It may not be in the form of iron pyrites, it may be in the form of sulphur. Iron pyrites may exist in the coal, and not be in the form of brass. Brass is usually the form of pyrites in North and South Wales, but there is sulphur in the coal, though there may not be the brassy appearance.

821. It must be in the condition of pyrites, must it not ?—Of course it would be pyrites, but it would be in such a form that perhaps you would not find it. I can only say that the best steam coals in the north country go to white ash, and that I can positively say, because I have stood by a chemist when he has been analysing them. I can positively say that there was iron pyrites in those coals, and yet I could not distinguish it. A chemist might distinguish the presence of a small quantity of iron pyrites, though I could not.

822. Iron pyrites when burnt must produce oxide of iron in the first instance which would produce a red ash ?—Suppose you had ·50 or ·60 of iron pyrites, how would you distinguish it ?

823. The quantity is so infinitesimally small, that it may be disregarded ?—No. Though the quantity of it in the coal may be so small, if the coal is made wet it will do its work. I would not buy a single coal if I had my own way that contained over 1 per cent. of iron pyrites, that is to say, for certain purposes.

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According to our investigations we have never found in the best Welsh coals more than 1·20 to 1·50.

824. Is that sulphur or iron pyrites?—Iron pyrites. It is the same thing.

825. Iron pyrites is sulphur combined with iron, and sulphur is sulphur alone, is it not?—But they are in close brotherhood.

826. You consider that all coal contains iron pyrites?—Yes.

827. And that all coal is liable to spontaneous combustion if it is wetted?—I think so, save and except anthracite coal.

828. If anthracite coal contained iron pyrites, would not that be liable to ignite spontaneously?—Some of it does not contain any, some of it is nearly pure; and, besides, it is like stone.

829. Are you aware that anthracite comes from the same veins as the steam coal, only more to the westward?—I am perfectly aware of that, but there is a distinct difference in the strata.

830. In what respect is there a difference in the strata?—You will find a great difference between the steam coal and the anthracite. As you go north from the Aberdare Valley the coal becomes more and more anthracitical all the way up till you get right over the mountain.

831. In what way do the strata differ?—I mean the superincumbent strata. If you were sinking a pit higher up, you would find the strata different to what you would if you were sinking to the 4-feet seam lower down the valley.

832. (*Mr. Cory.*) I understood you to say that the Cowpen Hartley was more free from sulphur than any of the other Tyne coal?—I said three or four of the best coals were about on a par.

833. You did not intend to say that Cowpen was more free from sulphur than any of the rest?—No. I qualified that by saying there were three or four equally good.

834. Did you ever know of any ship ventilated on your plan?—One has gone out to Madras from Liverpool a little while ago ventilated in that way.

835. Exactly upon your plan?—Not exactly, but an imitation of it. It was not exactly carried out as I should have liked to see it done.

836. Is she a coal-laden ship?—Entirely a coal-laden ship.

837. Is that the first ship you know of that has adopted your plan?—The first I know of.

838. What is the name of that ship?—The "Maori." I may say that this ship sprang a leak after being about a fortnight out, and they had to come back again and discharge her coals, and reload fresh coals.

839. (*Mr. Icely.*) What form of iron pyrites distributed through coal do you consider to have the most dangerous tendency?—I do not care whether it is iron pyrites or sulphur; if there is any difference, in my opinion they both tend to the same thing.

840. There are some varieties of coal which you consider especially dangerous?—Yes, there are some coals very dangerous.

841. And you reject those in your selection of coals for the navy?—Yes.

842. Could you name any of them?—I should not like to name them.

843. After you have made a purchase of coal, is not there a coal viewer at the ports to inspect the coal?—Yes, a coal inspector at each of the ports.

844. What are the instructions which the Government give him in inspecting coals?—He is particularly ordered, first, to watch the proportion of small; secondly, to see that they are kept clear of slate and stones and other impurities, and thirdly, to see that all the brasses are picked out if possible.

845. If, for instance, he observes that the coal is dirty and small, has he the power to reject it?—Yes.

846. Or to prevent the shipment of that cargo?—At once. In all our tenders we make that a *sine quâ non*.

847. Do you attribute to this careful inspection the comparative safety with which cargoes of coal for Admiralty use have been carried?—I have no doubt a great deal of it is owing to that.

848. (*Mr. Young.*) What kind of coal was it that was shipped in the "Numea" in May 1874, that took fire?—Cowpen Hartley.

849. You said you investigated the circumstances both of that case and of the "Palmyra." What do you mean by investigation?—I made inquiries of the captain and the crew by their report.

850. After they came home?—Yes.

851. You read the protests and saw the captain and the crew afterwards?—I read all the protests and had to make out a regular statement for the underwriters.

852. And you saw the captain and crew after they came home?—I saw the captain and crew's reports. Perhaps I may here refer to a question which the underwriters raised. They got into their heads that the cause of the fires being now so numerous was that the coals were shipped direct from the pit into the ships, whereas formerly they used to be laid in heaps. Some of Lloyd's people asked me whether I did not attribute it to that cause, and I said it was a perfect fallacy. I said I would look upon dry coals shipped direct from the pit as being infinitely more safe than coals that have been lying in a heap; and moreover they would become completely valueless by being exposed a long time to the wind and the rain and the air, because their virtue would be gone to a great extent.

853. Have you known cases of spontaneous combustion of coals stored on shore?—Many cases.

854. More frequently than on board ship?—More frequently than on board ship.

855. You said that want of air causes spontaneous combustion?—Want of proper ventilation, not want of air exactly.

856. Spontaneous combustion could not take place without air?—There is always sufficient air in a ship; you cannot hermetically seal a ship.

857. (*Mr. Abel.*) You have given us only three cases of spontaneous combustion on board ship; those are the only cases that have come to your knowledge in all your experience?—Those are the only cases that have come directly under my own cognizance.

858. May we take it that these are really the only three cases that have occurred in all the shipments of coal with which you have been concerned?—You may.

859. Would you necessarily hear of every case of spontaneous heating of coal on board any ship carrying coal under contracts?—Certainly.

860. How would it come to your knowledge?—It would come to my knowledge by a complaint being made. If coals get heated they become discoloured, and though they may not be very hot, yet they are so discoloured that when they are inspected at the dockyard, or wherever they are landed, they are reported on; and those reports have always come before me for my opinion, that is as far as the Admiralty is concerned.

861. Therefore any case in which any real damage was done would come to your knowledge?—Yes, and the same as regards shipments for the Madras railway.

862. Supposing a case of heating occurred, but no damage was reported as having been caused to the cargo, you would not hear of it?—No, we have such a system that they are not apt to report cases of heating of coal of trivial importance, because coal cannot become heated without destroying a portion of it.

863. Those three are the three important cases of heating on board ship that have come to your knowledge?—Yes.

864. Personally, you have no positive knowledge of the cargoes of coal that those ships took out?—We have an inspector at each part who inspects the coal. I do not go to inspect each cargo. I know what kind of coals we purchase.

865. As regards one of the three cases which you

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mentioned, I understood you to say that, speaking from your own knowledge, the coal was practically free from pyrites?—I have just told Mr. Vivian that I believe in all coal there is pyrites sufficient to cause spontaneous combustion under certain circumstances.

866. I think in the earlier part of your evidence you stated, with reference to one particular cargo, that to your knowledge or belief there was no pyrites in that particular class of coal with which the ship was laden?—I think I said that the particular coal I referred to was as free from pyrites as any coal.

867. Your view with regard to spontaneous combustion of coal on board ship is that it is entirely due to coal getting wet?—Entirely.

868. You attribute it to no other cause?—There might be another cause—red hot ashes put on it might cause it to ignite.

869. You think every coal is liable to spontaneous combustion?—Every coal except anthracite.

870. And you consider the only point to be guarded against is to keep water from the coal?—I do.

871. You spoke of the explanation the captain gave with regard to one of those cases of spontaneous combustion with which you did not agree; what was the captain's explanation?—I did not mean that the captain attributed the casualty to anything but water getting in. He said he was perfectly sure no moisture had got in amongst the coals unless, as he said, it had got in through the crevices of the deck or somewhere else in such a proportion that it could not do any harm. He wanted to show that the quantity of water had been so small that it could not do any harm. If what he had stated was true it could not have done any harm. No doubt a very small quantity on the top of the coals would not do much harm, but there was more than that; and in fact he admitted afterwards, in his report, that in a storm no doubt water got down somewhere or other, through the scuttles, or somewhere.

872. Do you think it is possible to prevent coal containing water when placed on board ship, even if placed there directly it leaves the pit's mouth?—I believe chemically all coal contains a certain quantity of water, but I do not believe there is sufficient water in coal internally to make it at all liable to spontaneous combustion.

873. You said that the larger the waggon the worse for the coal; will you explain what you meant by that?—Simply because one waggon will contain 2 tons 13 cwt. and the other will contain 10 tons. If a heavy shower of rain were to come on to those coals contained in the 10-ton waggon there would be a larger surface of coal exposed than in the other case, and if the coal were to be exposed long to the air it would destroy the coal. If you expose coal to wind and rain you destroy its power in a great measure. That was what I meant, that the waggon being larger, more surface was exposed.

874. (*Mr. Ferguson.*) Seeing that you attribute spontaneous combustion to water mixing with the coal, is the system of ventilation which you propose solely to counteract the dampness of the coals?—It would carry off the heated atmosphere from whatever cause.

875. I understood you to say that spontaneous combustion was only caused by the coals being damp?—That is my opinion.

876. Therefore your system of ventilation would be to counteract that?—Certainly.

877. In the many thousands of tons of coals that you have shipped, have all the vessels which have carried those coals without damage been ventilated in that way?—Not that I know of.

878. Have they been ventilated at all?—Not that I know of.

879. With regard to the vessels in which you say the heating was caused by water getting in, did that water come from above or below?—It came from above, I believe, in both cases.

880. From the quantity of coal you ship for abroad you must be aware, I suppose, of the extent to which the coal depreciates after being carried a few months in the ship's hold; if 10 tons of coal will do a certain amount of work here, will it do the same amount of work when it arrives at its destination?—You must take a little per-centage off for the breakage, but apart from that it really suffers very little harm, it does the same work and evaporates the same amount of water. If you stack it in the sun and rain for a month you will take away 10 per cent. of its value at once.

881. Do not you find that the gas accumulates in the ship's hold during the passage, and does not the coal thereby lose some of its combustive power?—No; I think the best steam coal retains its gas in its integrity.

882. Is it not the fact that coals lose their combustive property to a great extent in the course of the voyage?—Very little. There is no way of escape for it.

883. (*Mr. Duncan.*) You have described a method of ventilation which you think would make ships perfectly safe from spontaneous combustion?—I think so.

884. In describing that mode of ventilation I understood you to say that you would have a horizontal shaft in the centre of the ship?—Yes.

885. About what size?—According to the size of the ship.

886. What would be the size of the shaft for a ship 1,000 tons register?—I should say a couple of feet square.

887. You would have that go right along the centre of the ship?—Right along the centre of the ship.

888. Not quite in the centre, because you would have to keep clear of the iron stanchions that go up and down?—You could not lay it along the keelson, but I should prefer it being about 3 feet from the bottom, just laid along the whole length of the ship; then I would have a connexion at each end with the deck, so that the air might come down. There would be a funnel forward going up above the deck as they have for ventilating the cabins and holds of ships, so that it would catch the air and draw it into this shaft; by that means there would be a continual current of air. I would have upright shafts at certain distances made with louvres, so that the air would pass along each one of those continuously. Then if there were any heat it would pass up through those louvres into the open air.

889. You would carry these louvres 4 or 5 feet above the deck, so that in all weather they would be open?—Yes; but loaded so that water could not get admission.

890. Would it alter your judgment in reference to the safety of your plan if you were told that it had been stated to this Commission that spontaneous combustion had taken place in ships ventilated in that way?—It would not at all, because I know that ships that have taken fire have not been ventilated.

The witness withdrew.

Mr. Samuel E. Derry examined.

Mr. S. E. Derry.

891. (*Chairman.*) You were, I think, from 1859 to 1868, inspector of coal shipped for the Admiralty in South Wales?—Yes.

892. What was your district?—All the ports of South Wales.

893. You are now an engineer in the service?—I am a retired chief engineer in the Navy.

894. During that period, from 1859 to 1868, you had some experience of cases of spontaneous combustion?—Three cases; they were all one description of coal.

895. What was the coal?—Parsons Abbey Graigola.

896. All the three cases of spontaneous combustion

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which came within your knowledge were of that one description of coal?—Yes.

897. Will you give to the Commission some particulars of those cases?—In 1864 a complaint came officially before me with regard to coal having caught fire at Jamaica and Barbadoes, and also in a vessel arriving while they were extinguishing the fire at Barbadoes with the same coal on fire on board of her.

898. Were those cases all in the same year?—Yes, in 1864.

899. How long had the ships which carried that coal been out?—I should think about six weeks.

900. Those were the only cases which came under your knowledge in your 10 years' experience?—Those were the only cases of spontaneous ignition that came under my official knowledge during the whole of that time; but I think the same coal caught fire in Keyham Yard at the same time.

901. That was not within your official knowledge?—No.

902. What is the character of that coal?—It is a smokeless coal from Llansamlet, not produced in the regular steam coal district.

903. Was it shipped under any particular circumstances?—Not at all.

904. Was it shipped in fine weather?—I do not think the weather would have anything to do with it. I think the same coal would catch fire under the same conditions if sent out again.

905. What is the chemical character of the coal; does it contain a large proportion of iron pyrites?—It is not that the iron pyrites is in a large proportion, but it is so intimately mixed with the coal as to be in a most favourable condition for catching fire. If you took a piece of coal from that vein you could see that the coal and the iron pyrites were intimately mixed, perhaps there would be an eighth of an inch of iron pyrites and an eighth of an inch of coal, and so on, as if laid alternately on purpose to facilitate its catching fire. In other steam coals there is plenty of iron pyrites, but if it is localised in large masses it is not so likely to take fire as where distributed in the manner I have mentioned.

906. Was this coal shipped smaller than the average of South Wales steam coal?—It naturally works smaller than the average of South Wales steam coal.

907. Otherwise there was nothing peculiar either as regards the length of time that it was exposed or as regards moisture, which it was subjected to?—Not that I can remember.

908. Nor was there any report made showing that during the voyage anything occurred which might have pointed to the cause of the coal catching fire?—No; the fact of the coal catching fire in so many different directions at the same time would appear to indicate that it was in the nature of the coal itself and not in any particular circumstances connected with it.

909. Did you in the course of your official duty lay down any particular regulations in connexion with the shipment of coal?—I did. I put the conditions for the shipment of coal for the Admiralty in South Wales into order. The conditions I laid down were that the coal should be free from small at the time of shipment, but of course one necessary condition is that the coal must be clean, that is to say, free from large quantities of iron pyrites. You cannot get coal perfectly free from sulphur, but it must be free from large quantities, and it is required to be free from small coal. Those were the stipulations that I had to see fulfilled.

910. Was there any stipulation in the contract as to the ventilation of the ship?—None; it did not become necessary for the Admiralty to look into such regulations as that, for if the coals were not supplied at the port, the Admiralty did not pay for them.

911. They were paid for on delivery?—Yes.

912. Still the Admiralty were interested in the coal that they required for a particular station not disappearing on the way?—So far as keeping up the supply went they were interested.

913. When those casualties took place was the matter referred to you?—It was.

914. In each case you satisfied yourself that it was the fault of the coal?—Yes.

915. And not the fault of the ships?—Not in any way.

916. (*Mr. Vivian.*) How much of this Parson's Abbey Graigola coal did you ship?—2,000 tons I should think, at least that?

917. It fired in three cases?—Yes, at the stores in the dockyard at Jamaica; at the stores in the commissariat department at Barbadoes, and also in a vessel arriving at Barbadoes while the coal was on fire on shore with the coals on board on fire.

918. What quantity out of that 2,000 tons would that represent?—Perhaps nearly half the quantity.

919. There are considerable quantities of that coal shipped are there not?—They have one particular vein, the coals from which are liable to take fire spontaneously.

920. What is that?—The Tyr Edmond. When they do not ship the Tyr Edmond the coal is free from danger, when they do I think it is certain to ignite.

921. Had the coal been long on the bank before being shipped?—No, it never is.

922. Was it wet or dry?—I cannot remember whether it was rainy weather or not.

923. That is the only South Wales coal which in all your experience you have found liable to spontaneous combustion?—Yes; it depends on the condition of the coal. In the case of all coal where the coal is small the particles of sulphur get more intimately blended with the coal, and in this particular coal ignites spontaneously; but I never heard of good marketable South Wales steam coal catching fire spontaneously in the main body of the coal?

924. How much coal did you ship altogether during the 10 years you were in South Wales?—It must have been millions of tons. I shipped 27,000 tons in one month.

925. Out of the millions of tons you shipped only those three cases of spontaneous combustion occurred?—That is all.

926. Did you ship any patent fuel?—Yes.

927. Have you known a case of spontaneous combustion in patent fuel?—Never.

928. (*Mr. Icely.*) Have you ever inspected jettisoned cargoes in cases where spontaneous combustion has occurred in other vessels not connected with the Admiralty?—Never. I never knew South Wales coal catch fire except in those three instances to which I have referred.

929. Did you never know of any other case of heating in South Wales coal?—No. I have known of explosions of gas in vessels, not spontaneous ignition.

930. (*Mr. Young.*) Are those explosions noted? Do they come under your notice?—No; those explosions have not been in the case of any cargo that I have had officially to do with, but when they improperly put on the hatches before the cargo has had time to give off its gas, an explosion takes place now and then; the gas ignites.

931. When a light is put to it?—Yes. It is not uncommon for a vessel to come back to Cardiff after it has got out into the Bristol Channel.

932. On account of damage having been done by explosion?—Yes, the coal gives off gas.

933. Does that refer to all kinds of South Wales coal?—I should think so. You may see the coals giving off the gas in the pits.

934. You think it is a very important thing to keep the hatches off in all vessels?—It is a standing order in the docks, but which is disobeyed very frequently.

935. Do you think ventilation would do away with that danger of explosion?—I think ventilation is absolutely necessary.

936. You have mentioned three cases in which this coal took fire. Did you ship any more of that same coal after that?—No.

937. You ceased shipping that coal at once?—Yes.

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938. Have you had any remonstrances from the colliery owners for tabooing them in this way?—The colliery owners were only too glad to be quiet under the circumstances.

939. (*Dr. Percy.*) Could you give us a specimen of this coal?—No, I cannot furnish the Commissioners with a specimen.

940. (*Chairman.*) Is the seam still worked?—I do not know.

941. (*Dr. Percy.*) I think you said that the danger of spontaneous combustion was lessened by the small coal being separated from the large coal?—Yes.

942. If so, can you assign any reason for that?—I have tried to assign a reason for the fact that the presence of small coal is liable to assist spontaneous ignition, and I think it is because in the small particles of the coal the sulphur and the coal become more intimately mixed together, and then the coal is in a more favourable condition for catching fire than when the pyrites is in large masses.

943. Do you know whether any investigation has been made by the direction of the Admiralty with regard to the proportion of iron pyrites in small coal as compared with large coal?—I am not aware of any.

944. You spoke of the mode in which the iron pyrites was intermixed with this coal. You spoke of the mixture as being intimate?—Yes, there would be a thin layer of coal and a thin layer of pyrites.

945. Would you call that an intimate mixture?—Yes.

946. Is not there plenty of coal in which the iron pyrites is diffused so minutely that you cannot see it?—Yes, and it is not dangerous then, I should think.

947. What was the thickness of the layer of iron pyrites which you saw?—The layers varied. Perhaps on an average they would be about an eighth of an inch.

948. Was the layer pretty continuous throughout?—It was not straight.

949. Was it all through the coal; did all the lumps contain it more or less?—Yes. I think it was pretty well distributed throughout the whole of the coal of that vein. There were coals of two veins in the cargo; but in the case of that vein I think the iron pyrites was pretty well distributed throughout.

950. What examination was made by the Admiralty of that coal before it was ordered?—Only the examination I made of it at the time of shipment.

951. Simply an inspection?—Yes.

952. Was any test taken of its heating power?—Yes.

953. Was there any other examination made of it?—No. In fact any trials of the coals would be to a certain extent fallacious; for supposing the cargo to consist of coals from two veins, it is easy to send the best for trial, and to send the coal from the other vein for shipment.

954. Is any examination made on the part of the Admiralty with the view of ascertaining whether coal which is being shipped for the use of the navy contains iron pyrites?—In all cases before coal was admitted for Admiralty use it was sent to the dockyard for trial.

955. In the particular case of which you have been speaking, was the proportion of pyrites contained in the coal directly ascertained or not?—Not at all. I think it exceedingly probable that no coal containing pyrites was sent for trial at all.

956. Is any examination made of coal at the Admiralty with respect to the proportion of pyrites contained therein?—Now I have nothing to do with it; I have ceased to have anything to do with it for five years.

957. In your time was it the practice to have an examination made with a view of ascertaining the proportion of iron pyrites contained in coal?—I merely went on my own judgment by inspection.

958. (*Mr. Duncan.*) In your judgment the circumstance of the coals getting wet in the wagons between the time of their leaving the pit's mouth and of their being put on board ship was not a matter of serious importance?—I think it would assist to set up spontaneous ignition if the coal was shipped wet.

959. Are you able to explain at all what the action of wet upon coal is?—I am not a chemist. I only know that it is desirable to avoid shipping coals containing an excessive amount of sulphur.

960. Have you any reason for thinking that the application of water to coal is likely to induce spontaneous combustion?—No; I only know that it is the generally received opinion that moisture superinduces heat.

961. You have no practical experience in that direction?—I have not.

962. (*Chairman.*) Is it your impression that coal containing iron pyrites distributed in the manner you have described will burn spontaneously without moisture, or is it your impression that moisture is a necessary element?—Supposing the iron pyrites to be so mixed in the coal as to cause the coal to be in a state favourable for spontaneous combustion, then I think that either damp or friction will cause spontaneous ignition. I think friction might cause it in the working of the vessel at sea in the same way as striking a lucifer match.

963. (*Mr. Duncan.*) From your observation of the lumps of coal when they have been coming down for shipment, is it your opinion or not that the pyrites is lying on the surface of the pieces rather than in the centre of them?—No; I do not think that the iron pyrites is present in one part of the coal more than in the other. I think coal brought from any particular part of a seam is equally liable to contain iron pyrites as coal brought from any other part of the seam. I have noticed pieces of coal which bore signs of having been wrought from the bottom or the top of the seams, but I do not think that they gave any particular indications of sulphur.

964. You do not think that the dust coming off the top and the bottom of the seam would contain a larger proportion of iron pyrites?—I do not suppose my opinion upon the point is of much value; but I think the pyrites is distributed through the whole of a seam, here, there, and everywhere. I think there is no rule for it.

965. You do not think that there is more pyrites among the small than among the large coal?—No, not ton for ton.

966. (*Mr. Vivian.*) I think you said that the small of the steam coal in your judgment was liable to spontaneous combustion?—More so than the large.

967. You have been at Aberdare?—Yes.

968. Formerly large heaps of small coal used to lie at the pit's mouth, where it was subjected to the very best conditions for catching fire, the small coal being exposed to the weather for many years?—Yes, for 10 or 12 years.

969. So that if the small coal were liable to spontaneous combustion it surely would have taken fire in that case?—It is not generally liable to fire; still, I have seen it on fire, and at the docks too.

970. Do you mean small coal?—Yes.

971. Are there any large heaps of small coal at the docks?—There used to be large accumulations in my time. When they began to screen coals they had a large quantity of small after screening, and it lay there perhaps some months.

972. You have seen that in some cases on fire?—Yes, and in the collieries in the Aberdare valley I have seen small heaps on fire.

The witness withdrew.

Mr. Orestes Brooker and Mr. James Harwood examined.

Mr. O. Brooker and Mr. J. Harwood.

973. (*Chairman to Mr. Brooker.*) You are now employed at Portsmouth?—I am.

974. (*To Mr. Harwood.*) So are you, I believe?—Yes.

975. (*To Mr. Brooker.*) You were formerly inspector of coal for the Admiralty in the Northumberland and Durham district, were you not?—Yes.

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976. How many years were you inspector there?—Nearly four years.

977. Do you remember how much coal passed under your eye during the time you shipped for the Admiralty?—I suppose about 50,000 tons.

978. During that time, were there any cases of spontaneous combustion of the coal so shipped?—Not at the port.

979. But after the ship left port?—Yes, I heard two years afterwards that spontaneous combustion had taken place in one case; I fancy it was at Jamaica.

980. In one single case?—Yes.

981. You only know of one case of spontaneous combustion in all the coal shipped from the north during those four years from 1864 to 1868?—One case only of government coal.

982. Were the particulars of that case referred to you for report?—Yes, and on examining my book I found I had inspected but very little of the cargo in consequence of my having other ships to inspect.

983. Do you remember what coal it was?—No, but I think it was shipped in 1865.

984. You ceased to hold that office in 1868?—Yes, early in 1868.

985. Does your recollection go to any other circumstance connected with that case of spontaneous combustion?—No, only that the report from that station was that the bulk of the cargo was small and contained pyrites.

986. All that you remember about it is that it was at Jamaica?—I think it was at Jamaica. I know it was at some hot station.

987. Beyond that, what the coal was, or how it was shipped, or as to any of the circumstances of the casualty, you have no recollection?—No, I was not present when it was shipped.

988. Were your instructions to avoid any particular classes of coal as liable to spontaneous combustion?—I had no instructions to that effect; my instructions were to receive what was considered first class coal.

989. The question of the liability of coal to spontaneous combustion was not one which came prominently under your notice?—No.

990. Was not it included in the Admiralty instructions at all?—No, at least I never received any instructions upon spontaneous combustion.

991. So far as the shipment of coal under your responsibility is concerned you cannot give the Commission much information as regards spontaneous combustion?—I objected, and complained if brassy coal were permitted to pass into the waggons or ships. I had a suspicion that if I were not present that this coal would be passed; that they would not be so particular about it.

992. You were specially there to prevent coal with sulphur in it from being shipped?—My instructions were that the coal were to be fresh raised and properly screened from dust. I occasionally went to the pits to see if they were so; the people at the colliery not being aware of my intended visit. When I found that in the case of this particular cargo there had been spontaneous combustion, I was inclined to think that it was in consequence of my not being there to inspect

993. That is to say that they had taken advantage of your being overworked to ship a considerable quantity of brassy coal?—That was a thing I could not prove.

994. Was that the only cargo you had not the opportunity of inspecting?—Yes; there would be several cargoes that I could only inspect one-third or perhaps two-thirds of, but I do not know there was any other entire cargo, that I did not inspect.

995. Were you supposed to see every ton of coal that went into the ship's hold?—If I possibly could.

996. We understand that you and Mr. Harwood can give us some information as to the storage of coal at Portsmouth. How is coal stored there?—The coals are stacked into heaps about the dockyard and exposed to all weathers; in other places they are under cover.

997. Is there any coal afloat also?—Yes, there is coal afloat in the hulks.

998. What sort of quantity is usually exposed to the weather on shore?—Probably 7 or 8 thousand tons; at the present time there is not so much, but a year ago there was a very large quantity in the dockyard.

999. How much coal do the coal hulks hold when full?—I think they hold about 2,000 tons each.

1000. Have you had any cases of coal heating in the dockyard or in the hulks?—No, I never heard of any.

1001. Though a large quantity of coal is constantly exposed to the weather?—Yes.

1002. Is there much small in the coal?—There is a very large quantity sometimes.

1003. And you have never had a case of heating?—I have never heard of any case of heating.

1004. (*To Mr. Harwood.*) Have you ever had any cases?—I have had one case of heating.

1005. Will you tell us the circumstances of that case?—When I was at Jamaica at the Port Royal Dockyard, we had a case of heating. There is a large coal store there.

1006. How did that arise?—I think it occurred from the coals getting wet on being taken ashore, and from their lying in the store; they were lying in the store for two months I should think; the name of the coal was, I think, Parsons Abbey Graigola.

1007. In what year was that?—In 1864.

1008. Did that coal visibly contain a large amount of pyrites?—I did not observe more than usual.

1009. You could not detect the presence of an undue quantity of pyrites?—No.

1010. That is the only case that has come within your knowledge of spontaneous combustion of coal?—Yes, that is the only case I know of.

1011. What did you do in that case?—I had the coals taken out of the store, and when it was replaced, I put iron tubes into it of various lengths suspended from the beams in the roof. I found that these tubes carried off all the gas. I always adopted that plan afterwards, and there was no more heating.

1012. You never heard of any case of dangerous heating in that store since?—No. It was a very large store.

1013. (*Mr. Cory to Mr. Brooker.*) When you were inspecting in the north, and were not able, as you say, to see the shipment of every cargo, were you absent at long intervals from a particular place, could your absence be calculated on?—I do not know that they could calculate on my absence; perhaps I would go to Sunderland, then to Tyne docks, and then to another place probably.

1014. As to this brassy coal which you suppose to have been shipped behind your back whenever you were absent, where did it come from?—I do not know, I cannot recollect it being got 10 years ago.

1015. If this brassy coal could be shipped during the interval of your absence, surely it must have been lying in the neighbourhood already?—No; because it comes five or six miles by train; to the mouth of the pit it would be five or six miles.

1016. Were they steam coals?—They were steam coals.

1017. Newcastle coals?—Newcastle coals.

1018. Were they Hartley coals?—Some description of Hartley coals.

1019. Would not they have to come 20 or more miles to the shipping place?—Not so much as that, I think.

1020. Did the coal come down the Blyth and Tyne railway?—I am not certain about that.

1021. You do not know that the coals come from the north of Blyth, 20 miles off?—No, I do not.

1022. You really think that these coal owners, or their agents, were laying by this brassy coal to put on board when your back was turned?—No, I do not say that, but it seemed to me strange that that should be the only cargo taking fire I could not inspect.

1023. You only know of one cargo being on fire?—That is all.

1024. And it happened that in that case you did

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not very closely inspect the coals ?—I was not able to inspect them.

1025. Do you draw from that the conclusion that whenever your back was turned brassy coals would be put on board ?—No, I have not said that, but I conclude that they would not be so careful.

1026. (*Mr. Fenwick.*) You say that there are occasionally 2,000 tons stored in the hulks ; is there any means taken to ventilate those hulks ?—I do not know of any

1027. (*Mr. Icely.*) Are you in favour of ventilating coal cargoes?—Yes.

1028. For what reason ?—I think that by ventilating the coal you get rid of the gases ; which may be done by putting tubes into the centre part of the cargo. I know this, in my own case, that in the "Crocodile" Indian troop ship the temperature of the coals near the boilers would rise to 136°, and by opening our outside ports the temperature would be lowered.

1029. Are you speaking now of the coals that were in the bunkers at the back of the boilers ?—Yes, at other parts of the bunkers the temperature would be about 80° or 90°. I am speaking of when we were in the Red Sea.

1030. What year are you speaking of ?—1871.

1031. You were in the Crocodile for some time ?—Yes, for $4\frac{1}{2}$ years.

1032. For what period ?—From 1868 to 1872.

1033. During that time had you any cases of heating of coal ?—None whatever.

1034. This heating up to 136° you attributed to the heat of the boilers ?—Yes.

1035. Do you suppose that eventually spontaneous ignition might have resulted from that heat unless you had taken steps to promote ventilation ?—Yes, and I began to work upon that coal immediately. I made use of it at once.

1036. Will you describe the custom on board ship with regard to ascertaining symptoms of approaching danger ?—All coal boxes are fitted with temperature tubes, and every four hours the thermometer is put down in the tube, and is allowed to remain for 20 minutes and then taken up to see what the temperature is. Where there is danger it is done every half hour. The temperature tube plugged at the bottom and partly filled with water gave, it seemed to me, a quicker indication of the heat.

1037. Did you notice in this particular case when the temperature rose to 136° that the temperature rose rapidly ?—No, it rose gradually ; it rose as the weather got warmer.

1038. What would be the ordinary temperature when you commenced to take it ?—About 90°.

1039. What period elapsed between the time when you found it was 90° and the time when it rose to 136° ?—Two or three days.

1040. And it was still increasing ?—No, it remained stationary there for three or four days till we got out of the Red Sea.

1041. You do not apprehend that it would have risen any higher ?—No ; at first I was rather uneasy, but on the second trip when it came to that temperature and did not go beyond, I did not apprehend any danger.

1042. Are you of opinion that the opening of those ports was sufficient as a means of ventilation ?—Yes, that was my opinion, the air entering from the bunker doors when raised caused a circulation.

1043. Do not you think a circulation of air would be better ; do not you think an inlet as well as an outlet would be more advantageous ?—Yes I do ; an outlet either into the steam funnel or the galley funnel would cause a draught.

1044. That would induce an active current ?—Yes.

1045. In the "Crocodile" your coal boxes were in compartments, were they not ?—Yes, seven compartments.

1046. And you had watertight doors ?—Yes, two to some compartments and one to others.

1047. In fact you hermetically sealed your coal boxes when the bunker lids were on ?—Just so, and the bunker lids were made like a joint to keep out the water lying on the decks.

1048. Do not you think there is greater chance of fire or spontaneous combustion where the bunkers are hermetically sealed, as it were, than there would be in the ordinary case of an old ship where the bunkers are not isolated in that way ?—I certainly do. I never knew of any case of gas explosion in the many wooden ships I have served in.

1049. Do you think it advisable that in the bunkers of those iron-clad ships there should be a means of ventilation ?—Yes, I do, by means of tubes placed in a zigzag direction, so that the coal may be more fully penetrated with ventilation.

1050. (*Mr. Duncan.*) Have you any impression in reference to the effect of shipping coals wet ?—I have no practical experience as to that, but I should imagine that coal with a large amount of small in it, when shipped wet, and especially when not well handpicked at the pits, would cause spontaneous combustion.

1051. For what reason ?—I cannot give you any other reason than that which is usually assigned.

1052. You cannot say from any chemical or scientific investigation that the application of water to pyrites would be likely to cause combustion ?—No.

1053. In the 150,000 tons of coal you saw shipped, many of the cargoes went on board wet, I suppose ?—Some of them were so.

1054. You did not object to their being shipped in rainy weather ?—No ; other ships were waiting to be loaded, and if I had detained a cargo, there would probably have been some "demurrage."

1055. You objected to them if you saw indications of sulphur on their surface ?—Yes.

1056. That would have the effect of causing the people at the colliery who were sending down coal for the use of the Admiralty to be careful not to send down waggons of coal that had such indications in them ?—Yes.

1057. Have you sent back coal which possessed such indications ?—No ; but I have always cautioned the colliery agents that I should do so. I may say that the coals I saw were very fair indeed. The difficulty I had to contend with was the small coal. I visited the pits to see what screens they were using.

1058. It is hardly a fair inference, is it, that because you happened to be away, in the case to which you have referred, they sent in sulphurous coals ?—The impression I had then was that this coal would not have been passed by me.

1059. Have you seen them shipping sulphurous coals to any other vessels when you have been inspecting coals shipped for Admiralty use ?—I have certainly seen some things going on, but it was no business of mine, that is to say, those things did not occur in connexion with the coals I was inspecting.

1060. Have you seen coals being shipped on board other vessels for private use that you would not have taken for the Admiralty ?—Certainly I have.

1061. In consequence of their containing apparently a large quantity of pyrites ?—Yes, and I have seen water thrown upon the coal, and when I have asked them what they did that for they would say to lay the dust. I thought it was to increase the weight.

1062. While you were inspecting coals for the Admiralty you must have seen many other shipments of coal for private use ; in your judgment, were there more sulphurous coals among those than among those shipped for the Admiralty ?—Coal under no inspection is not so well shipped, nor of such good quality, nor such large coal as that which is shipped under inspection. I have seen coals shipped in such a careless way that if they had been large they would soon have been reduced very much in size.

1063. When coals are wrought from the bowels of the earth, have they frequently evident indications on their surface that there is a large quantity of pyrites or sulphur in them ?—I have not seen much of it in

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the coal I have inspected; I know there are pyrites in all coal.

1064. Are you aware that the miners when they come upon it know it perfectly?—Yes.

1065. And that they would put it on one side if they were desirous of keeping it out of the coals they were shipping?—I know that coal containing pyrites will come down for shipment in spite of all precautions. When I have been at the pits I have seen two boys probably at each screen hand-picking the coals, and still I know there was a difficulty even then in keeping out the sulphurous coals, because while I have been watching the boys picking out the coals I have said "Here is one piece you have let slip by." I know they were very particular while I was there.

1066. You know there are coals with large quantities of iron pyrites in them which are picked out by the coal shipper?—Yes, the coal is supposed to be hand-picked.

1067. Do you think it probable that the more frequent occurrence of cases of spontaneous combustion during the time that coals were dear was due to the fact that they were not so particular to hand-pick those pyritic pieces?—Very likely it was partly due to that.

1068. (*Sir G. Elliot.*) You say you were stationed on the Tyne?—Yes, at the northern ports.

1069. You would be at the ship at the time the coals were shipped?—Yes, the greatest part of my time would be at the place of shipment.

1070. Had you any written instructions as to your inspection?—Yes.

1071. Have you a copy of them with you?—No.

1072. Do you remember what they were?—I was to see that the coals were not small, to be fresh wrought, and properly shipped. They were to pass over half-inch screens.

1073. Were you directed to take care that there should be nothing in the coals that would lead to spontaneous combustion?—I think not. I do not remember anything to that effect.

1074. The main thing you would pay attention to as inspector would be to see that the coals were large and as free from small as possible?—Yes, that would be one of the principal things.

1075. And that they were put into the ship with as little breakage as possible?—Yes, and this had to be equally well watched.

1076. That would be about the extent of your duty as inspector?—Yes.

1077. You attention had not been directed so much to the spontaneous combustion of coals on board ship at that time?—No.

1078. (*Chairman.*) When you said you rejected coal that looked as if it had much sulphur in it, did you do it on your own motion or under instructions?—When I found coal having those indications I made a complaint.

1079. (*Sir G. Elliot*). You mean this; you saw something go in which had certain appearances which caused you to reject it, but you did not reject it because you thought it would be likely to ignite spontaneously?—I knew that sulphurous coal would be liable to cause spontaneous combustion.

1080. You had no direction about it?—No.

1081. Therefore whether it had been a piece of stone or a piece of pyrites you would have objected to one as much as the other?—I would have objected to the stone as well as the pyrites, because my duty was was to see that coal shipped.

1082. You saw a great many stones go in as well as pyrites?—Not many. Not enough to cause any complaint to be made by me.

1083. One part of your instructions was to see that the coals were sent away free from small. Was not there also an instruction that they were to be clean?—Yes. You could not expect coal after travelling 10 or 15 miles to arrive at the ships without some dust in the coal.

1084. You said that coals that were shipped under inspection were larger and better than those which were shipped without inspection. Do you suppose the viewer at the colliery, who has the charge of the production of the coal, knows when he is drawing coals out of the pit and putting them into the truck, whether there is a ship at the drops for the Admiralty, or a ship for the Austrian Lloyds, or for any other large company who have no inspectors? Do you suppose they know the difference at the colliery?—I know I went into one pit, and found that the screens were not according to the Admiralty width; that is some of the screens were three-eighths instead of half an inch. I objected to the Government coal passing over those screens, and they did not allow them to pass over the three-eighths screens. The ship to which those coals were going was the "Constance," and the trucks were marked "Constance." They must have known that the coals were being shipped for the Government, because they always got intelligence from the agent at the docks that the coal was for the Government, and there was more care taken in consequence.

1085. You were not often at the colliery, were you?—Not often, but as often as I could be.

1086. What colliery was that to which you refer?—The Hastings Hartley.

1087. Do you know what quantity they raise in a day?—Yes, I believe I do.

1088. 1,000 or 1,500 tons a day?—Yes; and it is one of the best coals too.

1089. Do you think they know when each ship goes under the drops, and that they regulate their screens to suit it?—I saw that they were passing the coal over some three-eighths screen, and on my calling attention to it they passed the coal over the half-inch screens.

1090. You say you would not often go to the colliery to see how it was done?—No; I would go about once a month to the different collieries.

1091. How many collieries were you supplied from?—I suppose, roughly speaking, about a dozen.

1092. You would get to each colliery once in 12 months?—No; I would take one here and there, oftener seeing those I took the most coal from.

1093. Your inspection of what they did in loading could not have been very minute?—Not at the pits, as my visits were few; but as I said before, coals under inspection were better in every respect.

1094. When you were looking at them they took more care than when they were not being looked at?—Yes, that was the case.

1095. How many tons out of those 150,000 tons did you see shipped?—I dare say I saw 35,000 tons of them shipped.

1096. You had not much time to go to the collieries?—No.

The witness withdrew.

Mr. W. Covey and Mr. R. Moore.

Mr. William Covey and Mr. Robert Moore examined.

1097. (*Chairman to Mr. Covey.*) You are chief inspector of machinery?—I am.

1098. (*To Mr. Moore.*) You are chief engineer?—Yes.

1099. (*To Mr. Covey.*) You have charge of the steam reserve?—Yes.

1100. You can give the Commission some information as to the storage of coal at Devonport, can you not? how is the coal stored?—Some of it is housed, and some is not.

1101. Is any afloat?—Not at Devonport only what is kept in small hulks.

1102. All your experience, therefore, of storage at Devonport is storage on shore?—Storage on shore.

1103. How much coal is usually in stock there?—I could not say, as I have no charge of coals in the dockyard; I do not know.

1104. (*To Mr. Moore.*) Can you say?—I have no idea at all.

1105. (*To Mr. Covey.*) Can you give us any infor-

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mation as to any heating of coal in store either at Portsmouth or elsewhere?—I have never seen any heating of coal.

1106. (*To Mr. Moore.*) Have you seen any heating of coal?—No.

1107. (*To Mr. Covey.*) Have you seen any heating of coal afloat?—No, not heating. I have never seen any heating in any case.

1108. (*To Mr. Moore.*) Have you any information to give us upon the subject?—I was in a ship on one occasion when we had spontaneous combustion in the coal bunkers.

1109. What ship was that?—The "Gladiator." That was 29 years ago.

1110. (*Mr. Fenwick to Mr. Covey.*) Is the mode of ventilation now different from what it was eight or nine years ago?—In some respects. Pipes are carried away from the bunkers into the funnel casing, and also ventilators are put on the top of the coal bunkers to ventilate them.

1111. Is it desirable that there should be ventilators in the bunkers?—I think so.

1112. Do you know whether fewer cases of heating have taken place since ventilators have been put in the bunkers?—That has come under my notice. Why I think ventilation is necessary is because I have never seen any heating, but I have seen an explosion of gas.

1113. (*Chairman.*) Can you give us any information about that case of explosion?—It occurred in the "Agincourt" about two years ago.

1114. Do you know what coal it was?—No; I cannot remember with certainty the name of the coal; it was a mixed coal.

1115. Had it been long in the bunkers?—No.

1116. Had the lids been kept on some time before the explosion?—No; they were taken off at the regular times.

1117. You do not remember any circumstance connected with the explosion which would be of interest to us?—No more than that. I think in all vessels where there are tank boxes, ventilation is required to allow the gas to escape.

1118. Was there no special ventilation in the "Agincourt?"—There were small valves on the top of the boxes.

1119. Has any order been given, or has any change been made since that explosion in the "Agincourt?"—An order has been given that coal boxes are to be ventilated.

1120. Does that apply to the whole service?—Yes.

1121. Was that the result of that explosion on the "Agincourt"?—Not at all.

1122. Was that one case out of many?—I saw that one and there was a case of explosion on the "Devastation" while I was there.

1123. Do you think that it was in consequence of those cases that that order was given?—Partly.

1124. (*Sir G. Elliot.*) Do you know whether the ventilation of the ship is wholly self-acting or is it promoted by some mechanical appliance?—It is not promoted by any mechanical appliance, it is self-acting.

1125. Do you know whether such self-acting ventilation is sufficient in itself to keep the ship free from the accumulation of gas?—I think it would be sufficient. I have not actually seen the cases, but when the bunker lids have been on more than two or three days there has been an explosion whereas if they had been taken off every day such a thing would not have occurred.

1126. Do they go into the coal bunker with safety lamps?—They have safety lamps in working the coal boxes, they always use safety lamps.

1127. How is the explosion caused if they use safety lamps?—They would not have an explosion if they used them; but in this case, it being rather dark, the man took a light, though he ought not to have done so.

1128. So far as your experience goes, it is more limited to explosion rather than spontaneous combustion?—Yes, I have never seen any spontaneous combustion in any ships.

1129. What quantity of coal do you receive into the stores under your control, what is the largest quantity you have had at any given time in that depôt on shore, would you say 10,000 tons?—I should think there would be that quantity. I have no control or charge of coal in the dockyard; one building is three tiers high, others one; some coals have no covering.

1130. Are they all in contact?—There is a separation, a floor between.

1131. They are not more perhaps than 15 feet apart?—They are only divided by the flooring.

1132. The distance between the floors would not be more than 15 feet?—It would be quite that, it would be 20 feet.

1133. Would it make any difference, supposing instead of having those separate floors the coals were stored in one mass; would it have any effect in producing spontaneous combustion?—I should think so.

1134. However, by your appliances you never have had even heating; have you ever tried the temperature?—No, I have never had anything to do with applying tests in the store.

1135. Is there any opinion or theory amongst you gentlemen who talk about these accidents by spontaneous combustion, as to the cause of it; do you attribute it to any particular cause?—No, I have rarely heard of spontaneous combustion myself. I have heard of explosions. I have never known of any cases of explosion in wooden ships, where there is more likely to be an admission of air from their not being so perfectly tight.

The witness withdrew.

Adjourned to Tuesday next at 12 o'clock.

Tuesday, 22nd June 1875.

PRESENT:

THE RIGHT HON. HUGH C. E. CHILDERS, M.P. IN THE CHAIR.

Sir GEORGE ELLIOT, Bart., M.P.
HENRY HUSSEY VIVIAN, Esq., M.P.
FREDERICK AUGUSTUS ABEL, Esq., F.R.S.
RICHARD CORY, Esq.
GEORGE DUNCAN, Esq.
JOHN FENWICK, Esq.
JOHN FERGUSON, Esq.
CHARLES ICELY, Esq.
JOHN PERCY, Esq., M.D., F.R.S.
WILLIAM YOUNG, Esq.

J. M. CARMICHAEL, Esq., Secretary.

Mr. W. R. PRICE examined.

Mr. W. R. Price.

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1136. (*Chairman.*) You are a shipowner in London?—Yes.

1137. I believe you have had experience of more than one case of spontaneous combustion in ships belonging to your firm?—Yes, two cases.

1138. Both, if I am not mistaken, were cases of coal

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shipped from North Shields ?—Yes, in the Northumberland dock.

1139. They were both in the same month ?—Yes.

1140. The shipments in both cases were for the Peninsular and Oriental Company ?—Yes.

1141. Will you inform the Commission what the size of the cargoes was ?—Yes. The cargo of one of them, namely, the Calcutta, was 1,870 tons, to the best of my recollection, and the cargo of the other 1,600 tons ; the respective tonnage of the ships being 1,372 and 1,112 tons.

1142. They were both practically full ships ?—Yes.

1143. What were the names of the ships ?—The name of the 1,372 tons ship was the "Calcutta," the name of the 1,112 tons ship was the "Oliver Cromwell."

1144. Were they both wooden ships ?—Yes.

1145. Will you state what the character of the coal was ?—I did not see the bill of lading, but I believe it was Howard's West Hartley, I was told that was the coal.

1146. Were both cargoes shipped in the summer of 1874 ?—In June and July 1874.

1147. What was the ventilation adopted in the ships ?—I am relying on what I heard from my overlooker in the matter entirely ; the ventilation was a horizontal shaft near the bottom of the ship, from the keelson to the skin of the ship, boarded in, making a triangular current right through this communicating with two lines (about 18 inches square) of tunnelling along the tops of the 'tween deck beams, these being connected with upright shafts in each hatchway.

1148. Were the upright ventilators the one upwards and the other downwards ?—No, at least I imagine they would be self-acting.

1149. Do you know how the coal was put on board, was it by tips ?—From tips in both cases.

1150. Do you know from what height the coal fell ?—I could not state the number of feet. I was there when the Oliver Cromwell was under the tip, and it struck me as a very considerable height ; I do not know what the state of the tide was when I was there, whether the water is higher or lower at various states of the tide I do not know, but the fall was considerable.

1151. When you were there was the coal much broken in the fall ?—Very much indeed. I went into the hold of the "Oliver Cromwell" when she was about half loaded, and I noticed in the main hatchway where the bulk of the cargo was coming down a great accumulation of small coal in a pyrimidical shape, and I said to the captain as I came up, "I am thankful that "I am not the owner of this cargo, there seems to be "so much small in it."

1152. Can you give the Commission any information as to the nature of the casualty in the case of the "Oliver Cromwell," where and how it was observed, and what the result was ?—I took all the trouble I could to elicit the information, but the only parties who came home from the "Oliver Cromwell" were the captain and an apprentice ; the rest of the crew were paid off at the Cape and took other employments. Their information did not quite correspond, but at various times I had conversations with them, and the apprentice gave me to understand that there was no indication of anything wrong either in the fore or after peak of the ship where he was frequently down for various purposes. He never detected any escape of gas or anything of that sort, but he said that a fortnight before the fire broke out he noticed vapour coming up the main hatch ventilation, and he said as you passed between it and the wind, there was an unpleasant smoky smell, but they evidently treated it as nothing more than was usual in coal ships. I asked him whether they were not alarmed, he said not at all, and it was only the day before they had to abandon the ship, that the fire broke out and they noticed it by smoke oozing through the upper planking of the ship.

1153. Not coming up through the ventilators ?—Not coming up through the ventilators.

1154. How long had the ship been out ?—She sailed on the 18th of June and was burnt on the 6th of September.

1155. Did she become a total loss ?—She became a total loss.

1156. Was there any Board of Trade inquiry ?—There was an inquiry at the Cape.

1157. Do you remember what the result of the inquiry was ?—I remember that the general result was considered an unavoidable casualty.

1158. There was no scientific inquiry ?—No.

1159. That was the case of the "Oliver Cromwell" ?—Yes.

1160. Now will you give us the particulars of the other case ?—In the case of the "Calcutta" she sailed on the 2nd of July, and was burned on the 17th of September.

1161. In about the same place ?—A little wide of the same locality, but pretty much in the same region.

1162. Was she a total loss ?—She was a total loss, with a very heavy loss of life.

1163. What were the circumstances as reported to you about that ?—The few men that arrived at home were about as stupid a lot as I ever met with ; there was only one intelligent man among them.

1164. Was the captain lost ?—The captain was lost, he, a little boy of his, the chief officer, the carpenter, and several of the men ; 12 in all had the biggest boat, the others were in two other boats. It came on to blow the night after they left the vessel and the boats separated, and they never saw anything of the captain and his boat ; the others were picked up by a passing vessel.

1165. What information did they give you as regards the fire ?—They said they had no indication of the fire till it had gained considerable power, and a few hours afterwards they were obliged to abandon the ship.

1166. How did they notice it first ?—By smoke coming up the hatchway, and they immediately dug down into the coal and they got some distance below the line of 'tween deck beams, below the main hatchway, and then the heat was so intense and the smoke so suffocating that they were obliged to leave it. They poured down water and so on but without any effect ; it had gained a considerable hold of the cargo for almost immediately the masts fell over the side; they were iron masts. The water in the pump well with the intense heat at the bottom of the pump generated steam and blew all the pumping apparatus right out.

1167. Was she ventilated in the same way as the "Oliver Cromwell" ?—Exactly in the same way.

1168. Do you attribute the fire to any peculiarity of the coal ? Was any other coal of the same denomination shipped about the same time ?—In giving an opinion upon that it is simply my own individual opinion, I have no scientific knowledge upon the subject, but as far as I can reason upon the subject I attribute the loss to some inherent quality in the coals as in the other case.

1169. Was there any other ship loading with the same coal ?—The "Euxine" was loading in the dock at the same time ; she was an iron ship belonging to Messrs. Bates of Liverpool ; she was also burned.

1170. (*Sir George Elliot.*) Was the coal in each case from the same colliery ?—I think so. I could not positively say it was coal from the same colliery in the case of the "Euxine."

1171. (*Chairman.*) Were the coals loaded in the same dock ?—Yes.

1172. And probably in the same way ?—Exactly in the same way.

1173. And that vessel was lost about the same time as the others ?—A few days before the others ; she had not got quite so far. There was a curious fact connected with it that a ship called the "Corah," a large American ship, was loading about the same time. She was an American wooden vessel and the captain demurred to ventilation altogether. The Peninsula and Oriental Company coal inspector had some conversation with him upon the subject, as I gather from a report made by him to his directors, and the

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captain of the "Corah" said that he did not hold with ventilation except above the cargo, for instance, under the 'tween decks and under the main deck; that as to through ventilation he did not hold with it, but he said if the inspector insisted upon it he must do it; he said he had no instructions to insist upon his doing it, and so he left the captain to do as he liked.

1174. What was the result?—She arrived out at Bombay apparently without accident so far as I can see from the lists.

1175. I believe you have had great experience in shipping coal?—For the last 17 years I have scarcely carried anything else as outward cargoes to Bombay.

1176. Especially for the Peninsula and Oriental?—And for the Great Indian Peninsula Railway Company, and sometimes on our own account.

1177. So you can speak on the matter with very considerable experience?—Certainly; but not with regard to the Tyne, those were the only two cargoes I ever carried from the Tyne.

1178. Where have you carried principally from?—From Birkenhead in all cases, except patent fuel from Cardiff occasionally.

1179. Have you ever had any casualties in ships sailing from Birkenhead or Cardiff?—Never.

1180. Is the manner of putting the coal on board different in Birkenhead from what it is in the Tyne?—There are two modes of loading cargoes at Birkenhead; one is by tips similar to the mode of loading at the Tyne, and the other is by barrows. The railway runs alongside the dock and the railway wagons are brought alongside the ships, and when they are loading by barrows a stage is made right over the main hold; the coal is shovelled into barrows and wheeled over the hatchway; then the tackle is fastened to the barrow and it is lowered on to a platform in the hold, and it is then wheeled to the spot where it is wanted.

1181. In your opinion is that method preferable not only in reference to its doing less damage to the coal, but with reference to the subject of our inquiry?—Unquestionably that is my opinion.

1182. You think the distance that coal ought to fall should be a minimum?—I think there is less breakage, and consequently a better natural ventilation through the cargo; it does not lie in such a dense mass.

1183. You would recommend that all coal should be shipped in that way?—I put it this way; that in all cases I should stipulate for it where I could. I am bound to say that shipowners cannot command everything; but if I could, I would never ship a cargo otherwise than in that manner.

1184. With reference to ventilation, you have told us what the captain of the "Corah" thought about it. Have you formed any judgment upon the question?—I certainly have come to this conclusion, that if I were obliged to carry coal from the Tyne, I should certainly not adopt any other mode of ventilation than that which for 17 years I have successfully adopted in the case of vessels going from Birkenhead.

1185. With reference to your shipment from Birkenhead, what plan of ventilation have you adopted?—Exactly the same as I adopted in the Tyne; it has always been successful in the case of vessels going from Birkenhead; but it was fatal to me in the case of the vessels going from the Tyne.

1186. Your experience of the Tyne is limited to those two cases?—Yes; but there was the "Euxine" which was loading at the time.

1187. Was she lost also?—She was lost also.

1188. May that not have been from the coal in each case being supplied from one particular seam?—It may have been; I can call to mind several other vessels, one of them the "Bernice," that came from Dundee to the Tyne, she also was burnt close to Bombay, having on board Tyne coal. I could not say whether it was from the same seam. I have noticed more burnings in the case of vessels from the Tyne than I have from any other place.

1189. Are you aware that a good deal of the north country coal is shipped very much upon the plan you have described as being the one you prefer?—No, I am not aware of that; I never was on the Tyne except on the occasion when I had those two ships loading there.

1190. And when you were so unfortunate as to lose both ships?—Yes.

1191. (*Sir G. Elliot.*) The "Calcutta," you said, sailed on the 2nd July, and she took fire on the 17th of September?—Yes.

1192. How many days was that that she was out?—77 days.

1193. Now with regard to the "Oliver Cromwell," how long was she out?—I think she was out 80 days.

1194. Your two vessels, the "Oliver Cromwell" and the "Calcutta," both loaded Howard's West Hartley, did not they?—Yes; I have not seen the bills of lading. Directly I heard of the casualty to the "Oliver Cromwell," I went to the brokers office to ask what coal she had on board, and the message that came back was Howard's West Hartley.

1195. That was one of the vessels?—Yes, they were both chartered by the same parties.

1196. The "Calcutta" you say, was a total loss?—Yes, and the "Oliver Cromwell" was a total loss also. There were lives lost in the case of the "Calcutta."

1197. Then you said that the "Euxine" was loading at the same time, and that she also took fire?—Yes; she took fire a little earlier. I remember being in Liverpool and hearing an account of the loss, and it made me wonder how my ships stood, they having been loaded about the same time; it might have been in the early part of August that she took fire.

1198. Do you know what coal the "Euxine" took in?—I believe it was the same coal; I am speaking from impression; the parties would be very chary in giving me information about it.

1199. Then you mentioned the "Corah," and you said that she was loaded about the same time?—She was in the Northumberland Dock at the same time.

1200. The arrangements for the ventilation of the cargo in her case were dispensed with, and she arrived safely?—Yes.

1201. The other three vessels adopted the system of ventilation which you have practised for many years at Birkenhead?—I believe so; I am corroborated in that opinion by a report of the Peninsular and Oriental Inspector Mr. Steinson, in which he says, that he believes that they were so ventilated.

1202. (*Chairman.*) Will you read that report?—He says:—"November 1874. To the Managing Directors of the Peninsular and Oriental Steam Navigation Company, London. Gentlemen, I beg to acknowledge receipt of your letter of 2nd inst. respecting the burning of the 'Calcutta,' loaded with Howard's West Hartley, at sea, and referring also to the burning of the 'Euxine' and 'Oliver 'Cromwell,' with the same coal, and requesting me to institute careful inquiry into this matter, and as to the liability of this coal to spontaneous combustion. In reply, I beg to state that the 'Calcutta' and 'Oliver Cromwell' were both single decked wooden ships, and were well ventilated; the 'Euxine' was a double-decked iron ship, and I think also well ventilated. In looking over my journal I find these ships were all loaded in fine weather, that the coal looked very well, and the only remarks about these cargoes were that the ships were too high for the accommodation, and the coals consequently much broken in shipment. The 'Cora,' American ship, was loaded at the same time as the 'Calcutta,' and with the same coal; this ship was double decked, but the captain, who was a man of great experience, objected to through ventilation, but said, if I insisted on the ship being ventilated, he would do so, though he would rather not. I said I had no instructions as to ventilation, and my opinion was that it was unnecessary, and was dangerous in many cases, this ship was therefore not ventilated through the coal, but was well ventilated above it, and I find in my journal that a portion of this cargo got wet with hail and rain, and was thus more liable to heat. As

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“ to the necessity of ventilation in cargoes of north country steam coals, and the mode of ventilating, I may state I have been giving this subject much consideration for years, and more especially of late, when so many ships are reported as having been burned at sea by the spontaneous combustion of their cargoes, and to the fact that, so far as I know, all the ships which have been burned have been well ventilated, or what is considered good ventilation, viz., vertical shafts in each hatchway, connected at the bottom either with a platform under the coal, or with other shafts running fore and aft under the coal alongside the keelsons or sometimes vertical shafts only in each hatchway, whereas ships belonging to the north, as a rule, have no through ventilation, and they very rarely take fire. My opinion is that north-country steam coal, under ordinary circumstances, does not require through ventilation, but that it is actually safer without it for the following reasons, viz.—1st. The coal does not contain sufficient gas to require any through ventilation; 2nd. The coal when well cleaned does not contain in itself anything to cause spontaneous combustion; but if any pyrites should have been overlooked in the cleansing, and is then subjected to a heavy weight of coal passing on it, it is liable to heat, and if a current of air can get at it from the through ventilators, it might then ignite, and probably set the whole cargo in a blaze, which would be effectually fanned from the ventilators, and the ventilators themselves being usually made of dry soft wood would probably be the first thing to ignite. I would suggest that the ships should be well ventilated above the coal only, that as many ventilators through the deck-houses as can be conveniently fitted be well attended to, and that on all possible opportunities the hatches should be kept open. I submitted my reasons and my proposed mode of ventilation to my friend the practical chemist before sending them to you, and I have not altered one word of my original copy. He said what I stated is a great chemical fact, and not to be disputed, and that the most effectual way to set a ship on fire with the north-country steam coal is to put in through ventilators, as it is scarcely possible to avoid pyrites among the coal to a greater or less extent. As to Howard's West Hartley being peculiarly liable to spontaneous combustion, I am unable to say. So many ships being burned at one time looks suspicious; but then ships with other coal have been burned at the same time—the ‘Pocohontas’ with Hasting's Hartley, the ‘Susannah Johanna’ with Carr's Hartley, ships from Cardiff, from Liverpool, from the Clyde, from Dundee, and ships with general cargoes. It looks like an epidemic; and I fear the reason will have to be discovered from other causes than from spontaneous combustion. For many years very many ships, and many of large size, have loaded this coal for the company's Indian ports, and carried it safely. I would respectfully suggest that this question be submitted to some eminent practical chemist or other authority for confirmation.”

1203. (*Sir G. Elliot.*) You were speaking about the improved mode of loading the cargo by barrows with a ship like yours of 1,800 tons, would not it occupy a long time to load it in that way?—It does require a longer time, and costs more money.

1204. How long would it take to load an 1,800 tons ship?—The loading is not continuous. I do not know how long it would take to load an 1,800 tons ship; sometimes you have a great pressure of coal from the pits, and then you can have the three hatchways going; on other days you have only one hatch working, and so forth. I do not know how much they can load by barrows in a day.

1205. (*Mr. Duncan.*) You could load in about a week?—Yes, if you put plenty of hands on you could load 250 tons a day.

1206. (*Chairman.*) Loading at all the three hatchways?—Yes.

1207. (*Sir G. Elliot.*) Take Cardiff and other ports where coals are loaded; suppose that system were to be adopted, would it not take two or three times the amount of dock space?—At Cardiff the docks are so crowded, and the difficulty of getting your cargo in is so great, that I think it is quite an exceptional place as regards loading.

1208. Take the Northumberland dock; how long were they loading that 1,800 tons ship there?—I do not know; they were some days when they were not doing anything; it was done very quickly when they did get to work.

1209. Is not it a usual thing in the Northumberland Docks for a thousand-ton ship to go in and come out on the same tide?—Yes, I am aware that there is great rapidity in loading, but I think that at times there is a corresponding disadvantage; it may do for coasting voyages, but though there is great facility if you can get your ship under the tips and get her loaded right away, and though there is some saving, I think there are some disadvantages about it for long voyages.

1210. You think where you have long voyages and where the coals have a tendency to spontaneous combustion it might be in some measure guarded against by that more careful mode of packing them as compared with the rapid manner in which they are now loaded?—Unquestionably, of course the capabilites of the place must always be taken into consideration; the coal contractor at Cardiff may have several vessels loading at the same time, and sometimes your vessel may be waiting a fortnight or three weeks for a turn; and he may have three or four other vessels loading at the same time, and he may give you 500 tons a day, then a steamer may come in and you may be hauled out of your berth so that he may fill her up. It is not a question of a vessel going under the tips and getting her cargo right away at once. If they choose they could put 1,800 tons in a vessel in 48 hours. The practical result is that they ship a portion of the cargo very rapidly and then there is a stoppage for some time and that is where time is lost.

1211. If the coal at Cardiff were to be shipped in the manner you mention by barrows, it would take more than three or four times the area for the ships to lin in all the time that they would be detained?—Undoubtedly that would be required. I have loaded vessels on two or three occasions from Birkenhead by tips before I became alive to the advantage of the better mode of loading, and I have never had a heated cargo in a vessel going out of Birkenhead under either system.

1212. You have loaded by both tips and barrows at Birkenhead?—I have.

1213. And you have never had any heated cargoes from either mode of loading?—No.

1214. What is the advantage you attach to loading by barrows so far as Birkenhead is concerned?—Hitherto I have not done it voluntarily, I have done it because it was a stipulation in my charters that that mode of loading should be adopted in future, I shall always adopt it because I think the cargo is more equally distributed, the small coal does not get into a mass in the region of the main hatchway which is the locality where according to my experience fires usually take place.

1215. Your idea is that by shipping the coal as you do by barrows there is a partial ventilation on account of the roundness of the coal?—Yes, it does not get into a dense mass.

1216. Is not that rather against Mr. Steinson's opinion, that he thinks it is safer without any ventilation at all?—No, I think it corresponds rather with his theory of ventilation above the cargo, there is a great amount of natural ventilation that would give a current of air through. I asked the captain of the "Oliver Cromwell" whether his ventilator acted properly and he said that on several occasions he tested them by taking a piece of paper and tying it by the four corners with a piece of string and putting a pebble or something into it and dropping it down the ventilator, and said when he got it down a certain distance, he would

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let the stone fall out of it, and up would come the string and paper together showing that there was a strong draught through his ventilator.

1217. Those coals would be brought out of a mine at a temperature of about 55° and they would be put into the ship at a temperature of from 50° to 60°, that would be the temperature the coal would go in at; assuming that to be so, you put down those ventilators, you go to sea, and then when you get into the tropics you pass a current of air through this coal containing pyrites, perhaps, that current of air having a temperature of 130° or 140°, is that in your opinion calculated to help to ignite the coal?—I should think so, if the temperature of the atmosphere was much higher than that of the coal it would do so, but my opinion is that it would be very much cooler if you got a current of air than if you got a perfectly still atmosphere. A current going down through a ventilator like that would rather cool it than otherwise. When I have been down in the hold of a ship in the Tropics unless she has been very high out of the water, I have found it much cooler down below than up above.

1218. From your experience of over 17 years in shipping coal at Birkenhead where you have tried the two systems of loading, the one by barrows and the other by tipping, you have never had a single instance of heating?—I am giving my own experience as a shipowner.

1219. As a shipowner you have never gone to the Tyne except in the case of two shipments, and there you found that the mode of ventilation which seems to have been successful in protecting you at Birkenhead failed you in both instances when you carried the Tyne coal?—Quite so.

1220. (*Mr. Duncan.*) I suppose the necessity for shipment by barrows at Birkenhead would arise somewhat from the coals being wrought in much larger lumps than they are in the Tyne?—I imagine it is to ensure them being shipped in as big lumps as possible, to keep the coal in large masses, but I do not think it is a necessity arising from the coal being in large lumps when wrought, it is that the shippers of the coal, the Peninsula and Oriental and the Great Indian Peninsula, like it as much unbroken as possible. Taking it by barrows ensures its being shipped without getting too much broken. Then again I have frequently noticed (and my attention was particularly directed to it) that in loading the coal by barrows great care is taken to pick out every objectionable looking piece. I have seen at the end of a day's work a great pile of brassy looking lumps and slaty pieces all carefully taken out. Whether there is anything dangerous in those brassy looking lumbs I am not prepared to say. I imagine that they pick them out because they are not good to burn.

1221. You have probably seen coals loaded at the Northumberland Dock with an iron skid put down the hatchway to cause the coals to run to the side?—I was only there for a few hours when the ship was loading. I did not notice it particularly.

1222. In your judgment had the weather at the time the coals were shipped in the cases of the "Oliver Cromwell" and "Calcutta" any effect on the coals?—When I was in the Tyne in June when the "Oliver Cromwell" was loading, I think I remember there was a little drizzling rain, it was not actually dry weather, it was dull and cloudy weather.

1223. It was a dry summer altogether?—Yes, there was no rain to speak of.

1224. (*Mr. Ferguson.*) Do you know whether the coals put into those vessels that were burnt had been long out of the pits or not?—I have no knowledge upon that subject.

1225. Do you know whether in the cases of the shipments from Birkenhead any long interval has elapsed between the time of the coals being wrought and their shipment?—Sometimes they would be fresh wrought, and sometimes they would have remained sometime before being shipped. They have to come some distance by railway. I have known blockages on the line that have prevented our getting coal for some days, the colliery manager has said that it has left the pit but it has been detained in trucks on the line, and there it has been for days and days sometimes.

1226. Were all the ships that you have loaded from Birkenhead ventilated?—Yes, they were always ventilated in the same way.

1227. (*Dr. Percy.*) I think you spoke just now in favor of the system of loading by barrows on this ground, that there was not likely by that system to be the same accumulation in any one spot of small coal or slack?—Yes.

1228. Have you any reason to believe that spontaneous combustion is more likely to occur in an accumulation of fine coal than elsewhere?—The only reason I have for entertaining that opinion is that when coal is loaded by the tips I have always noticed a larger accumulation of small coal in the wake of the main hatch than at any other place, and speaking of the experience I have had in the cases of the "Oliver Cromwell" and the "Calcutta," in both those ships the seat of the fire was in that locality.

1229. Where the fine slack had accumulated?—Where the fine slack had accumulated, and I also asked Mr. Bates before my ships were burned what his information was as to where the fire took place, and he told me exactly in the same locality. An experienced ship master with whom I was talking yesterday, who had been in ships that had been on fire, and had attended three surveys of ships that either came into Bombay on fire or had taken fire while lying there with coal, said that the fire in each case occurred about five feet from the skin of the ships in the region of the main hatchway.

1230. When there was an accumulation of fine slack?—Yes, that is what he attributed it to.

1231. You said that in the case of the Oliver Cromwell the men noticed an unpleasant smoky smell, which is said to be usual in coal-laden ships?—I did not say it is usual, but the apprentice assumed it to be usual in coal-laden ships. He treated it as quite a casual matter.

1232. (*Sir G. Elliot.*) The main hatchway would be where the ventilating shaft would be placed?—Yes, the ventilating shaft would come there.

1233. It was not merely that there was an accumulation of slack there, but that would be where the main shaft for the ventilator would be placed?—Yes, but the mass of small coal would be so dense that the ventilator would not act in the same way there as in the fore and after hatchways, where there would also be upright shafts.

1234. (*Mr. Abel.*) I gather from your evidence with regard to what you believe to be useful ventilation, that what you require is ventilation outside the mass of coal, so as to facilitate the escape of gas from the coals. Is that your opinion?—I cannot say that that is my opinion at all, because my opinion, based entirely upon my experience, is that the ventilation I have hitherto adopted with such success at Birkenhead is the proper ventilation for certain descriptions of coal; what the chemical ingredients may be in other coal which may require a different treatment I cannot say, as I have only had the experience of those two ships; but taking Birkenhead or Cardiff coal, I should adopt the same mode of ventilation that I have hitherto adopted successfully.

1235. Would the object of adopting that mode of ventilation be to guard against fire, or to guard against explosion?—It would be rather to satisfy my underwriters than to guard against anything; for I tell you candidly, that having been free from casualties, I simply adopted that which the underwriters, surveyors, and such like people recommended without looking at it critically or carefully at all. The first thing an underwriter says to you when you are insuring a cargo of coals is "Will your friends see, or will you see, to the ventilation?" They make a special point of seeing to the ventilation.

1236. (*Mr. Young.*) Have you any instructions to try the heat of the coals in the course of the voyage by the thermometer?—I never gave any pointed instructions till recently upon the subject, but I have always

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told the captains to keep their hatches off to prevent the possibility of an explosion. I have seen a casualty from Cardiff coal in particular arising from keeping the hatches on, but I never told our captains to use the thermometer till recently, though the ships have always been provided with thermometers for the purpose.

1237. Providing a thermometer would be a way of telling telling the captain that he ought to use it?—I should think so.

1238. Do you instruct them to record in the log the heat of the coal?—I do so now.

1239. You did not do so formerly?—No.

1240. If they have a wooden ventilator as you describe, how do they get the thermometer down into the mass of coal?—The sides of the ventilators are two-thirds open, like a venetian blind.

1241. Louvred?—Yes; and any gas or heat that is in the coal will naturally find its way through. By taking the state of the thermometer on deck, and then putting it down and keeping it down for a certain time, and then taking the temperature below, you get at the difference.

1242. I understand you to say that you have been a shipowner for 17 years?—Yes.

1243. Mainly in the East India trade?—Yes.

1244. Practically, your ships have always carried coals?—Yes.

1245. Have those burnings interfered with your trade recently?—Very seriously.

1246. How?—In the question of insurance. The rates of insurance on coal for long voyages are almost prohibitory, to some places entirely prohibitory; for instance, round to the west coast of South America, and to Japan, and those places, and even for shorter voyages from the Tyne, I have been debarred from replacing those two ships with similar vessels or (new colonial ships) simply on account of the amount of insurance. If you are tied to coal cargoes, the cost of insurance now as compared to what it used to be becomes a serious item.

1247. (*Chairman.*) Will you give us what used to be the rate, and what it is now?—For many years the rate I paid to take coals from Birkenhead to Bombay was 60*s.* to 3*l.* 3*s.* per cent.

1248. (*Mr. Young.*) On the hull of the ship?—On the hull of the ship.

1249. (*Chairman.*) And on the cargo, what was it?—About the same; I am speaking of colonial ships. On iron ships it would be lighter; it then rose to 80*s.* or four per cent., and subsequently to 90*s.* or four and a half per cent.

1250. Is that what it is now?—No, it is now 6*l.* 6*s.*, that is the minimum; I had to pay in the early part of the present year as much as 8*l.* 8*s.* per cent. to complete my insurance.

1251. Was that to Bombay?—In one case to Bombay, and in the other to the Madras coast.

1252. (*Mr. Young.*) Do you think that this state of affairs is causing the good class of ships to fall into disuse, and bringing in a lower class of ships?—It is certainly deterring a great number of people from putting them into their high class ships.

1253. The result would be that inferior vessels would be bought for the purpose of carrying coals on those long voyages, the owner probably not insuring them at all?—I have been seriously contemplating doing that within the last 10 days. I have been talking the matter over with my friends, whether it would not be better worth our while to have good sound older ships which are of comparatively little value for the carriage of coal, and take our own risk rather than pay those prohibitory rates, whether it would not be better to take a vessel of 7,000*l.*, or 8,000*l.*, or 9,000*l.* value, and divide it into fourths among my friends, and taking $\frac{1}{4}$ instead of $\frac{1}{2}$, and running my own risk.

1254. That arises practically from the opening of the Suez Canal, and from the fact that ships do not get any outward cargo except coals?—Except coals and salt.

1255. (*Mr. Duncan.*) You find not only that the premium has increased so enormously, but that many underwriters will not take coal ships at any price?—The area of insurance on ships loaded with coal has been limited to an extent that I have never seen in my 25 years' experience, some underwriters decline them entirely; they will not look at them.

1256. (*Mr. Icely.*) You have informed the Commission that in the case of the "Oliver Cromwell" the coal was very small?—It was.

1257. That was your ship?—Yes.

1258. It was not your cargo?—No.

1259. Therefore it was not to your interest to see that the coal was shipped in good condition?—No; I took the coal as it came.

1260. Was that coal inspected?—Yes, I think so; I may say that I did not see the whole of the cargo; it was only what I saw when the ship was half loaded; I saw a dense mass of coal in the region of the main hatchway. I imagine the Peninsular and Oriental Company had their inspector there.

1261. This was for the Peninsular and Oriental Company?—Yes.

1262. In the case of the "Corah" the cargo was also for the Peninsular and Oriental Company?—Yes.

1263. She arrived safely?—Yes.

1264. In the case of the "Oliver Cromwell" the cargo ignited?—Yes.

1265. (*Mr. Fenwick.*) Do you know what quantity of coal the "Corah" had in her?—I should think from her size she must have had about 2,000 tons.

1266. The vessel was not ventilated?—She was not ventilated.

1267. You say that from your adoption of the mode of ventilation which you have adopted in the case of vessels sailing from Birkenhead, you have been kept free from accidents in ships going from Birkenhead?—I am not prepared to assert that that has been the cause of my having no burnings; but I have ventilated the ships, and I have had no burnings and no heated cargoes.

1268. That mode of ventilation has been successful in Birkenhead, but it has not been so in the Tyne. From your experience, supposing you were loading coal in the Tyne, would you ventilate your ships in the same manner as you do at Birkenhead?—I would move my ships to any part of the coast sooner than send them to the Tyne. If I had a ship at Dundee, I would send her round in ballast to Liverpool rather than go to the Tyne, because it is prejudicial to one's business and status to be losing one's ship, though the cause is ascertained, and it is proved, that you are not to blame. I have had sufficient casualties and I do not want any more.

1269. Still the fact remains that the other vessel, to which you referred, that was not ventilated, arrived at port without any accident?—Yes; but a burnt child dreads the fire. I do not want to try any more experiments with Tyne coal.

1270. (*Mr. Cory.*) I understood you to say that you were now contemplating arrangements for taking your own insurance risk?—Yes; it is passing through my mind.

1271. Are you under the impression that there is rather an unjustifiable panic amongst the underwriters and that they are charging too much?—I would put it in this way; they are not discriminating, they are panic struck as regards every port, but particularly so as regards the Tyne. They are not putting a differential rate on the different ports as they ought to do, taking into consideration the number of casualties in vessels sailing from one port as compared with vessels sailing from other ports.

1272. Have not the premiums gone on increasing on other risks besides those on coal laden ships?—Not to anything like the same extent.

1273. But to some extent?—On some voyages they have; on some risks they are very much lower.

1274. Lower than they were five or six years ago?—Yes, unquestionably; you may take it thus; that in a business like coals or rough cargoes, generally the rates are higher; but on those things which the

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underwriters have lost enormously, such as steamers through the Suez Canal and on goods coming from Calcutta, they are decidedly lower than they were some years ago. Directly we get a rough risk, that is to say, a rough cargo, coals or salt in a wooden ship, the underwriters do not care to name a rate, but particularly when we get coals.

1275. (*Mr. Vivian.*) Do you attribute the immunity from accident in the case of coal shipped from Birkenhead to the character of the coal or to the ventilation which you adopted?—I am inclined to think that it is a great deal more from the character of the coal than anything else.

1276. Have you known cargoes of coal from Birkenhead which were not ventilated take fire?—I do not know whether they have been ventilated or not, but I had recently through my hands a list of 834 coal-laden ships that sailed in 1872, 1873, and 1874 from Birkenhead to all parts of the world, a large number of them to the East. I put that list in the hands of the secretary of Lloyd's and asked him to have the books and records of Lloyd's searched and to give me the results of the voyages of those vessels. Some of the 1874 voyages were not then ended, but the result as far as I remember was this, that out of the 834 ships there were only three known to have been burnt, or supposed to have been burnt, and five missing.

1277. Or they may have foundered?—Yes, it was three or four months ago that that came before me. Therefore I am bound to consider with such a large number of ships, and such a small per-centage of burnings, there must be something in the coal itself.

1278. Have you had any experience of shipment at Cardiff?—I have had some experience, but nothing like the experience I have had at Birkenhead.

1279. Would you ventilate Cardiff coal, or would you not ventilate it?—I always have ventilated Cardiff coal when I have shipped it.

1280. Have you had any cases of spontaneous combustion from coal shipped at Cardiff?—No, not as a shipowner.

1281. Have you shipped any unventilated coals from Cardiff?—Never.

1282. Do you know of any unventilated coals having been shipped from Cardiff?—I do not.

1283. You said that the fires chiefly occur in the main hatchway, and you attribute it to the smallness of the coal in the hatchway?—Yes, to the density of the mass.

1284. It is customary, is it not, to load coal at the other hatchways as well as the main hatchway?—Yes, they trim the cargo in that way; when they cannot trim it by hand easily, they move the ship to load the coal at the ends.

1285. Ships constantly take coal on board at all the hatchways, do they not?—Yes.

1286. Why do you think that the coal becomes more liable to spontaneous combustion at the main hatchway than at other hatchways?—From the facts that have come before me I have found that it is in that direction the fires occur; the great bulk of the cargo goes in at the main hatchway; it is only a small proportion that goes in at the other hatchways.

1287. Have you ever shipped coal or taken coal on board under the box drops?—I cannot call to mind that I have.

1288. I presume your reason for preferring barrows to tips is chiefly from the breakage of the coal in the hatchway itself?—No, I have no interest in the breakage except the cargo is my own, and even then very little, because it is generally sold on the endorsement on the bill of lading. One of my preferences for the barrow system is founded on this, if you are loading by tips sometimes if the captain is away they will carelessly put in too much at one end, and the ship will strain herself, whereas when you are loading by barrows you can wheel them where required so as to trim the ship exactly as you want.

1289. That applies to the trimming of the ship. I am speaking of the liability to spontaneous combustion and the production of small coal; supposing small coal were not introduced into the hatchway by the adoption of another system of shipment, namely, by the box system, would that remove your objection to shipment by tips?—Next to the system of loading by barrows I should prefer the coal being loaded in boxes in the way you speak of.

1290. I presume when a column is once formed in the hatchway the liability to produce small coal is no longer so great?—I have always seen it come up in a pinnacle of small coal to the very top, and as long as there is room for it to run clear of the hatchways the big lumps of coal run down and the small coal generally culminates.

1291. After that there is not the same liability to breakage?—There is not the same liability to breakage.

1292. So if you formed your column in the hatchway by boxes and then used the tips the difficulty you have mentioned would be got over?—It would be got over to some extent certainly.

1293. (*Chairman.*) Your evidence has only gone to the shipment of steam coal, have you had any experience in the shipment of smithy coal or any other coal?—Nothing to speak of. The firm with which I was connected used to ship smithy coal largely to Bombay as merchants, and we never had any accident with it.

1294. Your evidence hitherto has been only as a shipowner, I believe you have been an insurance broker also?—Yes.

1295. Have you had any experience as an insurance broker with regard to this question?—I have had 25 years' experience as an insurance broker in London.

1296. Have you had any experience on this particular question, have you had any experience of cases of combustion in ships which you have insured?—Yes.

1297. Could you give us an outline of that experience?—I have not gone back many years because one's memory is treacherous upon these things, but in October 1867 I insured a ship called the "Sir George Seymour" from Birkenhead to Bombay, in which ship spontaneous combustion took place; that is the only case of burning in a vessel sailing from Birkenhead that has ever passed through my hands in my 25 years' experience, insuring coal to all parts of the world, or that I have known. In October and November 1871 I insured for two different owners a portion of the value of the ships the "Sea Pink" (a colonial ship,) and the "Baringa" (an iron high-class ship). Both loaded from the Tyne and both sailed for Bombay and neither of them was ever heard of; being well formed ships and well owned ships, the conclusion in my mind always has been that they were burnt.

1298. Do you know what the name of the coal was?—No, I do not. I am under the impression that I have seen in a parliamentary paper a list in which those two ships are included, where the name of the coal is given. So, assuming that those two missing ships were burnt, my experience embraces five ships, one of which was from Birkenhead, and four from the Tyne.

1299. You have been concerned with considerably more ships from Birkenhead than from the Tyne?—As an owner.

1300. And also as an insurance broker?—Of late years I have been more concerned with ships from Birkenhead, but previously from the Tyne also.

1301. A large number of them being coal ships?—Yes, to all parts of the world.

1302. You spoke of the rate of insurance having very much risen on coal. Does that high rate maintain itself at the present moment; has there been no fall lately in the rate since coal fell in value?—I have not perceived it; on the contrary, I have found increasing difficulties. For instance, if you have a ship going out of Birkenhead valued at 5,000*l.* you can find an area of underwriters for that vessel at say, taking Bombay, 6*l.* 6*s.* per cent., but if you have a ship of 15,000*l.* value you either have to send to Land's End, or John o'Groats, or to Paris for insurances before you can get

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such an amount as that insured, except you pay an enormous premium.

1303. Are you speaking of coal ships ?—Yes.

1304. You said you had an idea of ceasing to insure your coal ships and of spreading the risk among a limited number of friends. Would not you in such a case have very much higher wages to pay ?—No the sailors never question whether you are insured or not. I should think if anything I ought to pay lower wages; they would infer that my ship was so well found that I was prepared to take the risk of her.

1305. It would not be an objection to the ship ?—No, on the contrary, I think the minds of people would be operated on by the fact that the owner has so much confidence in his ship that he will not insure her.

1306. (*Mr Fenwick.*) You have mentioned five cases where the ships have been lost or not heard of, can you inform us how many ships you have insured in your experience ?—I cannot; it must have been a great many.

1307. Having the great experience you have, and having insured so large a number of ships, it would be very desirable if you could tell us approximately the enormous amount you have insured as compared with the five vessels that have been lost ?—Having paid as much as 60,000*l.* in some years for premiums, and 40,000*l.* perhaps on the average for the last 20 years, I must have had to do with a great many coal ships in my time. Some of the ships would be only loaded partly with coals.

1308. (*Chairman.*) You have had a hand in insuring hundreds of coal ships a year ?—In some years, certainly.

The witness withdrew.

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MR. ROBERT JOHN HANCOCK examined.

1309. (*Chairman.*) You are a Bristol merchant, I believe ?—I am shipping manager to Messrs. Charles Hill and Sons of Bristol and Cardiff.

1310. You have had great experience in connexion with the shipping of coal ?—I have had many years experience in it.

1311. You have been concerned in shipping many thousand tons of coal, I believe ?—I have shipped about 75,000 tons of coal during the last ten years.

1312. What coal has that been chiefly ?—All the various kinds of steam coal shipped from Cardiff and Newport. I could not give you the names of the pits. I have also shipped several cargoes of smelting coals.

1313. Have you had any cases of spontaneous combustion or explosion ?—We have never had a case of explosion. We have had one case, not of actual spontaneous combustion, but of coals becoming very much heated. That occurred in the case of a ship called "Her Royal Highness." The captain, fearing to go on to Cape Horn with them, sold the coals at Monte Video, finding that the expense of landing them and re-shipping them would be more than they were worth.

1314. That is the only case within your experience? —Yes.

1315. That being so, what precautions do you take with respect to ventilation, or with respect to the shipping of your coals ?—We always carry our coals on a platform from 16 to 20 inches from the bottom of the ship, so that there is always a cool place, and then the bottom of the coals does not get wet. I think wet is a great cause of spontaneous combustion.

1316. Have you any special ventilation ? Have you any ventilators up to the hatchways ?—We put iron ventilators as far forward as we can get them, and as far aft as we can get them, down through the poop. We put a ventilator in each hatchway.

1317. Is your experience that that system of ventilation is sound ?—I can only judge from our ship's having gone free from accident.

1318. Have you compared your immunity from accident with the casualties of others ?—Only in a general way. I know that a great many of the ships that have been burnt have carried their coals upon the bare skin of the ship, and that they were very often wet at starting.

1319. I understand you to say that you would recommend that all coals should be carried on a platform ?—Yes.

1320. Have you any recommendation to make with respect to the manner of taking coal on board ?—I think if it were possible to take only large coal on board at first, so as to have in each hatchway a good pile of large coal, and no small coal, it would to a great extent tend to prevent spontaneous combustion.

1321. How has the coal generally been shipped in the ships of which you have had experience ?—By tips.

1322. Always by tips ?—Yes.

1323. You have had no experience of the system of lowering by barrows ?—None whatever.

1324. And yet you say that your coal has been remarkably free from accident arising from spontaneous combustion ?—Yes, I think I can fully account for the near approach to accident in the ship I have mentioned. She was a new vessel, and very flat in her bottom, and in putting the platform in, which we do as much to ease the ship as for ventilation purposes, we kept it as high as we thought she would bear it, and after she had been at sea a few days they found that she was top heavy. They took her back to Cardiff, and discharged part of the coals, and while the coals were lying on shore it rained very heavily, and the coals got wet. They took 600 or 700 tons out of the middle of her, and took up the platform; and when they put the coals in again they let them run down on to the bottom. I think that fully accounted for their heating, and I further think that if she had gone out with the original platform no accident would have happened.

1325. Do you think that that accident arose from the coal getting wet, or because there was no platform at the bottom of the ship, and consequently no ventilation ?—Both combined I think tended to produce the heating.

1326. Was there any special report sent to you of the chemical cause of the combustion; was any special survey held ?—No. We always instruct the master to ascertain the heat of the hold with the thermometer; in doing this the captain stated that he found the hold getting gradually heated, and at last he took the main hatches off and forced iron bars through the coals as far as he could get them, and on withdrawing the bars they were so heated that a match would ignite against them; he then felt satisfied that the coals were very near ignition, and he ran back to Monte Video.

1327. Was there any suggestion that there was much iron pyrites in the coal ?—No; but I have always understood that that is the cause of spontaneous combustion; this cargo that I am speaking of was smelting coal.

1328. Do you know what the coal was ? Do you know from what pit it came ?—No; it was shipped at Cardiff for Messrs. Henry Bath and Son, of Swansea; it was a cargo of 1,800 tons.

1329. So that in all your experience of coal shipped from the tips, you have been entirely free from casualty, except in a single case where the platform which you uniformly insist upon had been taken up, and where the coal remained on the bottom of the ship ?—Yes.

1330. And you attribute that one casualty to the neglect of that precaution ?—I do, entirely.

1331. (*Mr. Vivian.*) Have you ever shipped coal by box drops, where the box is lowered through the hatchway ?—Yes; at the commencement sometimes they are used in putting the first part of the cargo in.

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1332. Do you consider that system a good one ?—I think it must tend to lessen the force of the fall; the impact would be less in the bottom of the ship.

1333. You said that you preferred the main hatchway being filled up with large coal ?—I think it would tend very much to reduce the tendency to spontaneous combustion if care were taken in stowing the first portion of the cargo by putting in large coal.

1334. In shipping by the box you would attain that end ?—To a certain extent ; the box is not lowered down the entire depth ; you have a fall, perhaps, of 14 feet.

1335. The box is lowered frequently through the hatchway, is it not ?—I have never seen it lowered down to the bottom.

1336. Do you know the system adopted in the Alexandra Docks, at Newport ?—I was there the other day; we have a ship loading there. I saw that they had a new system, but I had not an opportunity of seeing it at work.

1337. (*Mr. Fenwick.*) You say that in all your vessels you have this platform at the bottom ?—Yes.

1338. Is that plan generally adopted among shipowners ?—Most vessels in the Callao trade have platforms for carrying guano cargoes.

1339. Is this platform erected solely for the purpose of ventilation, or for the purpose of raising the cargo up off the bottom ?—It is erected for the purpose of raising the cargo off the bottom, and also for the purpose of ventilation. I have here a sketch of it (*handing it in*).

1340. I understand that the platform is put in chiefly for your return cargo ?—Not necessarily; in going to India we always put those platforms under the coals, and we then take them up and make dunnage of them for stowing the homeward cargo.

1341. Is it your impressson that it is very desirable that the coal should be kept free from wet ?—I think it is very desirable. I know there is great carelessness in loading coal; people treat it the same as ballast almost, and they very often allow the rain to go down the hatchways.

1342. You appear to think it very important that there should be large coals in the hatchway ; why do you advocate the stowing of large coals in the hatchway ?—Because I think the danger of spontaneous combustion arises from the small coal. I do not think if large nobs were stowed in by hand there would be any such thing as spontaneous combustion. I think the fire generally takes place in the hatchways underneath the drop.

1343. (*Mr. Icely.*) What is the quantity of coal that you think you superintend the shipment of yearly ?—Some years it is considerably more than others ; last year we took a great deal of iron out. I have shipped 75,000 tons in 10 years ; some years it has been 7,500 tons ; in some years double that; and in some years it has not been so much.

1344. At what period was it that you had this one casualty that you speak of ?—It was in 1865.

1345. During the whole of the 10 years you have had but that one casualty ?—That is the only one.

1346. Have you during the whole of those 10 years had your ships ventilated ?—We have had them ventilated in the manner I have spoken of, never otherwise.

1347. Have you been in the habit of taking the temperature of the cargoes ?—The captains have been instructed in a general way to watch by the thermometer if there is any heating in the hold, and to keep the hatches off in fine weather.

1348. Are they instructed to try the temperature at any specified period ?—They have not been so instructed.

1349. They have been left to exercise their own judgment ?—Yes ; I think in future I shall direct the captains to record the temperature in the ventilators of each hatchway in the log-book at noon daily.

1350. (*Mr. Young.*) You have stated that you have shipped 75,000 tons of coal in 10 years, that would be 50 or 60 cargoes, I suppose ?—68 cargoes ; some of the cargoes have reached 2,200 tons ; we had one cargo last year, which we sent on our own account to India, which was on board for six months before it was sold.

1351. (*Dr. Percy.*) You spoke of the importance of protecting coal from wet ; what reason have you for believing that moisture renders the coal more likely to ignite spontaneously ?—Our only accident is one reason, and I gather that that is the opinion of people who are constantly shipping coals and also of coal owners.

1352. Is it an impression simply, or can you give any well-established facts in justification of that opinion ?—The only fact I can speak to is our own approach to an accident.

1353. You say the fire generally occurs underneath the drop—is that the place where the fine coal is likely to accumulate ?—Yes.

1354. (*Mr. Ferguson.*) With regard to your vessel that took fire, was it ascertained that water had got on to the coals in the voyage out ?—She made some water in the top sides, but the coals were very wet when they were reshipped.

1355. (*Mr. Duncan.*) It would be the water they got on shore, rather than from any water that they got in the ship that you would attribute the heating to ?—I think so.

1356. Therefore the platform in that case would not have very much to do with it ?—Only if there were no platform there would not be the advantage of the cool air under the platform, and the coals could not drain if they had any moisture in them.

1357. Did you have a shaft up from the platform ?—Yes ; up each hatchway.

1358. You had no ventilation among the coals by the side of the keelson or otherwise, above the platform ?—No.

1359. (*Chairman.*) You said that of late you have paid more attention to this question of spontaneous combustion of coal than formerly, and that you give more specific instructions to your captains ?—Some years ago accidents rarely occurred, but since they have been more numerous we have instructed the captains to see that the ventilators are kept clear during the loading.

1360. Though you have not lost a ship during 10 years from spontaneous combustion, the question has assumed greater proportions lately ?—There have been a great number of accidents, but whether they are greater in proportion to the amount of coals carried or not I do not know.

1361. At any rate, your attention has been drawn to it more prominently lately ?—Yes.

1362. The Chamber of Commerce of Cardiff has taken great pains with regard to this question ?—Yes.

1363. Were you one of those who took part in the inquiry they made ?—Only in connexion with Mr. Hill. I am frequently in Cardiff, and he and I have talked this matter over together; he is president of the Chamber of Commerce of Cardiff.

1364. As president of the Chamber of Commerce of Cardiff, with your assistance, he has gone a good deal into this question ?—Yes ; he has furnished some statistics of the number of vessels lost.

1365. Am I right in saying that you are acquainted with the statistics which Mr. Hill has prepared ?—I have seen them. I saw them before they were printed.

1366. Did Mr. Hill consult you in the preparation of those statistics ?—He was about them when I went to Cardiff, and they were put before me. I went over several figures with him.

1367. Is the result of the inquiries made by your firm, which Mr. Hill in his capacity of president of the Chamber of Commerce has put forward, that the per-centage of loss by spontaneous combustion, whether measured by ship or measured by the quantity of coal, is not over one half per cent. ?—That is shown in this statement.

1368. That is over the whole kingdom ?—Yes, that is the result as shown by the statistics published up to October last.

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1369. On the subject of ventilation, is it the result of those inquiries that ventilation is very dangerous to one class of coal, and very necessary for another?—I do not think ventilation could be dangerous in any coal, but after a fire once occurred no doubt the ventilators would tend to feed it. I do not think ventilators would tend to cause the fire.

1370. Do you know whether there is any increase in the premium which is now charged on these risks?—I know that we pay very little more.

1371. Can you recollect what you used to pay?—I do not think that we are paying any more than we did seven or eight years ago.

1372. Is that because you have been so remarkably free from accident?—I take it that it is because we have been free from accident, and go to the same underwriters, and because we take a large amount of the risk ourselves.

1373. Your experience is that the rates of premiums on your coal-laden ships are pretty much what they used to be?—We do not pay more than one per cent. over, we should not pay more than one per cent. extra in taking a coal cargo as compared with any other cargo. I am speaking of voyages round the Capes for instance. We take a third of the risk always ourselves.

1374. Where are your risks taken?—In London.

1375. At Lloyd's or by companies?—Partly at Lloyd's and partly by companies.

1376. (*Mr. Fenwick.*) Is it the fact that you do not pay more than one per cent. more than you paid seven or eight years ago?—I may say positively that we do not.

1377. (*Mr. Duncan.*) Are any of your ships loaded with a general cargo from London?—Yes.

1378. What would be the premium on a ship loaded at Cardiff with a general cargo?—35*s.*

1379. What is the premium when you load the same ship with coals?—Coals to India would be higher at any time. When I said the premiums were not higher I should have said as compared with any other heavy cargo; the difference between the premium on a general cargo and the premium on a cargo of coals that we used to pay, did not arise so much from the danger of spontaneous combustion as from the weight. The underwriters prefer a general cargo to a weighty cargo. We do not pay more than one per cent. for coals over what we used to pay.

1380. That is to say, you are speaking of your Bristol Channel cargoes, machinery, and so on?—Yes.

In 1866–7 we paid on good colonial built ships with coals from Cardiff to the west coast of America - -	90*s.* to 105*s.*
And on similar vessels with salt to Calcutta - - - -	80*s.*
And with coal to Calcutta - -	84*s.*
In 1873–4–5 we paid on same class of ships with iron and machinery cargoes to West Coast of America - -	80*s.*
And with coals from Cardiff to ditto -	90*s.* to 105*s.*
One new colonial ship with 2,000 tons coals Cardiff to Valparaiso, was insured at - - - -	60*s.*
In 1874, on a colonial ship 9 years old, with coals, Cardiff to Cape of Good Hope, Galle, and a port of discharge in India we paid - - -	110*s.*

Most of these rates includes shifting to a loading port in England, or on to the Guano Islands after discharge of the outward cargoes.

The witness withdrew.

Mr. T. Beynon.

MR. THOMAS BEYNON examined.

1381. (*Chairman.*) You are a magistrate at Newport?—I am.

1382. What is your business?—I am a shipowner and coal merchant.

1383. I think you have had considerable experience in the shipment of coal?—Yes.

1384. Spread over a good many years?—15 years.

1385. Could you give the Commissioners roughly any idea of the number of cargoes you have been concerned in shipping?—I shipped perhaps 80,000 tons last year.

1386. In the course of your experience you have had several cases of spontaneous combustion at sea?—I have had one case in one of my own vessels, and two cases came under my jurisdiction as a magistrate, the "Minnie Graham" and the "Herbert Graham."

1387. The ship of your own that suffered in this way was the "Usk?"—Yes.

1388. We will take that first; in what year did the casualty happen?—She was a bark of 338 tons register, and she loaded a cargo of smelting coals at Swansea. That was in the year 1861.

1389. Was that cargo taken on board under favourable circumstances?—The weather was wet when the coal was shipped.

1390. How was the coal put on board?—The wagons were tipped into the shoot. The shoot rested on the ship's combings leading to the hold. The wagon was tipped up and it was eased off by the wing. The fall was gentle and the coal fell nearly perpendicularly.

1391. You say the weather was wet?—Yes, and the coal was wet, no doubt.

1392. What ventilation had you in that ship?—There were four pieces of wood running along very near the ship's bottom, and crossed with battens, and from this main shaft, as I may call it, there were similar shafts coming up through each hatchway.

1393. Was this a sort of a platform?—No, it was a kind of square pipe running through the ship, battened over.

1394. About how broad was it?—It was about 14 inches square, and there were shafts of the same description coming up through each hatchway.

1395. It would not protect the coal from wet if there was any wet in the ship's bottom?—No; personally, I have no faith at all in the ventilation of smelting coals.

1396. Do you know from what pit this coal came?—I do not know. It was the usual smelting coal. It was shipped by Messrs. Bath and Sons.

1397. How long had the ship been out?—88 days. They were 200 miles south of Valparaiso when they first discovered it.

1398. Were there any circumstances of special note in the way it was discovered?—The men complained one morning of great heat in the forecastle. The captain went down, and on opening the forehatches, a flame burst up as high as the foretop, and they immediately abandoned the ship. They had to get out a boat before the rigging was destroyed.

1399. They saved themselves?—They were picked up two days afterwards.

1400. This occurred about 14 years ago?—About 14 years ago. There is one circumstance connected with that ship that I would like to mention as showing the difficulty in accounting for spontaneous combustion. On the previous voyage she loaded a similar cargo precisely at Swansea; the captain went out, and having got as far as Cape Horn, he brought the ship right back to Newport, and he said that the Almighty had come on board at Cape Horn and warned him not to go any further. I came up to see the underwriters and they said they did not think they were bound to carry on the insurance, but looking at the circumstances of the case they would do so, but they asked me to discharge the ship so as to see whether the coals were heated. I discharged the coals out of the main hatchway, and the coal was thoroughly cool from top to bottom, and the ship went out and discharged her cargo with perfect safety.

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1401. It was not commanded by the same captain after ?—No.

1402. That is 14 years ago, and that is the only instance of spontaneous combustion that you have had in your own ships ?—Yes.

1403. Was there any inquiry in that case ?—None whatever.

1404. The other two cases you have mentioned are the "Herbert Graham" and the "Minnie Graham ?" Yes.

1405. And those came before you as a magistrate ?—Yes.

1406. You were one of the members of the court of inquiry ?—Mr. Hewertson, the then mayor, and myself were the magistrates.

1407. We have the minutes of the inquiry before us, but will you give shortly the leading features in each of those cases and state the impression they produced upon your mind ?—Samples of the coals were produced in court similar to that with which the ships were loaded ; they seemed to have come from several mines ; they appeared to be an inferior sort of coals altogether.

1408. Those casualties occurred at the end of 1873 ?—Yes.

1409. Will you first take the case of the "Herbert Graham ;" that inquiry I believe you held in the early part of 1874 ?—Yes. She left Newport, I see, on the 1st July 1873.

1410. How long was she out ?—From recollection, I think about 100 days.

1411. What coal had she on board ?—She had coal from different mines ; it seemed to have contained a great deal of iron pyrites.

1412. Was it all steam coal ?—It was all smelting coal ; what we call house coal at Newport.

1413. Where was she bound to ?—To Valparaiso.

1414. Do you remember how she was ventilated ?—No, I cannot say.

1415. What impression did the inquiry produce upon your mind as to the cause of the casualty ?—The impression that the inquiry produced upon my mind was that the coal was small, containing great quantities of iron pyrites, and I think it was shipped when the weather was wet.

1416. Do you remember how it was shipped ?—By the tip, something similar to the mode I described in the case of the "Usk."

1417. Was it stated in evidence that the coal must have been wet ?—I think it was. I think it was so stated in the evidence of the shipper.

1418. Then is it your impression that the system of shipping by tip is objectionable ?—No, I do not think it is. I do not think you could find a better mode of shipping coals than that.

1419. Then what precaution do you think was omitted in the case of the "Herbert Graham" ?—I do not think there was any precaution omitted at all, I believe it is only a question of time when such coals will take fire. If a cargo of smelting coal were shipped under the same conditions you could calculate almost to a day when they would take fire.

1420. What do you mean by the same conditions ?—If similar coal was shipped damp.

1421. Your opinion is that smelting coal if shipped damp will to a certainty fire after a certain time ?—I think it is almost inevitable unless it is shipped in a different manner than shipping it in bulk.

1422. And your opinion is that no method of ventilation will guard against casualty in the carriage of such a cargo shipped under those conditions ?—I think not, because the coal is so small and it gets so hard that ventilation would be useless ; it would not penetrate through the cargo.

1423. All that you recommend is that greater precautions should be taken to ship the coal dry ?—Further than that, I would recommend that the coal should be shipped in casks or barrels. I think that would be a sure preventative against spontaneous combustion.

1424. You would advise that it should be made a law or regulation to ship all smelting coals going to a great distance in cask or barrels ?—Yes, it does not follow that they should be very sound barrels ; the object would be to isolate the coals by separating them into small quantities. I believe that shipping the coals in barrels would be a sure preventative. In the case of the "Herbert Graham" the chemist that came down from the Board of Trade (Mr. Vassard) stated that carbonic acid or sulphurous acid, I forget which, if generated in proper quantities and introduced into the hold of a ship on fire from spontaneous combustion would put out the fire, and he put before us a very elaborate plan of introducing pipes into a ship's hold, running fore and aft in the sides and in the centre of the ship and they were to be perforated. We knew when the inquiry was finished that the "Minnie Graham" had been lost, and he stated that if the inquiry on the loss of that vessel was held in Newport he would bring his apparatus down and show us the manner in which he proposed to put the fire out.

1425. Was the "Minnie Graham" lost about the same time ?—She was lost a little after. They were both loaded in Newport with the same coal, and they were both lost under similar circumstances.

1426. I understand you to say that in both cases the coal was shipped damp ?—I think it is in evidence that the coal was shipped damp.

1427. Did this gentleman bring his apparatus down ?—He did, and during the inquiry he got a large fire made up in the court house, and he got a piece of wood to fit the front of the fire to prevent the air getting into it, he got his gas made and introduced it into the fire, but he nearly set fire to the court house ; he did not put the fire out.

1428. Do you mean it was his recommendation that all ships carrying smelting coal or house coal to distant places should be obliged to be fitted up with this special machinery ?—He recommended this system and I have no doubt it might answer if the air was totally excluded. I do not know whether it would or not ; but I mentioned to him my method of stowing the cargo in casks and he agreed with me.

1429. The members of the court of inquiry did not endorse his recommendation ?—Not under the circumstances, we had to put the fire out.

1430. I take it that your judgment is, so far as your experience enables you to give an opinion, that all that is required to be done in the case of smelting and house coal going on a very long voyage is to ship it in casks ?—I think all coal liable to spontaneous combustion, that is to say, containing iron pyrites and shipped in damp weather ought to be shipped in casks.

1431. Would you extend that to steam coal ?—No, I have never seen a well authenticated account of pure South Wales steam coal taking fire by spontaneous combustion.

1432. Do you know of any cases of spontaneous combustion in north country coal ?—No, but I know that during the high prices in South Wales they shipped coals as steam coals that were not steam coals, and some of the ships carrying those coals took fire. I do not think that a cargo of South Wales first-class steam coals would take care by spontaneous combustion.

1433. Would you apply a different mode of ventilation to steam coal from that which you would apply to smelting coal ?—I have always found in loading my ships with steam coals that if I keep the hatches off for a certain time after they are loaded it is a sufficient safeguard, and if a ventilator is introduced through the poop into the hold and another through the other hatchway it will carry off all the gases.

1434. Without any longitudinal method of ventilation ?—Yes.

1435. To keep the surface of the coal well ventilated ?—To keep the surface of the coal well ventilated, and I should allow no one to go into the hold without a safety lamp.

1436. That is more to guard against explosion ?—Yes.

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1437. From your experience those are the recommendations which you would give to the Commissioners?—Yes, I believe what I propose would be a sure preventative, and a very trifling matter of expense.

1438. Have you any other suggestion to make of the same character?—I feel so convinced in my own mind that the precautions I have suggested would be a preventative that I do not think any other precaution would be necessary.

1439. Would it be your advice that those precautions should be matters of law or would you leave it to the interest of the merchant to adopt them?—I am very much adverse to compulsory legislation. I think it might be left to the intelligence of the underwriters and shipowners if the underwriters say we will not insure a ship under a certain premium unless the coal is shipped in casks that would meet the case I should think, but if it would not I would rather see a compulsion for the sake of insuring safety.

1440. You would trust in the first instance to the publicity which perhaps the report of this Commission might give to some desirable rule than make it a question of Parliamentary enactment?—I think so at first.

1441. Have you any information to give us with regard to the present premiums that are charged on the shipment of coals from your part of South Wales?—It would depend on the ships. I should think that the premiums paid on ships going out with coals from South Wales to the west coast and coming back with nitrate would be 100*s.* per cent. if the owners are known to the underwriters.

1442. Has there been any change during the last few years?—The tendency is for premiums to go up especially on coal-laden ships; upon a ship which has just gone out from the north I had to pay 10 per cent. on her passage outwards—that was from Sunderland to Calderan.

1443. Was she a good ship?—She was a good ship because I ensured her at 80*s.* on the previous trip on the round.

1444. Was that with a coal cargo?—Yes, it was smelting coal from Newcastle in each case.

1445. Have you had any casualty from fire in ships of your own sailing from the north?—No.

1446. Is it your experience so far as South Wales is concerned that there has not been any serious rise in the premiums charged on good coal ships?—I have not insured a ship since the late losses, but I think the underwriters charge more on ships carrying coals to the west coast of South America.

1447. Both from South Wales and the north?—Yes.

1448. You have not had to insure a ship yourself lately?—Only one from the north.

1449. You have not had to insure a ship yourself from South Wales?—Not since the inquiries into the loss of those ships—not since the early part of 1874.

1450. (*Mr. Vivian.*) You say you consider that smelting coal will always take fire; do you mean that all sorts of smelting coal are liable to spontaneous combustion?—The common smelting coal that contains iron pyrites I think is very liable to take fire if it gets damp, but about the same time that the "Minnie Graham" and the "Herbert Graham" were loaded at Newport, I loaded a ship with steam coal mixed with large coal. I put two trucks of small coal to one of large coal. She went out to Coquimbo; it was Monmouthshire coal, which is a semi-bituminous coal; and I had a letter from the company to whom I sold the cargo in which they said it was the finest smelting coal they had ever had, and it turned out perfectly cool. It was in the proportion of two trucks of small coal to one truck of large coal.

1451. What coal was that?—A coal called the Tilleary coal; we shipped it as Beynon's black vein smelting coal.

1452. That is a case of smelting coal not liable to spontaneous combustion?—There was no casualty in that case. It was what they term a pure smelting coal. It was an experiment on our part. We recommended the coal, and it turned out equally good coal as the other. I suppose being a pure steam coal it did not contain so much iron pyrites.

1453. Very large quantities of smelting coals have been shipped from South Wales?—Very large quantities.

1454. Can you tell at all what proportion of those coals has caught fire?—I could not say from memory.

1455. But nothing like the whole?—Nothing like the whole.

1456. It is hardly correct to say that all smelting coals must necessarily catch fire, is it?—I qualified my answer by saying that I believe that all smelting coal that contained a large quantity of iron pyrites and which was shipped during wet weather would inevitably take fire.

1457. Are you aware that some of the smelting coals shipped from South Wales have never taken fire?—Yes; the "Usk" on a former occasion loaded a precisely similar cargo, and after being out 154 days she arrived safely at her destination.

1458. What coal was that?—It was shipped by the same firm.

1459. The Messrs. Bath purchase coal from many different people, do they not?—Yes; I do not know what the coal was. I know they shipped both cargoes.

1460. The origin of the two coals might not have been the same?—No.

1461. Are you aware that there are different kinds of smelting coal shipped at Swansea?—Yes.

1462. And some of those have never been known to take fire?—I know that several ships loaded with smelting coal from Swansea have never taken fire.

1463. You are not able to state what coals shipped at Swansea are liable to take fire, and what are not?—I could not specify them.

1464. What would be the cost of shipping coals in barrels?—As a shipowner, I have not gone into that question, but I would be glad to take a cargo of coals for 2*s.* 6*d.* more per ton, I finding the barrels.

1465. Are you aware what the value of barrels is?—I do not know what they would be in large quantities. I should think empty flour barrels, or old hogsheads or sugar hogsheads, would be the proper things to use for the purpose.

1466. A sugar hogshead is worth, I think, 5*s.* or 6*s.* What quantity of coal would a sugar hogshead carry, probably about 7 cwt. or 7½ cwt?—I am not able to say how much it would carry. It would add materially to the cost if that is the case. I do not think the old barrels would cost as much as 5*s.*; a trade would spring up in them no doubt. If you want hogsheads for a certain purpose, you have to pay more for them than if you took thousands of them every year.

1467. Are there many old barrels to be got at the shipping ports?—I should say there is an immence number at Liverpool.

1468. How would you get them from Liverpool to Newport?—I would bring them down.

1469. That would cost a great deal?—I have not gone into the cost. I am only saying what I am firmly convinced would be a sure preventative against spontaneous combustion. I think it would be much cheaper on the whole than the scheme recommended by the chemist, who propose to introduce iron pipes through the ship's hold, and pass carbonic acid through them. I think it would be a difficult matter to make carbonic acid in a gale of wind at sea. I believe if the coals were stowed in old bags even, it would prevent spontaneous combustion, so long as they were not stuck down close together; so long as there are interstices between the bags it would be a preventative, but storing the coal in bags would not be so good a plan as stowing it in old casks.

1470. Do you think that it would be possible to get the number of old casks requisite, even supposing the expense were not too great?—Yes.

1471. The casks would have to be brought from other ports?—They might make them at the ports where the coal was shipped.

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1472. A new hogshead would be a very expensive thing?—It depends upon what kind of hogshead you have.

1473. At all events you have not gone into the question of the cost?—No.

1474. You know no doubt the system adopted of shipping coal in the new Alexandra Docks at Newport; I believe under that system the coal has a short distance to fall?—It all depends on the depth of the ship's hold.

1475. Do you know whether provision is made for avoiding the fall from the ship's deck to the keelson?—I have not seen it in operation in the new dock. I have not seen them shipping coal since the docks have been opened. In the old dock we generally adopted the plan of lowering 50 or 100 tons in baskets to break the fall of the others.

1476. The old system of shipping coals was lowering a certain quantity in baskets, in order to break the fall of the others?—Yes, and they adopt that plan now in soft coals—they lower them through the hold.

1477. Can they ship a considerable quantity by baskets in a day?—Yes, but it is more expensive.

1478. What quantity can you ship in a day by baskets?—About 400 or 500 tons.

1479. Is it quite practicable to fill the hatchway up to a certain height by baskets and then to commence with the ordinary coal drop?—Yes, that is done generally with deep ships where there is a heavy fall; they lower a certain quantity down by baskets. I think, with regard to the royal mail steamers, they made it a contract in their charter that the coal should be so lowered.

1480. You cannot state what coal it was that caught fire in the two cases you gave, namely, the "Herbert Graham" and "Minnie Graham"?—In the case of both the "Herbert Graham" and the "Minnie Graham," three or four different sorts of coal went to make up the cargoes; they did not buy all the coal from the same man.

1481. Can you state what the different sorts of coal were?—I think it is stated in the evidence; it says in the case of the "Herbert Graham" that the stiffening was all brush coal, which came principally from the Mamhole Colliery; then there was small coal from Coalbrook Vale, and some from Cwm Serchen, and there was large coal from the Cwm Colliery at Aberbeeg; there were four different sorts of coal.

1482. And in the "Minnie Graham?"—In the "Minnie Graham" there were 60 tons 3 cwts. of Hope large coal, 20 tons 6 cwt. of Hope brush—I am not acquainted with these coals—and 323 tons of Tyrfilkin brush coal, so that there were four sorts of coal in the one and three in the other.

1483. (*Mr. Fenwick.*) Did you say that you paid 10 guineas per cent. insurance upon a vessel from the north to Coquimbo?—Yes, she discharged cargo in Dunkirk, and I sent to the agents in London to ask what they would get her insured for for the voyage, out and home, and the agents said I would have to pay the same as before, but when I came to settle it I had to pay 10 guineas on the balance of about 1,800*l.* on the voyage out only.

1484. What premium do you pay in loading small coal from Swansea?—I have not had many ships loading at Swansea, but I had an iron ship loaded with a cargo of iron in Newport in August last. She was launched at Stockton, and she came from Stockton to Newport with a half cargo of pig iron, and then she loaded a cargo of railway iron at Newport and a insured her for the voyage out to Talcahuano, and to come back to the United kingdom with nitrate at 100*s.* per cent.

1485. (*Mr. Icely.*) What description of coal is used for smelting purposes?—Bituminous coal—the small house coal.

1486. Is it the custom to screen it?—No.

1487. Is it what is called through and through coal?—It is not so large as through and through is as it comes from the pit. This coal would be coal out of which the large had been picked.

1488. It is all slack coal?—It was pretty nearly all slack coal that was shipped in those vessels.

1489. (*Mr. Abel.*) What is your reason for supposing that danger arises from the coal getting wet?—When coals get wet they get caked together, and becomes very hard, and a mass like that in the centre of the ship is a source of danger. It is the same with everything that is packed when wet; as it appears to me, it gets very hot after a time—a haystack will do so, or powder if stacked wet in the centre of a barrel, will generally get warm.

1490. You consider that coal containing iron pyrites, if it does not get wet, will not be liable to spontaneous combustion?—I do not think there is anything like the danger.

1491. You described the precautions that you would take to guard against explosions. Do you know any instance of explosion having occurred on a coal laden ship?—I have known several in Cardiff, but I have not inquired into the particulars. The mate of one ship, after the ship had been loaded, took a lighted candle into the hold and an explosion occurred. I do not know the particulars. The ship had been loaded four or six hours.

1492. Do you know the nature of the damage done by the explosion?—Part of the poop was blown away in that particular ship.

1493. Do you know of any instance in which fire followed the explosion?—No.

1494. (*Dr. Percy.*) I think you began by stating that personally you have no faith in ventilation?—Not for those smelting coals.

1495. Having stated that, you afterwards recommended a particular system of ventilation, did you not?—No; I stated the system I adopted, and I think in ships which I loaded at Liverpool, we were obliged to adopt that plan by the Board of Trade.

1496. You spoke of there being a great quantity of iron pyrites in smelting coal. How does the iron pyrites occur in smelting coal; is it in lumps or small particles?—In small particles.

1497. Did you see it distinctly?—We saw it in those samples before the court.

1498. Large heavy lumps of iron pyrites weighing half a pound?—No, it was distributed in the coal itself.

1499. Was any careful examination made to ascertain the quantity of iron pyrites?—Yes.

1500. You laid great stress on the importance of shipping this smelting coal dry. Have you known any cases of ships laden with smelting coal, shipped dry, taking fire afterwards?—I do not know of any.

1501. Do you know whether in those cases that have come under your own observation in which smelting coals had taken fire, the coals had been shipped when wet?—In all the cases it had been shipped wet.

1502. In all the cases of spontaneous combustion with which you are acquainted the coal had been shipped wet?—Yes; that is three.

1503. Do you know whether the same kind of smelting coal has sometimes fired and sometimes not fired?—I can only speak to the fact of my own ship being loaded by the same charterer in Swansea, the one time with wet coal and the other time with dry coal, and the time when she was shipped with wet coal she took fire.

The witness withdrew.

Mr. John Bladon examined.

Mr. J. Bladon.

1504. (*Chairman.*) What is your business?—That of a coal shipper at Newport.

1505. Have you been in that business for many years?—More than 25 years.

1506. Do you ship both large and small coal?—Yes, principally large coal.

1507. Will you tell us how coal is shipped generally at Newport?—It is shipped by means of a shoot

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which receives it from the truck and carries it to the hold of the vessel into which it falls.

1508. Are large and small coals both shipped in the same way?—Precisely in the same way.

1509. Is the fall considerable from the end of the shoot to the bottom of the ship?—No, the shoot generally rests upon the edge of the hatchway; the fall depends upon the size of the ship; in small coasters you would not have a fall of more than 6 or 8 feet; in a large ship the fall would be greater, it would be from 8 to 18 or 20 feet.

1510. Various kinds of coal are shipped at Newport?—Yes.

1511. Will you describe them?—There are two special descriptions, one is called the steam coal, which is used for marine and locomotive purposes, and the other is called house coal. The two coals are essentially different in their nature; the steam coal is a coal that is considered to be free from sulphur and tolerably pure, and is therefore not much subject to spontaneous ignition. The house coal on the contrary is a coal which contains a large proportion of sulphur. It is also of a softer nature than the steam coal, and it contains a larger proportion of iron pyrites, and we consider it much more subject to ignition spontaneously, than the steam coal.

1512. Do you include under the general term of "house coal" smelting and smithy coal?—Perhaps it might comprise both coals, but we ship very little coal for smelting purposes at Newport; it is shipped principally, I believe, at Swansea.

1513. What classes of ships are generally employed to carry those different descriptions of coal?—The large steam coal which is generally screened at the colliery and then taken down to Newport and re-screened there for the purpose of taking out the small coal that may have been produced by the journey you ship in the vessels of almost all classes. Coasting ships such as go to ports on the English coast where a large quantity of steam coal often is sent, would vary from 100 or 150 tons to 300 tons; vessels going to and from foreign ports will vary between 300 tons, or less probably, and a couple of thousand tons.

1514. With regard to the house coals, where do they come from principally?—They come from the same district as the other coal, but they are generally found in the upper series; the coals nearest to the surface are house coals generally.

1515. What are their names?—One coal is well known under the name of Mynyddyslwyn, another house coal well known in the district is called the Cwmtilery and there are some other coals that go by other general names, but the two I have mentioned are the principal ones.

1516. Will you describe the ships that are generally used to carry those coals?—The ships usually employed to carry those coals are principally for the coast, say from 100 to 300 tons burthen; occasionally they are sent foreign, but not often.

1517. As you have little or no smelting coal shipped at Newport, the amount of house coal or even steam coal shipped for long distances is small with you?—Comparatively small as regards house coal, but the reverse as regards steam coal.

1518. You said that you considered the steam coal much less liable to spontaneous combustion than the house coal?—Yes, and for this reason, the steam coals contain much less sulphur than the others, and of the two they are harder and also purer. The house coals contain apparently as you look at them a considerable portion of sulphur; the sulphate of lime, as it is generally termed, is apparent upon the pieces as you see them in the truck.

1519. What is the colour of sulphate of lime?—Yellow. Sometimes it has a silvery appearance.

1520. Do you mean there is sulphate of lime as well as sulphate of iron?—Yes.

1521. You say that the cargoes of steam coal vary very much, some being as low as 300 tons. Which do you consider most subject to spontaneous combustion the larger or the smaller quantities?—The larger the quantity of coal you have in the mass the more subject it is to spontaneous combustion. But I have never heard of any cargoes of large well shipped steam coal ever taking fire.

1522. Do you then attribute spontaneous combustion to moisture principally?—I do.

1523. It is in your opinion most important that the coal should be shipped dry?—Yes, with the view of avoiding spontaneous combustion.

1524. Have there been numerous cases, so far as you know of combustion, from the coal having been shipped in a damp state?—I have not heard of many such cases. I remember within the last year or two hearing of the spontaneous combustion of some cargoes of coal that had been shipped at Newport, those coals were red ash house coal, not white ash, or steam coal. The coal was a kind called brush or small coal. When the coal is taken out of the mine and brought out to the surface, the large nobs are put into the truck and sent down to Newport and shipped, and the inferior coal is thrown aside and dealt with either in the character of brush coal or small coal.

1525. You are now speaking of house coal?—Yes.

1526. House coal in your opinion is liable to combustion, and it is a danger to be guarded against if it is shipped damp?—Yes.

1527. Is it possible to prevent its being shipped damp?—It would be possible if you had no regard to the expense, but when the coal is brought to bank, and the large coal is taken away, the small coal is generally cast aside and put into a heap and left there till required for sale, when it is sent to Newport. It generally gets pretty damp as it lies on the ground, and when it comes to Newport we can never say that it is in a perfectly dry state.

1528. Have you studied at all the question of ventilation?—Not a great deal. There are very few ships that we load at Newport which are ventilated. I have known cases in which large American ships have been loaded with large coal, when great care has been taken in the ventilation, but as as a rule the ships are not ventilated at all that are loaded at Newport.

1529. Do you consider that ventilation tends to prevent spontaneous combustion?—I should say it would. When you get a large heap of heated small coal at the colliery you find the heat is not on the surface, but in the centre of the mass.

1530. Do you think that by passing a continuous current of air through it spontaneous combustion would be checked or promoted?—That, perhaps, might depend on the fact whether it was shipped damp or not. If it were shipped damp of course the ventilation would scarcely affect the condition of it so much afterwards. If it were shipped dry I should say ventilation would have a tendency to prevent spontaneous combustion.

1531. If the coal were shipped damp you would be rather afraid that ventilation would be likely to promote the fire, whereas if the coal were shipped dry you would encourage ventilation?—Yes, I think I should.

1532. Does spontaneous combustion often take place at the pit?—Not often; occasionally it does. When you stack small coal in very large quantities combustion at the pit's mouth is not at all an uncommon occurrence.

1533. Have you known many instances of spontaneous combustion of coal which has been stacked after purchase?—Small coal is never stacked before shipment by purchasers. A few years since, the Royal Mail Company used to purchase large coal screened and fit for use from the Risca collieries, and store it to the extent of several thousands of tons, 5,000 I believe, at Newport. The heap, as near as my recollection will enable me to judge, would be from 7 to 10 feet high, and I never heard of its taking fire, or being considered liable to spontaneous combustion at all. But instances of purchasers storing any coal at the colliery or elsewhere prior to shipment are rare.

1534. You do not know of many instances of coal

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taking fire spontaneously when in store or when stacked ?—I cannot speak to any such cases as having actually seen them myself.

1535. (*Mr. Vivian.*) Have you shipped any coal which has taken fire ?—I think not.

1536. (*Mr. Abel.*) Can you give any specific instances of spontaneous heating or actual taking fire of heaps of small coal ?—I am agent of the Ebbw Vale Company who possess many large collieries. The managers of those collieries have assured me that they had known not actual fires but heat to be generated in a heap of small coals which they prevented from actual firing by throwing the coal over a larger space.

1537. In all those cases the coal had become wet ?—Yes.

1538. Was it pyritical coal ? Was it house coal or steam coal ?—Steam coal. But all steam coals contain more or less pyrites as well as house coal but not to anything like the same extent.

1539. Did this heating in the heap of coal occur at any particular period of the year ?—I cannot say at what period it occurred. I think it occurred during the warm months, because the action of the sun and the wet upon the coals would be much more effective during the warm weather than in the cold weather.

1540. A large quantity of small coal is shipped, is not there ?—Yes.

1541. You do not remember any specific instances of fires with coal of that kind on board ship ?—No. We have sent and shipped from various works more than 200,000 tons in the course of a year.

1542. Are they all short voyages on which these ships sail ?—No, most of them go tolerably long voyages, the principal part of the coal being steam coal.

1543. Do some go to South America ?—Yes, and some to the West Indies and some to the East Indies.

1544. (*Dr. Percy.*) You said that some of the Ebbw vale coal when stacked in heaps was liable to take fire or to become hot ?—

1545. From what particular seams would that coal come ?—It would be small coal thrown together in a heap without regard to the particular seams from which it came.

1546. You cannot state whether the coal from one particular measure or seam is more likely to ignite spontaneously than the coal from another ?—I could not do that.

1547. As to patent fuel, have you known of a case of spontaneous combustion from patent fuel ?—I have not heard of any.

1548. (*Chairman.*) Is there any patent fuel shipped at Newport ?—No.

1549. (*Mr. Ferguson.*) Could you tell the depths of the heaps and the stacks which you say became heated or took fire ; is there any limit of depth at which combustion or heating would be preventible ?—I cannot give the depth at which heating would be likely to occur. I have seen heaps of coal 30 or 40 feet high.

1550. You said that large quantities were more likely to take fire than small quantities ?—Yes.

1551. But there is no limit of height at which you would draw the line ?—No, small quantities of small coal thrown together, say to a height of 6 or 8 feet, would not be liable to spontaneous combustion probably.

1552. (*Mr. Duncan.*) Did I rightly understand you to say that the 200,000 tons which you said you had shipped in the year were small coal ?—No, it was principally large.

1553. What proportion of it might be steam coal ?—I should say nineteen-twentieths.

1554. Properly screened steam coal ?—Yes.

1555. So that in speaking of the number of tons of coal that have gone in the year without accident, you were speaking principally of steam coal ?—Yes.

1556. And not of these house coals or the small slack which you think would take fire more readily ?—No ; in former days we shipped from a place called Abercarn considerable quantities of coals which contain this objectionable iron pyrites, but the shipment of them has been given up for several years that in giving a description of the two sorts I bear in mind what our workings were many years ago.

1557. I gather from what you say, that a shipper, like yourself, and those working the coals, are perfectly cognizant of the coals likely to generate spontaneous combustion ?—Yes, I think if you put the question to any shipper of coal, he would be able to tell you whether they were liable, more or less, to spontaneous combustion.

1558. They judge by their own observation ?—Yes, they judge from past experience.

1559. Seeing that we have had such a great number of cases of combustion on board ship lately, that is to say, in the years 1873 and 1874 as compared with what we had formerly, do you think it is likely that that increase may have arisen from the fact that when coals were very dear, a considerable quantity of those coals which used not to be shipped were put on board ship ?—I think not. I think that the shipment of small coals at Newport in large quantities is rather a new trade. I think that formerly the small coal shipped at Newport was generally considered smiths' coal and it was generally sent to the various ports round the coast, but the shipment of small coal for foreign districts is somewhat of a new trade. At Newport I know that I purchased for a London firm a cargo of 1,000 or 1,200 tons of this red ash, small, or brush coal, but that was an exception to our general trade. We are not in the habit of sending cargoes of small coal from Newport in ships of anything like that size.

1560. Taking you away from Newport, would it be a fair inference to draw, seeing that, as you have stated, coal shippers know that a particular kind of coal is likely to ignite and ought, therefore, not to be shipped for long voyages, that if there was a great pressure for steam coals, and high prices were being paid for them, it is possible that men may have been tempted to send away coals which they would not have sent away in former years ?—I hardly could go so far as to impute such an action to any of the coal shippers, there is no doubt that many fresh coals have come into the market within the last few years, but coal shippers have a pretty good knowledge of what they ship, they know pretty well whether the coals would be likely to take fire or not on the voyage.

1561. From your own knowledge you are not able to say whether it is possible that the cargoes were mixed sometimes when they were pressed for cargoes, that is to say, that steam coals were perhaps mixed with house coals which would not have been sent except under such pressure ?—I cannot say.

1562. (*Dr. Percy.*) I think you said that the Abercarn coals were likely to take fire ?—The steam coal I should say was not liable at all to take fire but the house coal which we formerly used to ship from Abercarn was liable to take fire.

1563. What was the name of the seam ?—We used to call it No. 1. We had another name for it in Welsh.

1564. You say that the shippers of coals generally know what kinds of coal are liable to spontaneous combustion ?—Yes.

1565. Can you state what kinds of coal in South Wales are particularly liable to ignition ?—I could scarcely do that. Reputation does give an evil name to many coals that are found in the neighbourhood of Swansea ; the intermediate coals lying between Swansea and Newport are generally supposed not to be so subject to spontaneous combustion as those at the far end, but I am not able to give you the names of the coals nor could I give you a general classification of them.

1566. Can you tell us how we can get the names of these coals ?—No ; the only suggestion I could make on that score would be to inquire from the inspectors of coal employed under the Admiralty. If they could not give it you I do not know anybody who could.

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1567. (*Mr. Vivian.*) Whence do you derive the unfavourable reports with respect to certain coals which you have mentioned ?—You hear them occasionally.

1568. From whom have you heard them ?—From people generally.

1569. Is there not considerable competition between the Welsh ports, between Newport and Swansea for instance ?—Yes.

1570. That is a point you would be jealous upon, is it not, that is to say, house coals shipped from the neighbourhood of Swansea ?—I spoke without any reference to the question of jealousy ; everybody would as a matter of course ship as well as they could.

1571. Do you remember a deputation of Newport house coal owners coming down to Swansea to beg that the competition might be lessened ?—I do not.

1572. (*Mr. Duncan.*) In your experience are the coals shipped now in as short a time after they are raised from the bowels of the earth as they were formerly ?—There is no difference whatever in that respect.

1573. They are not brought to bank and laid out on the hill and exposed to the weather, and then shipped some time afterwards ?—In a case of necessity where you have no sale for your coals of course you put them on bank, but as a rule the coals are taken direct from the pit's mouth to the ship, and in the case of house coals more particularly, you would even stop your work for the time being and not raise the coal unless you had the opportunity of shipping it at once, for if it were deposited on the bank it would deteriorate in quality. The worker of the coal, as a rule, gets rid of his large coal as soon as it is landed, by putting it in the trucks, and sending it away.

1574. Practically your experience is that they are sent away and shipped just as rapidly after being raised at the present time as they were 10 or 15 years ago ?—Just as rapidly.

1575. (*Chairman.*) The suggestion that the coal being shipped in haste is the cause of the greater number of casualties from combustion or explosion which have occurred of late years has no foundation in your mind ?—None whatever.

1576. (*Mr. Abel.*) In what way does the coal deteriorate in quality by being kept ?—All coal, more or less, suffers by exposure to the sun and air.

1577. In what way ?—The softer kinds of coal perceptibly becomes softer ; it becomes disintegrated, and so much has that evil been felt that in most contracts made between the coalowners and the large steamship companies, they stipulate that they shall have fresh wrought coal.

1578. The general effect is a breaking up of the coal ; that is the principal deterioration ?—Yes, I think so ; for instance, if you put a quantity of coal on the ground, and it contains a certain per-centage of small, when you come to take it up you will find that the small coal has increased more *pro rata* than it ought to have done.

1579. Can you call to mind a shipment of any coal that has become small by exposure ?—I cannot. As a rule we are not shippers of small coal.

The witness withdrew.

Adjourned to Tuesday next at 12 o'clock.

Tuesday, 29th June 1875.

PRESENT :

THE RIGHT HONORABLE HUGH C. E. CHILDERS, M.P., IN THE CHAIR.

HENRY HUSSEY VIVIAN, Esq., M.P.
FREDERICK AUGUSTUS ABEL, Esq., F.R.S.
GEORGE DUNCAN, Esq.
JOHN FENWICK, Esq.
JOHN FERGUSON, Esq.
CHARLES ICELY, Esq.
JOHN PERCY, Esq., M.D., F.R.S.
J. M. CARMICHAEL, Esq., Secretary.

Captain GEORGE STEINSON examined.

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1580. (*Chairman.*) You are coal inspector for the Peninsular and Oriental Company at Newcastle ?—I am.

1581. For how long a time have you acted in that capacity ?—Over 14 years.

1582. You also act for some of the public Departments in the same capacity, I believe ?—Yes, for the Admiralty, for the India Council, for the Crown Colonies, and in 1864, 1865, and 1866, I inspected for the Egyptian Government but not since then.

1583. Do you inspect the whole of the coal shipped on the Tyne for those Departments ?—Yes, on the Tyne and the Wear, and at Seaham as well.

1584. Can you give the Commission any statistics as to the number of ships that have been loaded under your inspection ?—I have loaded for the Peninsular and Oriental Company 1,147 ships, for the Egyptian Government, 95, for the Admiralty, 96,for the India Council and for various private parties 170, making altogether 1,508 ships.

1585. What was about the average tonnage of coal carried in each ship ? — The average tonnage 1861 was 770 tons ; in 1862, 867 ; in 1863, 706; in 1864, 960 ; in 1865, 841 ; in 1866, 841 ; in 1867, 864 ; in 1868, 776 ; in 1869, 780 ; in 1870, 919 ; in 1871, 1,093½. I have not the average for the other years, but, I suppose, it would be from 1,000 to 1,100 tons. As regards the ships for the Egyptian Government, the average would be about 500 tons, and in the case of the general ships it would be 600 tons.

1586. That is the average amount of coal carried in each ship under your inspection ?—Yes.

1587. I observe that the average has increased pretty steadily ?—The average has increased pretty steadily, but I think it has increased more lately on account of so many steamers being loaded with coal.

1588. All those are ships carrying coal foreign ?—They are all carrying coal foreign, to Malta, Alexandria, Aden, Bombay, Galle, one or two to Calcutta, Singapore, Hong-Kong, and Yokohama.

1589. Those are for the Peninsular and Oriental ?—Yes.

1590. For the Admiralty, where would the coal be going ?—For the Admiralty, it would be principally going to the dockyards ; some would be going foreign.

1591. The coal for the Egyptian Government would be foreign, and the coal for the India Council would also be foreign ?—Yes.

1592. Have you had any cases of ships being burnt at sea from spontaneous combustion ?—I may first mention the case of one ship burnt in 1863 ; that was a very peculiar case. She was fitted with a ventilating shaft, not for coal, but for rice. The owner fixed a fan to keep a current of air over the rice at sea so as to keep the rice cool. That ship was burnt within 60 miles of Galle. I took notice of that casualty, and I made inquiry about it, and I found that the ship took out a cargo of coals from Newcastle, and she got aground somewhere on this coast, and the cargo was landed and was on shore a fortnight or three weeks, and I found that nearly all that time it rained, and the cargo was like muck when it was again put on board the ship.

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1593. Do you knew what the tonnage of that cargo was?—I do not.

1594. What was the name of that vessel?—The "Eliza Bulcke"; she was on a voyage from Newcastle to Galle.

1595. In that case the coal was landed, where?—Somewhere on this coast. I am only giving these particulars from second-hand information. I asked the captain about it, and the captain said that was the case. I know the cargo was put on shore, and I know it was very wet when it was again put on board.

1596. Was it your opinion that this fan contributed to the danger?—No, not at all. That was in 1863. Then I know of no other casualty having occurred in any vessels that I have had to do with till 1872 when the "Bernice" was burnt.

1597. What where the particulars of that case?—She loaded with 1,869 tons of Davison's West Hartley, in 1872. She was burnt very near Bombay. She was a very large wooden ship, and a very fine ship, but I have not any particulars about her ventilation. We did not think anything about ventilation or anything of the sort then.

1598. No particular precautions were taken as regards ventilation in her case?—She was ventilated. All Liverpool ships must be ventilated.

1599. How?—By shafts up and down each hatchway and fore and aft shafts alongside the keelson in the way Mr. Duncan's ships are ventilated.

1600. Have you any particulars with regard to the sort of weather it was when she was loaded?—

1601. Do you remember how the coal was put on board?—The coal was loaded into the ships by a lowering apparatus which we then used, but in loading Davison's West Hartley, they generally have to ship very quickly. There is not much accommodation for loading, and they have to ram the coal in as hard as they can.

1602. Would the coal fall any distance in loading?—It was loaded with a box at the end of the spout.

1603. The bottom of the box would open?—Yes.

1604. So that there would be no great fall for the coal?—There would be no great fall, but we could not often use that apparatus in the hatches.

1605. To what do you attribute that casualty?—I do not know how to account for it. There are many ways of accounting for it. It may have been from pyrites, or it may have been accidental.

1606. Was the coal double screened?—No; none of the coal is double screened.

1607. What sort of inspection would there be with a view to prevent the introduction of coal which was full of pyrites?—In those days they worked night and day, and even if I had been there at night, I could not have seen whether pieces of coal went in full of pyrites.

1608. So that coal containing pyrites might have passed, which in the daytime you would have rejected?—It might pass even in the daytime when I could not see it, because it goes so quickly over the spout.

1609. Are there no precautions taken at the pit's mouth to prevent coal containing much iron pyrites being shipped?—Yes, it is all screened there, but there may be big lumps of coal containing pyrites, the existence of which may not be discovered until the lumps are broken.

1610. You can assign no special cause for that casualty?—No.

1611. What was the next case?—The next was the "Oneiza."

1612. In what year?—December 1872. She was merely missing.

1613. Then the next?—The "Shalimar," she was a very fine new ship.

1614. In what year was that?—In 1873. She loaded 1,942 tons of steam coal, 100 tons of coke, and 148 tons of wall bottle nuts. She was never heard of. She was ventilated in the old-fashioned way with a large platform over the keel. She was rather crank, and her cargo was rather high on account of the platform. She was chock full.

1615. What was the next case?—The "Moorsfoot," an iron ship. She loaded in November 1873, with 1,664 tons of Howard's West Hartley.

1616. Was she burnt?—Yes, I have not the particulars of the case. I merely know that she was burnt. I saw the particulars in the paper.

1617. All you know of that case is that it was a cargo of Howard's West Hartley coal. Do you know anything about the particular pit or the particular seam it was from?—It is all from the same seam. They do not work a great deal of coal.

1618. All the coal of that name is practically the same coal?—Practically the same coal.

1619. Of the circumstances of that loss, you know nothing?—I know nothing of the circumstances.

1620. How was the ship loaded?—She was loaded by the apparatus. The lowering apparatus was used at that time, but the men did not handle it so well as they did formerly. They were beginning to be masters then. They have almost got to be masters now altogether. In that shipment the coal was very much broken.

1621. You think at that time the coal was not handled so well and so carefully as it used to be?—I think so.

1622. What was the next case?—The "Euxine." She was an old Peninsular and Oriental paddle-steamer turned into a sailing ship. She loaded in June 1864, with 2062 tons of Howard's West Hartley.

1623. Precisely the same coal?—Yes.

1624. Was she loaded in the same way?—Partly, not altogether; she was very high, and we had great difficulty in getting the cargo into the ship at all. The coal got very much broken in going in.

1625. It had to fall from a great height?—Yes, she was a very deep ship.

1626. Whose ship was she?—She belonged to somebody in Liverpool; I do not know the owner's name.

1627. Have you any further particulars about that case?—There was one thing that struck me about her; the captain had a quantity of ale on board as a speculation and this ale was stowed on the top of the coal. We know that sailors like ale, and some of them might have been at the ale, and in doing so might have set the ship on fire in the same way as, no doubt, the "Cospatrick" was burnt.

1628. Beyond that you know nothing of the particulars of that case?—No.

1629. What was the next?—The "Oliver Cromwell," a North-American built ship, and then there was the "Calcutta," both belonging to the same owner and both loaded about the same time; in fact, the "Euxine," the "Calcutta," the "Oliver Cromwell," and another ship called the "Cora," were all loaded together.

1630. With what coal?—The same coal, Howard's West Hartley.

1631. From the same pit and the same seam?—From the same pit and the same seam.

1632. What information have you about those vessels?—The "Oliver Cromwell" was ventilated. The captain came and asked me how I wanted the coal ventilated, and I said, "I do not want the coal "ventilated at all, it is no business of mine." On that he said he was obliged to ventilate by the charterparty. I said, "Then ventilate according to the "charterparty but, as far as my own opinion goes, I "would not ventilate at all." He further said, "The owners say she must be ventilated." "Then," I said, "ventilate according to the owners' instructions." So he did ventilate according to the owners' instructions, with shafts up and down each hatchway, connected with fore and aft shafts. The "Oliver Cromwell," the "Calcutta," and the "Euxine," were all ventilated, but the "Cora" was not ventilated.

1633. Were all those vessels lost?—The three that were ventilated were lost, that is to say, the "Oliver Cromwell," the "Calcutta," and the "Euxine;" the "Cora" which was not ventilated was not lost. The

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captain said he did not want any through system of ventilation, and I gave him my ideas of ventilation and he said those were his own ideas about it, that is to say, plenty of ventilation through the deck, plenty of ventilation on the surface of the coal, but no ventilation right through it.

1634. Do you attribute the loss of the other three ships to the fact of their being unnecessarily ventilated?—I do; the "Cora" was loaded along with those other three with the same coal, sometimes one ship was taking in coal and sometimes the other; they got identically the same coal.

1635. Nothing happened to the "Cora?"—No; in the case of the "Cora," when she was shipping her cargo there was a heavy thunderstorm, and the rain came down in torrents and she got a wet cargo. The other ships had dry cargoes.

1636. Nevertheless, the three ships which had dry cargoes and which were ventilated, were lost, whereas the "Cora" was saved?—Yes; the captain was a shrewd American, and he and I put our heads together, and after having a conversation about it he decided not to ventilate her at all.

1637. What other cases have you?—The next ship was the "Broughty Castle;" she is missing; she was an iron ship belonging to Dundee.

1638. Where was she loaded?—She was loaded in the Tyne in December 1873 with West Hartley main; she was a splendid iron ship.

1639. Nothing has been heard of her?—No.

1640. Was her coal wet when it was put on board?—I would not be quite sure. She was loaded at Christmas time; she was not loaded with the lowering apparatus.

1641. Was she loaded from the tips?—Yes.

1642. Was the coal from any particular seam?—It was splendid coal, West Hartley Main, the Low main coal.

1643. What is the next case?—The "Ontario." I believe she took fire; she was not burnt altogether; she was loaded in August last year.

1644. For what port was she bound?—Bombay, she was carrying out coals for the Peninsular and Oriental Steamship Company; she took fire in Bombay harbour, or somewhere near the harbour.

1645. Have you any other case?—The "Poco hontas" which was loaded for the Indian Council in June 1874.

1646. She was loaded with what coal?—Hastings Hartley, 1,580 tons. She was an American wooden vessel.

1647. Can you give us any particulars about her loading? was she loaded from the tip?—Yes, she was not loaded with the lowering apparatus, she was loaded by another plan which we sometimes adopt. We lay a flat platform upon the lower beams, and then the coal lies upon this platform and falls easily down to the hold; there is very little fall by adopting that plan. We make the fall as little as possible.

1648. What happened to that ship?—She was burnt at sea.

1649. Was she ventilated?—She was ventilated with shafts up and down. I cannot say whether fore and aft. I have a suspicion about the cause of that casualty from what I saw myself when the ship was loading. The captain was always going about with a pipe and lighting it with matches and throwing them down, and I said to him, Do not you think that that is rather dangerous?

1650. What is the next case?—Then there was the "Sidra," a steamer, she is missing, she was bound for the Mediterranean, and she carried 1,850 tons of Carr's Hartley. There is nothing to lead to the conclusion that she was burnt. I fancy she has gone down.

1651. Do you know whether she was fitted with water ballast?—Yes, I believe she had water ballast.

1652. You have no reason to think she was lost by fire?—No.

1653. She is a missing coal ship?—Yes.

1654. Will you mention any other case that you are acquainted with?—The "Indian Empire" one of Mr. Duncan's ships, she loaded a cargo of Howard's West Hartley, in 1873. I have not the particulars of whether that case, so as to be able to say she was burnt altogether or not.

1655. How many of the cases you have given us were missing, and how many were burnt at sea?—There were seven ships burnt at sea, and including the "Ontario" there were eight. Those are ships that are said to have been burnt at sea, that is all I know about them.

1656. You cannot state positively that those seven were cases of spontaneous combustion?—They call them cases of spontaneous combustion. They were burnt at sea, that is all I know about them.

1657. Summing up the number of cases you have given us there are eight cases of coal ships burnt at sea, or in harbour, after arrival, four cases of missing ships, including the steamer?—Yes.

1658. Then you have known a few more cases of accidental fire which were clearly not cases of spontaneous combustion?—Yes, three cases in the Northumberland Dock, one ship took fire in the sail room, that was the "Ettrick Dale," and she was very nearly burnt in the dock when she was under the spout. Another vessel took fire at night from some cause or another, the deck was burnt, and some of the burning timbers fell down and fired the coal. Another vessel took fire when the boys were taking in stores after she was loaded, and no doubt they caused the fire.

1659. Those three cases are altogether outside those you have stated before?—Yes.

1660. In none of those cases was the fire spontaneous?—No.

1661. Those eight cases of ships certainly burnt, and four cases of missing ships are out of how many altogether?—Out of 1,510 ships.

1662. Since what time?—Since 1861. In fact those have all been since 1872 except one case.

1663. Since 1872 you have had seven cases of ships lost by fire at sea, or in Bombay harbour, and three cases of ships lost at sea, cause unknown, but which may have been from fire?—Yes, the "Oneiza," the "Shalimar," and the "Broughty Castle."

1664. That is out of how many that you have superintended the loading of since 1872?—215.

1665. So that between 1861 and 1872 you had one, only one decided case of a ship lost by fire out of about 1,300 ships, whereas since 1872 you have had seven cases, and three possible cases out of 215?—That is so.

1666. That indicates therefore a very grave increase in the number of casualties from fire during the last two or three years?—Yes, but I do not believe they were all owing to spontaneous combustion. For instance, in the case of the "Calcutta" the other day, I looked over a list of the crew and there were not two of one nation, they were all mixed up together; I should draw from that the inference that there would be constant rows on board the ship; that they would be always quarrelling.

1667. And you think the ship may have been fired?—I think it not at all impossible that the ship may have been fired. I happened to see a list of the crew and I at once formed that opinion from looking at the list.

1668. You spoke of the charterparties in certain cases requiring ventilation, and you told us that you said to a captain if you are bound to ventilate, ventilate accordingly, but in a case where it was not stipulated that the ship should be ventilated, you talked the matter over with the captain, who was an American, and he arranged that the ship should be ventilated upon your principle, and you told us that in that case no casualty occurred. Will you be kind enough first of all to explain to the Commission whether in those cases where ventilation was required it was required by the Peninsular and Oriental Company or whether it was a matter of stipulation between the owners and the charterers?—The charterparty says that the ship is to be ventilated.

1669. Would the Peninsular and Oriental Company

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be a party to the charterparty?—The Peninsular and Oriental Company would be indirectly a party to the charterparty, but the chartering brokers make the charterparty. I do not know whether the Peninsular and Oriental Company insist upon ventilation or not.

1670. Do the Peninsular and Oriental Company charter ships directly, or do they arrange with intervening people who charter from the owners?—The ships are chartered through intervening people.

1671. So that the Peninsular and Oriental Company themselves do not require any conditions as to the ventilation?—I do not know whether they insist upon such conditions being put into the charter.

1672. At any rate you as representing them on the spot know nothing about any conditions as to ventilation?—No, I have no intructions about it.

1673. What is your opinion about the ventilation of ships carrying north country coal?—The other day a very large iron ship loaded with coal, and the owners were very anxious about ventilation, and they asked me about it. The ship belonged to Messrs. Hall Brothers, of Newcastle. I said that I had no intructions about ventilation. I said, "It does not seem to be decided " yet whether ventilation has a good or a bad effect, " but my own opinion is that it has a bad effect, " but I have nothing to say about the matter." Mr. Hall said, "I wish you would write to the Peninsular " and Oriental Company about it." So I wrote to the Peninsular and Oriental Company about it and said that the owners were very anxious to know what was to be done about the ventilation. They wrote back to say, "We leave it to yourself and the owners to make " the best arrangement you can." I went and saw Mr. Hall and he said, "What do you propose?" I told him what I would do would be this, I would have no through ventilation at all, but I would have a small shaft in each hatchway to let the thermometer down, so that the captain should not be surprised by flames coming up the main-hatch. He said he was not a practical man, but what I suggested was what he thought a very proper thing, the ship was newly-built and it has great square ventilators, three or four feet square, and ventilators fore and aft alongside the keelson full of holes. So the owners cut off all this ventilation, blocked up all the holes, stopped the connexion with fore and aft, and merely put small pipes down for the lowering the thermometers, and she took in 2,478 tons and arrived at Hong Kong all right.

1674. That opinion which you gave to Mr. Hall expressed your judgment that north country coal had better not be ventilated?—That is my opinion, that it had better not be ventilated.

1675. All you require is to take the hatches off?—I would have ventilation through the deck, as much open space through the deck as possible.

1676. To carry the gases off?—Yes.

1677. So far as regards ventilating the interior of the coal, you think it mischievous rather than otherwise?—It would not do the coal any harm if there were no pyrites in it, but if there is any pyrites present, it is almost sure to take fire, if the air gets at it.

1678. You would endeavour to keep the air from the interior of the coal, and only keep the exterior clear of gas?—Yes; I would ventilate the upper surface of the coal, no gas goes below, it all rises.

1679. Do you know anything of South Wales coal?—No. There is one case I forgot to mention. A foreign ship was missing some years ago, which was loaded partly with steam coal and partly with gas coal. I had to do with part of her loading. That gas coal was put in very wet. That was some years ago.

1680. You had not the entire responsibility of inspecting that?—No, I had nothing to do with the gas coal. She was so badly contrived that I would not let any more steam coal go into the after part of the ship, and so they filled her up with gas coal.

1681. As to the methods of loading, you have described the method of lowering adopted by you in the case of shipments for the Peninsular and Oriental Company, do you approve of that mode of loading?—I like it very much, but the men will not work it.

1682. Not at all?—No.

1683. How long is it since they have refused to work it?—This last two years.

1684. Is the coal now entirely loaded from tips?—No, I have a better plan than that, which I mentioned, by which you can do without the men at all. You lay a large platform of wood on the lower beams, and then the coal falls there first and forms a cone in the hatchway, then the coal slips off that cone, and then it has only the depth of the lower hold to fall. I have loaded a large ship in that way. It is better than the other way, and you do not have to depend on the men.

1685. Is that the way in which you are now loading all coal?—Yes, we try to save breakage as much as we can.

1686. When did you begin to load in that mode?—Partly ever since I have been on the Tyne, when we could not adopt the other plan.

1687. What is the greatest fall where you adopt that mode of loading?—It depends on the depth of the ship. We had a ship the other day which had a 20-feet fall after the coal reached this platform. Her depth of hold was about 28 feet; she was a very large ship.

1688. When you say the men will not work the other mode of loading, what do you mean?—The men are masters now and they decline to do it.

1689. Why?—Because it is so much trouble.

1690. (*Mr. Duncan.*) What kind of coal was loaded on board that ship of Messrs. Hall?—Partly Bower's West Hartley and partly Bebside.

1691. That ship arrived at her destination quite safely?—Yes.

1692. And she had no ventilation whatever?—No; only pipes through each hatchway, by which to let down the thermometer. There were ventilators fore and aft alongside the keelson, but Mr. Hall had them all blocked up, and the connexion with the upright ventilators was all cut away, so that they were not ventilators at all. He blocked up the bottom of them.

1693. You had two or three large ships of mine last year?—Yes.

1694. They were ventilated, were they not?—They were ventilated.

1695. And they arrived safely at their destination?—Yes; it does not follow because they arrived safely that it was due to their ventilation. There may not have been any brassy coals in their cargoes.

1696. Is it your judgment that I was not wise in having the ships ventilated?—Some of your ships have taken fire.

1697. And those were ventilated?—Perhaps not, but ships that have been ventilated have taken fire. I argued the point with Mr. Stewart about ventilation, but he persisted in having the ships ventilated.

1698. Though you thought they would be better without it?—Yes. It is a curious thing that none of the north country ships that I have had to do with that have not been ventilated have been burnt. I was talking to an old captain, who told me that he had never had a casualty happen to a ship except in one case, and that was the only time he ever ventilated the ship, and he said he would not go to sea in a ventilated ship.

1699. Your opinion is that ventilation does harm rather than good?—Yes, with north country coal; you cannot have coal very well without pyrites in it; if you could get coal without pyrites in it you might ventilate as much as you liked.

1700. (*Mr. Ferguson.*) In the case of the American ship which went out safe, did I rightly understand you to say that the coal was wet when it was loaded?—Yes.

1701. So that though the coal was wet, and though the ship was not ventilated, the ship arrived safely at her destination?—Yes, a portion of the cargo was very wet.

1702. In the case of the other three ships that were

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ventilated, and which were burnt, was their coal loaded in dry weather or wet ?—In dry weather; there may have been just a thunderstorm.

1703. In the case of the ship which you spoke of last, where you advised the captain to do away with the ventilation, was her coal shipped wet or dry ?—Dry, I believe.

1704. You say the owners blocked up the ventilators ?—They cut off the bottom and filled them up at the bottom so that they did not act any longer as ventilators.

1705. You approve of ventilation from the upper deck upwards ?—Yes, above the coals.

1706. (*Mr. Duncan.*) Do you think the coals were as well cleaned during 1873 and 1874, when there were so many cases of burning, as they were formerly ?—No, I do not think they were.

1707. You do not think they were so freed from lumps of coal containing appearances of pyrites ?—I do not think they were so well cleaned.

1708. Do you think they are as well screened now as they used to be ?—The screens are much about the same; the screens are a little narrower.

1709. Do you think that the fact of coals having greatly risen in price would be a considerable inducement to the colliery owner to send coals less carefully screened than formerly ?—I do not think that.

1710. You think in the main they pass through the same process as formerly ?—Yes, there is one difference which I believe the Mines Regulation Act has had something to do with. Formerly the coals passed over the screens and fell into wagons, and all brasses were picked out before the coal went into the wagon, but if a piece happened to fall into the wagon there was a boy in the wagon to pick out any little bit; perhaps a lump would break and show a brassy appearance, and the boy could pick out the brasses and put them on one side. Now there are no boys there. I think the Mines Regulation Act may have made them afraid to put boys to work at that; at all events the boys are not there now as they used to be, so that if a piece of brassy coal fell into the wagon it would be likely to remain there.

1711. There was such a great demand for coals when the price was so high that there was no difficulty in disposing of almost any coals ?—No.

1712. (*Mr. Ferguson.*) Would you dispense with ventilation for all coals, or only for pyritical coal ?—I can only give an opinion about north country coals.

1713. (*Dr. Percy.*) I did not quite understand what you said about the rice-laden ship; was she laden partly with coal ?—No. She was a rice ship; she brought rice home.

1714. You spoke about a fan in that ship; do you know whether that fan would be effectual in preventing the heating of rice ?—It would keep a cool current of air on the top of the rice.

1715. Is rice liable to spontaneous ignition ?—It heats very much and spoils.

1716. The cooling of it by a draught of air prevents that ?—It is supposed to do so.

1717. Do you know of your own personal knowledge whether it does ?—No.

1718. The "Euxine" was a ship that took fire ?—Yes.

1719. And the coal which was shipped on board of her was dropped a long way down?—Yes.

1720. There would be a quantity of small coal accumulated about the spot where it fell ?—Yes.

1721. Was any examination made with respect to the particular place where the great fall occurred ?—I cannot tell you that. Falling a great distance very often exposes brasses which would be covered up otherwise.

1722. In another ship which took fire I understand you to say that the captain was generally to be seen smoking furiously ?—He was always trying to smoke, but his pipe never seemed to light.

1723. A fire occurred in that ship ?—Yes.

1724. Have you any evidence to show that the fire took place in the coal and not elsewhere ?—No, I merely know what I saw in the papers about it.

1725. You say that from 1872 numerous cases of what was supposed to be spontaneous ignition have occurred, has there been any alteration in the mode of loading, or in the ventilation, or in the kind of coals shipped between 1872 and the present time ?—There was a little difference in the loading in the case of those ships. An accident happened at the spout where Howard's West Hartley coal were being shipped, and we had to get the coal in anyhow we could for a while on account of that accident. Several ships were on at the time.

1726. Do you think the effect of that probably was to produce more small coal ?—Yes, I reported so at the time. The coal appeared to be very much broken.

1727. The kind of coal was the same ?—Yes.

1728. You said that between 1861 and 1872 there were very few cases ?—Yes.

1729. Is it certain that the same kind of coal was shipped in that former period as in the period when the numerous cases occurred ?—It was the same coal exactly.

1730. (*Mr. Abel.*) You ascribe the heating of coal on board ship entirely to the presence of pyrites ?—The firing of it; it will heat, but not take fire without pyrites.

1731. Will you tell the Commissioners a little more explicitly on what you base your opinion as to the undesirability of ventilation ?—I believe if coal containing pyrites is heated and a great pressure is put upon it, it merely wants fresh air to make it ignite. I submitted that opinion to a chemist at Newcastle, Mr. Glover, and he said it was a great chemical fact. That was my own opinion, and I got it confirmed by him as a chemist.

1732. You do not object to the circulation of cold air over the coal ?—No.

1733. Therefore, as far as that goes, you would consider ventilation to be advantageous ?—Yes, above the coal to let the gas escape.

1734. Have you ever known a case of explosion on board a coal-laden ship ?—Not with our north country coal. There is not sufficient gas in the coal to cause explosion.

1735. (*Mr. Icely.*) Were ships generally ventilated prior to 1872 ?—Many ships were ventilated, and the ventilators were generally full of coal before the ship was fully loaded. We could not get the ships loaded for the ventilators, though the ventilators were there they were full of coal. That was what first drew my attention to it. They were of no use because they were full of coal, therefore they did not seem to be necessary.

1736. You mentioned that the boys who formerly picked out the pieces of brassy coal no longer do so ?—Yes.

1737. At what date was the practice of employing boys to pick out the brassy pieces of coal discontinued ?—It has gradually been discontinued since the Mines Regulation Act came into force.

1738. When did the Mines Regulation Act come into force ?—Within the last two or three years.

1739. Those accidents have been more rife since the cleaning of the coal has been neglected ?—The men have been getting more careless in every way since then; since they got the big wages they have been getting more careless.

1740. Are you of opinion that the coal has been much more carelessly cleaned since 1872 than previously ?—I will not say much more carelessly; it is not so carefully cleaned.

1741. Have the places of these boys been filled by anyone else ?—There is a screener at each side of the screen; he is supposed to pick out the brasses.

1742. Did the boys supplement those men ?—The boys supplemented those men.

1743. Therefore there was a double check, now there is only a single one ?—Only a single one. I do not see much more brasses going in than formerly, and

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I have watched the coal sometimes over 12 hours at the end of the spout.

1744. Do you inspect the whole of the cargo?—I see as much as I possibly can. I cannot see the whole of it.

1745. Sometimes you have two or three ships loading at the same time?—Yes, but I have assistants.

1746. You have authority to reject dirty coal?—Yes, and I order it to be taken out of the ship sometimes when it is in.

1747. Is it the custom in ships which you load or inspect the loading of, for the trimmers to fill the coal right up to the square of the hatchway?—Yes.

1748. Is the square of the hatchway filled with small coal or large coal?—The small coal lies in the hatchway, and before we finish we separate the small all round and put a wagon or two of big coal in the hatchway.

1749. The small is not taken out?—No.

1750. Therefore, it in a measure seals the hatchway; it takes the place of the hatch when the hatch is off?—Before the hatchway is filled up they cut through on each side, and we tip a wagon or two in the hatchway at last so as to allow a circulation of air. In the case of a ship loading in Sunderland some years ago there was a lot of small coal being shipped the last thing into the hatchway, and I wanted the trimmers to take the small out of the hatchway, but they would not take it out.

1751. (*Mr. Vivian.*) Have you heard that there is a great difficulty in working the coal clean underground owing to the men declining to obey the orders of the overlooker?—No, I have not heard so.

1752. You said you had not remarked that there were more brasses in the coal now than formerly?—No, I think not.

1753. If that is so, to what do you attribute the increase in the number of cases of spontaneous combustion in coal-laden ships?—The brasses might be there without my seeing them; a big piece of coal will contain a brass inside, and you will not see it till it is broken.

1754. Are you aware that brasses exist chiefly in certain portions of the seam of coal?—They seem to be all over the coal. You cannot see the brass till the piece of coal is broken.

1755. You spoke of the difficulty which you have had with the trimmers. When did that difficulty begin?—There was a strike some years ago; they used to work night and day, and they struck for shorter hours. I think about two or three years ago.

1756. At the time when labour was so scarce?—Yes.

1757. Did they carry their point?—Yes; and instead of working night and day they knocked off at 5 o'clock.

1758. Since that time the moral effect of their having gained their object has made them less amenable to discipline?—They are decent men enough, but they will not work long hours now.

1759. Was the working the long hours the cause of the coal being better put on board ship?—No, the first part of the cargo was generally put in in daylight.

1760. You would not attribute the difference to the cessation from working long hours?—No.

1761. You attribute it to the negligence of the men, and their indisposition to work?—They will not work the lowering apparatus.

1762. Because it gives them trouble?—Yes.

1763. Is not that a question of wages; would not you be able to induce them to work it if you gave them more wages?—They have told me sometimes they would not work it if I gave them the ship and the cargo too.

1764. There is a general indisposition to put themselves to any trouble?—That is it.

1765. That has arisen since the strike, two years ago?—They always objected to work this thing; they used to call it the devil; they consider it is a nasty thing to work.

1766. They used to work it?—Yes.

1767. Since the strike, when they got the better of their masters, they have declined to work that machine?—Yes.

1768. What was the objection they had to this machine?—There is a good deal of care required in handling it; one or two men got their fingers snapped off in working it; they have a good deal of trouble in working it.

1769. Are any more wages paid for working it?—The men were paid 10*s.* a ship-load more, but that was stopped.

1770. Do you think if the men had been properly paid for working it they would have declined to work it?—Yes. I do not think any money would have induced them to work it.

1771. It is dangerous to work it?—There is little danger about it.

1772. Could not that danger be obviated by better mechanical arrangements?—Yes; the thing was never properly fitted.

1773. The fault seems to lie really with the shippers?—The colliery people ought to fit proper things, but they will not take any trouble about it.

1774. You would rather blame the colliery people than the men?—They are both to blame, I think; the men do not like anything that gives them more trouble, and the colliery people do not seem to take much trouble about it.

1775. It is to the advantage of the colliery owner to ship coals in as good a condition as possible, is it not?—They do not seem to think so; they charge the same price for coal whether badly shipped or well shipped.

1776. You give a preference to coal which is well shipped over coal which is badly shipped, do you not?—We want the coal as good as we can get it. I am supposed to look after it, and I do the best I can.

1777. Supposing one colliery shipped coal well, and another colliery shipped coal badly, you would report that to your principals, would you not?—Yes.

1778. Have you done so?—Frequently.

1779. And still it has produced no effect?—Yes, it has produced some effect; we cannot always get the coal we want; we must take the coals we can get.

1780. At present there being no such great demand, you can pick and choose?—What I mean is this, supposing we have got a ship to load with coal, and we have only got so many days to load her in, and suppose we cannot get the coal we want, we have to get some other coal.

1781. The fault seems to be a very divided one?—Yes, it is.

1782. (*Chairman.*) I suppose when the coal fever has thoroughly subsided, a little more attention is likely to be paid to improvements in modes of loading?—I do not know about that. The fever has subsided in the north now I should think.

1783. Coal owners are still hoping for a rise in price?—The next thing should be a fall I think.

1784. Do not you think if it is found that this method of shipment is really advantageous, and that the men are unwilling to adopt it from the danger arising from it, that some exertion will be made to improve it?—For my own part, I prefer this new plan to which I have referred, when I can get plenty of planks with which to make a platform.

1785. Is that plan adopted by anyone but yourself?—No, I do not think any one takes any trouble about the matter except myself.

1786. The Peninsular and Oriental Company stand alone in trying to ensure as far as possible the proper shipment of coal?—There is no one else that I see taking any trouble about it.

1787. Do your instructions from your principles at all point to the desirability of so shipping coals as to avoid the danger of spontaneous combustion?—There is nothing about spontaneous combustion in my instructions.

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1788. It is not a danger to which your attention has been called ?—No, I have had very little instructions about it.

1789. Your instructions are to see that good coal is shipped rather than to consider what may happen during its transportation ?—I never heard anything about spontaneous combustion till lately when these ships took fire, and then they wrote to me to give them my opinion upon the matter, and I wrote to them upon the subject.

1790. They being now in possession of your opinion upon the subject, they have not issued any particular instructions ?—No.

1791. You were commander of a ship yourself, I think ?—Yes.

1792. Did you ever command a ship laden with coals ?—When I first joined the company, I took a ship up the Red Sea, and I happened to fall in with the log the other day in which I kept a record of the temperature as shown by the thermometer in the cabin, in the fore-hold, and in the aft-hold.

1793. When you commanded a coal-laden ship, you were very careful to observe the temperature every day ?—Yes, every day.

1794. Is it the custom now in coal-laden ships to observe the temperature from day to day ?—No, I spoke to some captains of ships about carrying a thermometer with them, and they said it would be a very expensive thing, but a thermometer only costs a couple of shillings, and they also said it would be an expensive thing to put down a tube, but that would not cost much.

1795. Did you record the temperature in the log ?—Yes, I find in this log which I kept I have got the latitude and longitude, the course of the wind, the state of the weather, and the state of the temperature according to the thermometer in the cabin, in the fore-hold, and in the after-hold.

1796. Would you think it desirable in coal-laden ships that a note should be made in the log daily of the temperature of the coal ?—Yes, I think so.

1797. And that that should be required by law ?—I think so. When I wrote to the company, and said I had induced the owners instead of having ventilators to put down a thermometer, they said, in reply, that they quite approved of it, and they would like if I could induce the owners of all the ships to do the same thing.

1798. (*Mr. Abel.*) You carefully noted the temperature at different times ?—I marked it every day at noon.

1799. What would you have considered an indication of danger in connexion with those observations of the temperature ?—If the temperature was gradually rising, I should think the coal was heating.

1800. What steps were you prepared to take in the event of there being indications of danger ?—I should let in water, or do something of that sort.

1801. You had no definite measures laid down beforehand that you would have taken ?—A sailor is always prepared for difficulties; if he is not, he is not fit to be a sailor.

1802. You could not tell the precise nature of the means you would have adopted ?—I was never beaten yet.

1803. (*Mr. Fenwick.*) It is chiefly or entirely Hartley coals that you have inspected ?—Yes; I have shipped other coal for the dockyards, that is only for short distances.

1804. Speaking of Hartley coals, they are all from the same seam, are they not ?—No; there is the Low Main coal and the Yard seam.

1805. Is there any marked difference between those two seams ?—A considerable difference. Before they come to work the Yard seam the Low Main is generally pretty well worked out; the coal is crushed out, you can tell the difference because there is a great deal of pillar coal in it.

1806. Is one seam more liable than another to contain pyrites ?—I could not say that.

The witness withdrew.

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MR. JOHN GLOVER examined.

1807. (*Chairman.*) You are a chemical manufacturer at Newcastle ?—Yes.

1808. And a member of the Chamber of Commerce of Newcastle ?—Yes.

1809. You have given your attention to the subject which this Commission was appointed to inquire into ?—I have.

1810. Will you tell us what led you to turn your attention to the subject, and what opportunities you have had for forming an opinion upon it ?—My attention was more particularly directed to it from reading the lamentable accounts that appeared in the newspapers from time to time of the serious loss both of life and property from such occurrences, but my attention had been previously directed to the question as a chemical manufacturer storing large quantities of coal. I conversed with several of my friends upon the matter from time to time and from the chemical education I had received I formed certain opinions. Those were deemed to be sufficiently cogent to lead the Northern Maritime Insurance Association of Newcastle to write to me asking my opinion as to the causes of fire in coal ships, and they forwarded to me from time to time certain protests which they received from captains giving their accounts of fires, and I wrote certain letters on the subject. Our local authorities asked me if I had any objection to my letters being sent either to the Shipping Gazette or to the Liverpool Insurance offices. I said, on the contrary, I was anxious that they should be made use of in any way that was desired. The result was my being requested to attend this Commission.

1811. Will you tell the Commission what the nature of that correspondence was ?—The Secretary of the Maritime Insurance Association asked my opinion of the effect of ventilation on coal-laden ships, and, after due consideration, I came to the conclusion that ventilation ought rather to be avoided than sought for. I then suggested to them that if they could cover the surface of the coal with small coal and hammer it down as close as possible so as to keep the air from the cargo, it would be an advantage instead of putting in trunks to carry air into the centre of the cargo. I may mention that ventilation would, in my opinion, be advantageous if it could be applied to the whole bulk of the cargo, that is to say, if it could stream up through the coal as water would stream through a filter; but that desirable state of things cannot be practically arrived at. Therefore, instead of taking the air into the coal I would endeavour to keep it out, because, granted that spontaneous combustion was imminent in the cargo, an abundant supply of air would tend to fan the flame.

1812. You are speaking of north country coal ?—I am speaking of north country coal. That is the only coal I have had experience with.

1813. You have not studied the question in connexion with Welsh steam coal ?—Not at all.

1814. It is only with reference to north country coal that you are giving your evidence ?—Yes.

1815. What do you consider to be the cause of spontaneous combustion in north country coal ?—In my opinion the principal cause of combustion is the presence of bisulphide of iron in the form of coal brasses with a little moisture in the coal. Dr. Percy in one of the tables in his recent work on fuel, draws attention to the fact of a coal containing a much larger amount of sulphur than another not being so spontaneously ignitable as that other coal. If I had had time I would have produced specimens to the Commission showing that brasses (or pyrites) exists in coal in two or three distinct forms. It exists in the massive

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form, and it exists also in the form of very fine filaments, almost microscopically fine, those filaments being combined with filaments of equally finely-divided coaly matter. We have a condition in that latter case which we have not in the former, and though there might be more sulphur in the former case than in the latter the coal in the latter case would be more liable to ignite spontaneously on account of the finely-divided structure of the coal and the brasses. Dr. Percy also mentions in his valuable work on fuel, that coal very finely divided will spontaneously ignite. There is little doubt that it will, if you have the necessary condition of quantity of that finely-divided coal. I can scarcely imagine that there could be that condition of quantity of finely-divided coal in a coal cargo. Those are two conditions; and then there is another condition: all our coal contains a quantity of shale, and that shale contains a quantity of a very volatile and highly bituminous substance; it may be in the form of paraffin or some of the compounds that yield paraffin. I have obtained from shale from 12 to 15 parts of volatile matter of a highly inflammable character. By the table in Dr. Percy's book it appears that the most highly inflammable coal is schistose coal, that is, coal containing schist, and I would throw out a suggestion to the Commission that probably the quantity of schist in coal may have a great deal to do do with spontaneous combustion. Certainly the bisulphide of iron has to do with it, of that there can be no question, and as regards the second condition, when a gentleman like Dr. Percy is satisfied with the result of the experiments of continental chemists which went to show that finely divided coal would also spontaneously ignite I think we may accept it as a fact.

1816. You divide the conditions of spontaneous ignition of coal into three classes?—Yes.

1817. What remedy would you propose to apply?—Speaking of spontaneous combustion, and not of explosion, the remedy I would propose would be, in the first place, that there should be inserted into the coal, through the deck, if possible, a number of $\frac{3}{4}$-inch iron gas pipes placed 8 or 9 feet apart, down which gas pipes should be lowered at fixed intervals a self-registering thermometer. Every day the captain should enter in his log the temperature of each of those pipes, and if he found the temperature rising in any of them, he should be prepared with an apparatus of which I submit a drawing to the Commission (*handing it in*): an apparatus consisting of a wooden cistern with a false bottom, into which you would put chalk, and upon that you would pour hydrochloric acid. I have made some calculations with the detail of which I need not trouble you, but I find that taking, what is called in our district round coal, that is to say, the best coal, a ton of coal when packed in a ship leaves about 12½ cubic feet of interstitial space; that interstitial space is all that we require to be filled with carbonic acid gas, carbonic acid gas being antagonistic to combustion. Given, 1,000 tons of coal in a ship, we would then have 12,500 cubic feet of interstitial space to fill with gas, and to fill that interstitial space with carbonic acid gas would require 36 cwt. of chalk and 80 cwt. of hydrochloric acid, therefore, for 1,000 tons you may say the expense would be about 80 cwt. of hydrochloric acid, which costs, exclusive of carboys, about 2*l.* per ton, the chalk being merely nominal. The size of the cistern which would be required would be about 12 feet long, 6 feet wide, and 6 feet deep. Probably an objection might be raised to having to provide a cistern of that size, but it might be used in the passage out as a store for fresh water. If even the chalk was placed in it, the chalk would do the water no harm, but the chalk might be kept outside. The expense of the apparatus would be merely nominal. The temperature at which coal might ignite is an important question. I see Professor Graham in his report on the firing of the "Amazon," goes into that question, and he shows that coal may ignite at very low temperatures; but the conditions that Professor Graham refers to would not be present, such as laying the coal upon hot spaces. We don't expect that the coal would be laid upon hot spaces, except it might be in a steam ship.

1818. You have explained to us the precautions you would take in the case of actual combustion. Have you any recommendations to make to the Commission with regard to what should be done to prevent the spontaneous combustion of coal either in respect to the loading of the coal, or in any other respect?—I know of no means that would prevent the firing of the coal taking coal as it is ordinarily shipped on board ships. I think the firing of the coal is only a question of time.

1819. You mean there is no one method of loading better than another?—I know of no one mode of loading better than another. The coal is usually lowered down, and the interstitial space is a matter of accident. When I gave you 12½ cubic feet in a ton, I took that from an average of 27 different kinds of coal.

1820. You do not consider that the coal falling any distance, and breaking and becoming smaller adds to the danger of spontaneous combustion?—If we take the observations of Dr. Percy as regards small and finely-divided coal, of course if you increase the quantity of that finely-divided coal by a greater fall, you increase the danger from that source.

1821. Do I gather from what you have said, that you think the less amount of air which has access to the interior of the coal the better?—That is my meaning, taking other things into account.

1822. So that in the ordinary sense of the word the ventilation of north country coal is prejudicial?—Decidedly. I have come to that conclusion.

1823. Your recommendation, therefore, would be to do absolutely without internal ventilation, and to take that precaution which you have described to us in the event of heating?—Yes, that the captain should always have the means of knowing whether his coal was heating. Given the application of carbonic acid, if it was not applied till the coal was in active combustion it might be prejudicial rather than beneficial. If there was any mass of coal in active combustion it would decompose the carbonic acid and become carbonic oxide, and so tend to feed the fire.

1824. In all cases you say the spontaneous ignition of the coal is only a question of time. Are not some denominations of coal much more liable to combustion than others?—I cannot speak from absolute observation. I should say there would be. I know when I want coal brasses I go to different collieries to get them. I go to a particular colliery if I want a hard and massive coal brass, if I wish to burn it to get the sulphur out; and I know that in other collieries the bisulpide is so finely divided, and the coal is so finely divided, that it is of very little use for my purpose, and the coal of those collieries in which it is in that finely-divided state would be more likely to take fire.

1825. You consider that some coal is much more likely to take fire on board ships than others?—As a matter of opinion, I do not speak from experience. I have told you on what I base my opinion.

1826. You are not prepared to mention any facts tending to show the unfitness of certain classes of coal to be shipped long voyages?—No. I may mention that I have had put into my hand a great number of returns from the Board of Trade, on looking through which it appeared to me that it would be very useful if, on the captain applying to the consul for the usual protest in any case of loss or damage to the ship or cargo by the spontaneous combustion of coal, certain questions were put to him. The questions which I would suggest should be put to him are as follows: 1. Name of ship? 2. When, where, and of what build; also class, sailing, or steam? 3. Was vessel tight when loaded? 4. Was vessel tight when in a gale or straining? 5. How often did you pump bilge, and what quantity each time 6. Was bilge water warm, hot, or cold? Colliery coals came from; and was it round, unscreened, or small? 8. Was cargo put on board dry, damp, or wet? 9. Was cargo ever wetted from the deck or bilge? 10. Date when loaded and where? 11. Date when first satisfied cargo was on

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fire, and what latitude and longitude? 12. In what part of ship did fire commence? 13. Was cargo ventilated, and if so, state method employed? 14. Weight of cargo?

1827. (*Mr. Abel.*) In referring to the influence of pyrites in bringing about spontaneous heating of coal, you spoke mainly with reference to the state of division of the pyrites; do you consider that the state of division is the only reason why pyrites in some instances favours spontaneous heating of coal more than in others?—I do come to that conclusion most decidedly. I find that a mass of pyrites, free from admixture with coaly matter, is not liable to ignition; for instance, we bring large quantities of pure pyrites to this country without the admixture of coaly matter, but we never have such a thing as spontaneous combustion in them. I keep perhaps many hundreds of tons in our works for 12 months, and find no appearance of combustion, that is in pure bisulphide of iron, but I cannot keep coals for half or perhaps a fourth of that time without their igniting spontaneously; they may ignite spontaneously without any appearance of heating on the outside. I have found when I have got to the bottom of the gears, (which are the woodwork supporting the platform on which the waggons run bringing the coal to our works,) that the gears are burnt through.

1828. Are there some kinds of pyrites that are more liable to change than others, not speaking exclusively of pyrites which exists in coal?—I think some change more than others, but not in the direction of spontaneous combustion.

1829. In what way do you consider the hydrocarbons which you spoke of as having found in schistose coal contribute to the spontaneous heating of coal?—Granted a slight heating in the coal and an evolution of that highly volatile gas, I think it quite possible that that gas might ignite; some of these gases ignite at very low temperatures, much below that necessary to ignite coal. It is just possible that that gas might be given out by the gentle heat that is evolved from coal.

1830. You think it would ignite at a lower temperature than sulphur would ignite at?—Considerably. The flashing point as defined by the Act of Parliament of many of the hydro-carbon oils is from 120° to 150°.

1831. That is the temperature at which the vapour is given off but not the temperature at which the vapour ignites?—No.

1832. Whatever the temperature at which the vapour is given off all those hydro-carbons ignite at very high temperatures, do they not?—I am not aware of that.

1833. And sulphur at a comparatively low temperature?—About 230°. Its melting point is about 230, and it spontaneously ignites at 300 or thereabouts.

1834. You do not consider that the hydro-carbons in any way directly contribute to spontaneous heating, do you?—It is a theory I throw out mainly based on what I observe in Dr. Percy's table with regard to the non-inflammability of some coal bearing a relation to the schistose quality of the coal. And from having experimented on the schist of the coal measures and finding a quantity of light hydro-carbon vapour, given off, it struck me that there might be some connection between the two which this Commission might work out.

1835. Is not the schistose coal a more porous coal than the compact coal?—The schist is a very compact substance, like slate; it is called slate in our neighbourhood.

1836. You do not consider that it is more porous than compact coal, that is to say, than ordinary pit coal?—Not more porous, I should say, it was less porous. You can split it up into thin laminæ.

1837. Would not that indicate that it would be more permeable by gas or air?—The laminæ are perfectly invisible; with a sharp instrument you can strike the schist in such a manner that you can separate it into aminæ.

1838. As it separates into laminæ or very thin layers the structure must be comparatively speaking open?—Not so loose as coal; you can break coal into thin layers.

1839. You do not consider that this schist would be more liable to absorb oxygen or other gases than hard coal?—I could not answer the question without experiment.

1840. You consider that the state of division of the coal is of considerable importance as affecting the question of spontaneous combustion?—I do consider so.

1841. Have you any personal experience which you can give as a reason for considering that the state of division of the coal must exercise a considerable prejudicial influence?—I know that all carbonaceous matter when in a state of fine division is subject to spontaneous ignition, and coal would be no exception, and especially gas coal, but it must be in a large mass; you must have a considerable mass before you can have either from brasses or from finely-divided carbon such an evolution of heat as would lead to combustion.

1842. You have no special views or facts to give us with regard to the influence of a fine state of division of the coal?—No, my views are somewhat vague upon the question of the effect of that state of fine division of the coal.

1843. In speaking of applying carbonic acid for the purpose of extinguishing fire in a cargo of coal, you spoke of the inadvisability of applying it when the fire had reached some considerable dimensions or any dimensions at all?—Yes.

1844. Why would it be inadvisable to apply it under those circumstances?—When you have coal in active combustion, that is when the heat is high, that is a condition in which the carbonic acid may be decomposed.

1845. The fire must have reached to a considerable degree of intensity to produce that result?—Yes, but it might be localised, and then it would reach a state of considerable intensity. It would scarcely have reached that intensity without having manifested its presence even if there were none of those tubes which I recommend, but with the tubes I think it could not possibly have reached that state of intensity, therefore I would have the tubes as an essential part of the apparatus.

1846. You consider your method of applying carbonic acid would be applicable provided proper temperature observations were made?—Quite so.

1847. (*Dr. Percy.*) Have you any personal experience of the spontaneous ignition of coal in a mass?—As a manufacturer I have. I have had many such cases.

1848. Did you observe the conditions under which that ignition took place?—The condition that would be necessary to satisfy you I think was not observed; the only fact that I did observe was that when the coal was worked out and the men had taken it away in their barrows, I saw that the gears were burnt through. What was the condition of the coal in contact with the gears at the moment they were bared, I do not know.

1849. You had not an opportunity of inspecting the phenomena that occurred in those cases?—No.

1850. Your conclusions are based upon chemical knowledge, and not on individual personal experience?—Based on my chemical knowledge, but in our chemical works we do sometimes use large quantities of brasses; we have them laid down in a mass, and we know from experience that we cannot have them lying there a month without great risk of the whole mass firing. We have to use them up as fast as we get them.

1851. You are speaking of pyrites from coal measures?—Yes.

1852. If I understand you correctly there are no precautions requisite to be taken with regard to the other kind of pyrites?—Not the slightest. After 30 years' experience with pure pyrites from all parts of the world I may say that I never knew a case of spontaneous ignition in any of the pyrites properly so called.

1853. You have not known any case of spontaneous

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ignition of pyrites unless intermingled with carbonaceous matter ?—Not unless intermingled with carbonaceous matter.

1854. In your opinion that is the condition of this ignition ?—That is the condition of spontaneous ignition. With bi-sulphide of iron alone, so far as my experience goes, there is no danger, but where you have bi-sulphide in combination with carbonaceous matter, then there is danger.

1855. With regard to this schist, the schist weathers pretty rapidly when exposed, does it not ?—Some of the schists do.

1856. Do not most of them, those, for instance, that are raised out of the collieries, and which you see in mounds at the top of the pit's mouth, do not those weather and split up rapidly when exposed ?—I have often gone round a colliery mound to examine the heaps, and I have not observed that they are weathered in all cases.

1857. Have not you seen some rapidly fall to pieces ?—Yes. I have seen some rapidly fall to pieces.

1858. Have you made any experiments with regard to the quantity of pyrites in those schists ?—No, I have not.

1859. You do not know whether it would be present in any sensible amount ?—No.

1860. The schist to which you refer contains coaly matter ?—Yes.

1861. Have you seen heaps of this schist on fire ?—Yes, I have frequently seen them on fire; it burns from quite black to white, which is a proof that it is coaly matter, I think, and not bisulphate of iron.

1862. As to gas coal, in your term "gas coal" would you include good cannel coal ?—Yes.

1863. Do you think that that is likely to ignite spontaneously ?—I think finely-divided portions of cannel coal would be very likely to ignite spontaneously.

1864. Is that simply a chemical opinion you have formed, or is it the result of personal experience ?—Entirely a chemical opinion. We do not use cannel coal in the north to any extent.

1865. With regard to your proposal to use hydrochloric acid to generate carbonic acid, how would you recommend that acid to be kept on board ship safely ?—I know of no other method but the ordinary method of keeping it in carboys.

1866. If they were to break it would be rather awkward, would it not ?—Yes, but it is not a very corrosive acid.

1867. Still it would be unpleasant ?—Yes.

1868. With regard to spontaneous ignition of finely-divided coal have you any personal experience upon that subject ?—No, I have not. How far the tendency to spontaneous ignition is due to the finely-divided state of the coal, or how far it is due to the presence of brasses, I have no means of knowing.

1869. (*Mr. Ferguson.*) Do you consider that coals shipped wet are more liable to spontaneous ignition than coals shipped dry ?—Decidedly; moisture is one of the main conditions tending to the firing of coal.

1870. If coal were shipped dry there would be less danger of ignition ?—If coal were shipped dry there would be less danger of ignition. That reminds me of a suggestion I was going to make, that perhaps it might be possible in case of coal being shipped wet to inject through the coal a current of hot air by the tubes I have mentioned. It would not cost much to do that.

1871. If coal were shipped dry there would be less danger, you think ?—If shipped dry and kept dry there would be very little danger, and therefore I would suggest that it is very important that the surface of the coal be ventilated, and the shafts for ventilation should be carried well up he mast, so that in shipping a sea it should not go into the coal.

1872. There is a danger in ordinary ventilators of water getting down the ventilators ?—Yes.

1873. You do not approve of the ventilators being carried through the coals ?—Not through the coals, but it is imperative that the surface should be ventilated, there should be a wooden trunk carried up the mast, so high that in shipping a sea there would be no danger of its going into the cargo.

1874. (*Mr. Duncan.*) You have stated that you think the coals being shipped wet is a great predisposing cause of their taking fire spontaneously, what inference would you draw from this fact, that it appears from the ascertained statistics that the cargoes of the greater part of the large number of ships that sailed in 1873 and 1874 were shipped in the driest months of the year ?—I am not prepared to draw any inference at all from that fact. I would first require to know all the particulars in such cases; though the cargoes were shipped in the driest months of the year, it does not follow that they were shipped dry. I would require to know whether there were thunderstorms at the time, and whether the bilge water was allowed to come in contact with the lower part of the coal, and whether the ship in straining made water. All those facts would have to be ascertained before I could give an answer.

1875. Are you so confirmed in your opinion that water is the cause of spontaneous combustion that you would still hold to that opinion, notwithstanding such cases were brought to your notice as we have had brought before us in evidence to-day, by Mr. Steinson, who told us that three cargoes were shipped in perfectly dry weather, and that the ships were well ventilated, notwithstanding which the ships were burnt, whereas in a fourth case, where the cargo was shipped very wet, and where the ship was not ventilated, it went perfectly safe ?—As I said before, if any inference is to be drawn from the dryness of the coal, or as to the effect of moisture in producing spontaneous combustion, you require to know all the facts; for instance, you would require to know not only whether the three cargoes were shipped dry, but whether they were kept dry.

1876. If you had a pile of coals on your own premises you would roof them over, I presume, in order to keep them dry ?—Yes; the drier coals are kept the less they will deteriorate, and they will not be so liable to take fire, whereas if coals were not covered, they would take fire.

1877. (*Mr. Vivian.*) Do you think that the coal that is wrought and shipped now in the north of England contains more shale than it used to do ?—I cannot say whether it does or not.

1878. Not being interested in shipping coal, you have had no means of observing ?—No.

1879. Do you find that it is more dirty than it used to be ?—I find the small coal decidedly dirtier than it used to be.

1880. What per-centage of ash does your small coal contain ?—The per-centage of ash varies; from the best quality of coal we got 8 to 10 per cent. and I have found in some descriptions as much as 22 and 23 per cent. of ash.

1881. Are you able to state what it used to be in former days ?—In former days, (unless we went to a particular coal, which I need not specify,) the average amount of ash, so far as my recollection goes, would be about 10 or 12 per cent.

1882. 10 or 12 per cent. against what ?—At present from the best that we get it is about 10 per cent. In a great deal that we get from the steam coal field it is about from 18 to 22, and I have known it 23, but that is only in some steam coals. I observed that large per-centage in a description of coal from some collieries which I had not previously gone to, so that it does not follow that the per-centage would be larger in that kind of coal, because I had not an opportunity previously of trying that kind of coal. These per-centages are the result of experiment; we try every cargo of coal that comes into the works for ash.

1883. So that the small of the steam coal in your district contains a large amount of ash as now delivered ?—It contains a large amount of ash as now delivered; what might have been the amount previously I have no means of stating authoritatively.

1884. If as we have heard, shale is spontaneously ignitable, that may to some extent account for the

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increase in the number of cases of spontaneous ignition which have occurred during the past few years?—If a certain amount of shale is present in the coal, and if it is spontaneously ignitable, as I believe it is, the amount of shale would render the cargo more liable to spontaneous ignition, but I may mention that most of the shale comes into the small coal, in fact I should think nearly all the shale comes into the small coal.

1885. In your judgment would coal which contains no sulphur ignite?—It would not ignite from the cause that leads to the fixation of oxygen by sulphur and iron combined with sulphur; but taking Dr. Percy's view of the matter, if it contained very finely-divided smal coal, it would be as liable to ignition if it contained no sulphide, owing to the finely-divided state of the small coal

1886. You said in the early part of your evidence that in order to ignite under those conditions a certain quantity of coal must be heaped together?—Yes.

1887. Can you state what quantity that should be?—I would be a rather bold man to state the quantity, but certainly it would be a very large heap, not less I should say than from 200 to 500 tons.

1888. The depth would have to do with the spontaneous ignitability of such coal?—Yes, certainly.

1889. What would you put as the depth of a heap in which you would say spontaneous combustion would be likely to take place?—If you had a depth of 8 feet to 12 feet, you would have a condition in which the coal would spontaneously ignite. I have certainly seen ordinary small spontaneously ignite under a depth of 15 feet repeatedly.

1890. That was coal that contained pyrites?—Yes, and shales.

1891. I am speaking of coal which contains no pyrites?—I have no experimental proof in that direction; it is all matter of opinion.

1892. You said that coal brasses would ignite, what per-centage of sulphur would those coal brasses contain?—They vary a great deal, they contain from 22 up to 30 per cent. of sulphur.

1893. And the ordinary Spanish pyrites contains what?—From 40 to 50 per cent.

1894. So that the difference between the two is probably made up of carbonaceous matter?—It is made of chiefly carbonaceous matter; almost entirely; you may say entirely carbonaceous matter.

1895. Have you ever received cargoes of small pyrites?—Frequently.

1896. Have you ever known them to heat?—Never we have kept small pyrites in the works for 11 years.

1897. Do you know of any sulphur existing in coal which is not in combination with iron?—I do not know of any, certainly.

1898. If sulphur were found by analysis in coal, you would naturally conclude that it existed in a state of pyrites?—Yes.

1899. Can you imagine any other combination in coal in which sulphur might exist?—I do not know of any, unless it is sulphate of lime.

1900. Sulphate of lime would be wholly undecomposable?—It is quite inert.

1901. What would be the cost of the apparatus you have suggested to the Commission?—I have not gone into the question of the cost, but the whole apparatus would not cost 20*l.*, perhaps 25*l.* would fit up the whole apparatus on board a large ship.

1902. Where would you place the apparatus?—In the most convenient part of the ship; of course in a coal-laden ship the apparatus should be placed on deck.

1903. How would you apply it?—The *modus operandi* would be simply when the thermometeric indication showed an increasing temperature, increasing up to a point which ought to be specified to the captain in charge of the ship.

1904. Interrupting you for a moment, what would that point be?—If I had charge of a ship, and observed the heating going on from the ordinary temperature till it had risen to about 200°, I should then give a dose of carbonic acid, observing still the thermometeric conditions, that dose would be given by pouring in the hydrochloric acid gently down the orifice, so that there should not be too much effervescence; the carbonic acid would find its way to the bottom of the ship, and you could fill the ship full with it, in the same way as you could with water.

1905. Would you connect the pipe leading from the apparatus with the hose?—I would have iron gas pipes laid down into as many compartments as the ship may be divided into, and connect them with a pipe issuing from this apparatus, probably by a coupling or connection of indian rubber pipe; indian rubber pipe would probably be the best, because it would give with the motion of the ship. On pouring in the hydrochloric acid, the gas would be evolved, and it being a very dense gas, it would find its way to the bottom of the ship, filling all the interstices; of course, no human being must be below at the time.

1906. You would pass it down the pipe which showed heat?—No, those pipes would simply act as indicators; but, certainly, if those pipes were perforated at the bottom, you might put it down the pipes themselves.

1907. You would continue to pour in the carbonic acid till the whole of the interstices of the coal were filled up with carbonic acid?—Till the whole of the interstices were filled. As I mentioned, a thousand ton ship would require about 36 cwt. of carbonate of lime or ordinary chalk, and about 80 cwt. of acid of the common strength of the chemical works. You would get it at 2*l.* a ton, then there would be the cost of the carboys, which would be more than the acid, where the contents of the carboy might cost 2*s.*, the carboy itself would cost 3*s.* or 4*s.*

1908. You believe that carbonic acid existing in the condition of dry gas, would be sufficient to arrest the decomposition of the pyrites?—I think it would entirely. I think it would prevent the decomposition of the pyrites, and therefore the evolution of heat. I do not think pyrites would decompose even with moisture in the presence of carbonic acid.

1909. You consider the presence of carbonic acid would arrest the decomposition of the pyrites?—Both arrest decomposition and put out any fire present, provided the fire had not got into a state of violent incandescence.

1910. Fire would hardly exist at the temperature you have given, 200°?—The heating might inadvertently rise to 400° or 500°.

1911. If you poured it on when there was a temperature of 200°, it would be when the decomposition of the pyrites was going on?—Yes.

1912. Would the presence of carbonic acid arrest that decomposition?—I think it would. Experiments would have to be made; it is a thing to ascertain by experiment with a large quantity of coal, whether the decomposition might not go on further, because many a ship has got to its journey's end safely, which if the voyage had been continued a little longer, so that the process of decomposition had gone on a little further, might have been burnt.

1913. Carbonic acid being a very heavy gas, would, I presume, expel the air from the interstices and take its place and remain there permanently?—Yes, unless it was highly heated.

1914. Would there be any danger to the men in discharging that ship?—The ship would have to be carefully attended to if the dosing with carbonic acid had been just before the ship making its port. The presence of carbonic acid could be easily determined by lowering down a light which would be put out at once.

1915. It is conceivable that the carbonic acid might remain occupying the whole of the interstices of the ship till she reached the end of her voyage?—If the ship was heated up to 250°, so that when the carbonic acid got heated up to that heat, it would be apt to be displaced by the surrounding air, it would simply arrest it for a time, but that time might save the ship. If a portion of the coal had really got into a state of slow combustion it would arrest that combustion; but

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as to saying that when once filled with gas it would remain there the whole voyage, I think it would not.

1916. You would not recommend giving the ship a dose of carbonic acid before she started as part of her cargo?—No, I would recommend, if possible, drying the cargo by giving it a dose of warm air. When you give a dose of carbonic acid, you must have no living being below deck.

1917. (*Mr. Duncan.*) That is a practical question that has been occurring to me, because in many ships the crew live forward below the deck, they would have to live on deck during the administration of this carbonic acid?—They would have to choose the least of the two evils and live on deck.

1918. (*Mr. Vivian.*) Or you might have tight bulk heads to keep the gas away from that portion of the ship?—Yes.

1919. (*Mr. Abel.*) With regard to your proposed application of carbonic acid, I believe, however dense a gas is, it has a tendency to diffuse itself into the air, has it not?—Yes.

1920. So, that unless your hold were perfectly gas-tight, the carbonic acid would gradually diffuse itself into the air in other portions of the ship?—Into all the other portions of the ship; but supposing your bulkheads were perfectly tight it could not diffuse itself through an iron bulkhead.

1921. It would be necessary to have gas-tight bulkheads in order to imprison the carbonic acid?—In one particular place.

1922. In order to prevent its finding its way to other portions of the ship?—Suppose you had a ship divided into three bulkheads, you would have three separate pipes proceeding to your carbonic acid apparatus. Suppose the middle compartment was heating, you would connect the middle compartment with the carbonic acid apparatus.

1923. With reference to the sulphur existing in the coal, if you found the proportion of sulphur in a coal to be considerably in excess of that required to combine with the iron which you found, you would come to the conclusion that there must be some sulphur not in combination with iron?—Yes.

1924. Have you not known that to occur in many instances?—No.

1925. Have you not seen some analyses published by Dr. Percy of coals, in which the amount of the sulphur is very much in excess of that required to combine with the iron in order to form pyrites?—I have not read that in Dr. Percy's work.

1926. Have you ever heard of a schistose mineral containing no iron at all, but containing a considerable quantity of sulphur in its composition?—No.

1927. (*Chairman.*) You said you would require as much as 80 cwt. of muriatic acid to produce enough carbonic acid to permeate the whole mass of a thousand tons, how much does a carboy hold?—A carboy holds about 1 cwt.

1928. So that you would require to carry 80 carboys on deck. You could not put the carboys below?—The carboys could be carried easily on the surface of the coal.

1929. If I am not mistaken acid is obliged to be carried on deck in an ordinary merchant ship?—Yes.

1930. You would make a special exemption in a coal carrying ship, and allow a certain quantity of acid to be stowed below?—Yes; there is no other apparatus that would carry it, unless you fitted up a wooden cistern to hold the muriatic acid, and then it would be difficult to keep it tight, perhaps you could make it perfectly tight by gutta percha.

1931. Otherwise 80 cwt. of muriatic acid on deck would be rather an awkward load, would not it?—Yes.

1932. (*Dr. Percy.*) You would have a wooden case tarred?—Yes, it would last the voyage, probably. My experience of gutta percha is so adverse that I do not recommend it; a stone cistern would hold it; a wooden case tarred would hold it a long time, but the slightest permeation of the acid through the pores of the wood coming in contact with the bolts that bound the cistern together, would eat them away in time. I know that in storing muriatic acid we never use wooden cisterns, we always use stone cisterns.

1933. Have you ever made any experiment with regard to the effect of steam in extinguishing fire?—Never, but I should think it would not put out a fire.

1934. Supposing we had steam at our command, and we blew that steam down on a mass of coal in ignition, what do you think would be the effect?—Whether it would put the fire out or not I would be inclined to doubt along with Professor Graham in his report. Professor Graham seems to think that it would be of little use; the objection I would have to the use of steam would be the quantity you would have to blow in, and the consequent condensation of that quantity, wetting the coal again.

1935. You say you have made a determination of the per-centage of ash in the coal, did you ever make a determination of the quantity of water contained in the coal supplied to you?—Yes; we find taking 19 cargoes out of 20 we got about three per cent. of moisture in the coal.

1936. Quite sufficient moisture, supposing the air gets access to the coal to ignite the pyrites?—I think so; if that three per cent. remained in the coal as water, it would have that effect.

The witness withdrew.

Mr. EDMUND SQUIRE ROBERTS examined.

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1937. (*Chairman.*) You are inspector for the Peninsular and Oriental Company in respect of their coal shipments at Cardiff?—Yes.

1938. How many years have you been in their employ?—Seven years.

1939. Can you give the Commission any idea of the number of the ships, and the tonnage of the cargoes that you have inspected?—We ship about 150,000 tons during the 12 months, and I should think nearly 100 vessels are employed. I did not look at my books, but I think about 100 vessels load during the year.

1940. I may take it roughly that you have inspected 1,000,000 tons of coal and some 700 ships?—Yes.

1941. Have you had more or less during the last few years?—More. I ought to say perhaps in the first year we did not have so much as the quantity I have mentioned. It is lately that we have had more.

1942. Have you had any cases of spontaneous combustion or explosion of coal shipped from South Wales?—I have had only one case of explosion.

1943. And none of spontaneous combustion?—None of spontaneous combustion.

1944. What do you attribute that remarkable immunity to?—That we ship the best steam coal in the district.

1945. And that the best steam coal is not subject to spontaneous combustion?—I think that the best steam coal is not subject to spontaneous combustion.

1946. With regard to ventilation, does the Peninsular and Oriental Company lay down any rules as to ventilation?—No rules whatever.

1947. What do you find as a matter of practice is generally done as regards ventilation?—We find as a rule, they wish to construct large wooden ventilators throughout the ship.

1948. And they do so?—Yes.

1949. You do not charter the ships?—No.

1950. You only contract with the charterers?—Yes.

1951. Therefore you do not interfere?—Not in the least.

1952. What is your opinion with regard to the question of ventilation?—That it is better left alone altogether than done in the way it is generally done.

1953. That is that air should not be allowed to permeate through the coal?—Yes.

1954. All that you require in your judgment is to

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keep the gases from the surface?—I think you require ventilation on the top. I look out for something of that sort. I have no orders to do so; but I impress upon the captains to have the hatches a little free, and to have ventilators at either end of the vessel.

1955. That is in order to carry off the gas and keep a current of cool air on the surface?—Yes.

1956. Beyond that you would not go?—No, I think it prejudicial to the safety of the coal to do anything else.

1957. Why do you think it prejudicial?—I think by having those ventilators in the bottom of the ship and having them up and down the hatchways, you create a draught and you raise the enemy you are trying to keep out.

1958. If the coal gets heated and a draught passes through it, it is more likely to increase the combustion?—I think so.

1959. At any rate from what experience you have had, you think the risk is less in keeping air from the interior of the coal altogether?—Yes, I do; all the ships I have heard of, where they have had, what they call spontaneous combustion, which I do not believe in, have been ventilated.

1960. You say that you do not believe in spontaneous combustion. You have no doubt heard of ships with which you have had nothing to do, in which fires have taken place?—Yes, which have been said to have been caused from spontaneous combustion; but I think, it has been from carelessness. It has no doubt been from explosive gases.

1961. Are you speaking of South Wales coal?—Yes.

1962. You are aware that with regard to the north country coal, there is no question that the coal does take fire?—I understand so; I do not know much about north country coal.

1963. Speaking of South Wales coal, you know of no case well substantiated, of actual spontaneous combustion?—I do not.

1964. Though you do know of cases of explosion?—Yes.

1965. Where gas has collected which has been accidentally lighted?—Yes; by people taking a light, or by some means or other.

1966. Not gas which in itself would be liable to take fire by contact with oxygen?—No, not at all.

1967. Can you tell us anything about your method of shipping coal? does the Peninsular and Oriental Company insist upon shippers shipping in any particular way?—Yes; we are very particular, we make them lower the coal into each hatchway till there is a sufficient quantity raised in each hatchway, so that it should not break.

1968. So that there should never be a heavy fall?—No great fall.

1969. Will you explain the method?—We have a box which we put at the end of the shoot, which box, after it is filled, is lowered down to the bottom of the ship till we get a good pile of coal, then the remainder of the coal is allowed to run on to this pile of coal which has been formed by the box.

1970. You say the box is put at the end of the shoot?—Yes; the coal goes down the shoot; there is an iron tub or box which comes to the mouth of the shoot and is filled; when that box is filled they stop the run of coal in the shoot and then they lower the box down.

1971. And then they raise the same box up again?—They raise the box up; it is a system that requires a great deal of attention.

1972. It also takes a great deal of time?—Yes.

1973. How many tons can be put into the ship in an hour upon that plan?—I am not prepared to answer that.

1974. Can you tell how long it takes to fill up the hatchway?—That would be according to the depth of the hatchway. I should say 10 waggons could be carefully shipped in an hour; but it would be expensive, and would take a long time comparatively, because the same quantity of coal could be tipped in 15 minutes.

1975. Your preference for this method is on account of the avoidance of breakage?—Yes.

1976. You have not in view the consequent risk of combustion?—No; only I consider that it is the small coal that is likely to generate gas.

1977. All that you look to is to see that you get the coal you contract for?—To see that we get large lumps rather than small.

1978. Your instructions from the company do not point to the danger of combustion?—They do not allude to it in any way.

1979. The danger has not been thought of?—It has not been thought of in South Wales at all.

1980. Have you had any experience in respect of the storage of coal in large quantities?—I have.

1981. Where?—In Alexandria.

1982. Where you agent for the company there?—No; I was in charge of a coal depôt. I was their marine superintendent, and I looked after all the cargoes shipped, and I looked after the supplying of the ships with coal. The agent was a gentleman on the shore who looked after the freights.

1983. You transacted their coal business?—Yes, for 12 months.

1984. While you were acting in that capacity had you any large amount of coal stored at any time?—A great quantity.

1985. Did you have any experience of combustion in the case of any of that coal?—Not with our own coal; but during the time I was there I saw other coal ignite belonging to other people, notably the Russian Government. They had a store close to us which ignited.

1986. (*Dr. Percy.*) What coal was it?—I am unable to say.

1987. (*Chairman.*) What was your coal?—Both kinds, north country and South Wales.

1988. Did you mix them?—No; we keep them in separate heaps.

1989. Did you apply any system of ventilation?—Yes, in each stack of coal as we landed it we always ran a tunnel through the coal fore and aft and athwart, and we had a sort of chimney built of the coal in the middle of the stack; that would be in a heap of 1,000 tons, for instance.

1990. What was the object of that ventilation?—To prevent spontaneous combustion.

1991. Did you apply that to both kinds of coal, both bituminous and South Wales?—Yes. The Arabs always understood they had to build this tunnel; that was a part of their contract.

1992. Did you consider that that ventilation was any good?—Yes.

1993. Why should it be of use on shore and not on board ship?—It is perfect on shore. There is air all round, as it were; the mass of coal is not confined in any way. There is a tunnel running in the coal one way, and across it in the other, and you have the air all round it, whereas in a ship you have not got the same conditions; it is confined on all sides.

1994. You do not think ventilation in itself mischievous, but it is mischievous because you cannot make it perfect?—Exactly.

1995. Were your stores covered at any time?—Yes, they were all covered, so that the coal was not exposed to wet.

1996. You had very little rain at Alexandria?—Except at certain seasons in January and December we have a great deal of rain, but all our coal was under cover. I should not like to leave it exposed to the rain.

1997. Was the reason you kept it covered to preserve it from deterioration, or because you thought if you did not keep it covered it would be liable to spontaneous combustion?—To preserve it from deterioration.

1998. You did not think there was any risk of its taking fire from its being uncovered?—No; we considered that the coal being exposed to the atmosphere, and the rain and the sun would disintegrate it, and thereby deteriorate it.

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1999. You would not ventilate the interior of the coal on board ship; you would keep a current of air on the surface, and you would let the coal fall when it was being loaded as little as you could help?—Certainly.

2000. Have you any other suggestion to make to the Commission whose object is to see what should be done with reference to spontaneous combustion?—The only suggestion I could make is, that only the best coals should be sent on long voyages, and not those inferior coals.

2001. When you say "those inferior coals," the Peninsular and Oriental Company buy the best, I presume?—They try to do so.

2002. Have they occasionally found themselves buying inferior coal?—They find themselves with inferior coal, though not buying it.

2003. You mean the coal owner does not give them what they expect to get?—It does not always turn out so.

2004. Do they make their contracts at first hand with the coal owners?—Direct with the coal owners; but what I was more particularly referring to, was that there are certain coals from South Wales that it would be dangerous to carry on long voyages.

2005. Why are those coals dangerous?—Because they contain a great deal of iron pyrites.

2006. In your seven years' experience have you known of any casualties happening in ships carrying that description of coal?—I have heard of them, but I have not taken any particular notice of them.

2007. You know generally the kinds of coal that are to be avoided?—Yes.

2008. It is your duty to keep your principals well informed upon that point?—Yes, that is my principal duty down there to see that they do not get that sort of coal.

2009. The liability to spontaneous combustion is a matter you specially direct your attention to?—Yes, individually. I have no instructions to do so. I should warn my principals if I knew that coal was being shipped which I thought would be dangerous, but I have no instructions to do so.

2010. What classes of coal do you think ought not to be shipped for long voyages?—All those that have much iron pyrites in them, and also the coals in which there is a large intermixture of sulphur. No. 2 Rhondda from my district is a very dangerous coal.

2011. Is that an extensive seam?—Yes, it is very largely worked by nearly every colliery in the Rhondda Valley.

2012. Is there any other that you could put in the same category?—Some of the Monmouthshire coals I think are very dangerous.

2013. Could you give any general name that would describe them?—All the coal raised in Monmouthshire, which is not called the Elled vein.

2014. All other Monmouthshire coal you think liable to combustion?—Yes.

2015. (*Mr. Vivian.*) Would you say that all the Monmouthshire coals except the Elled vein are liable to spontaneous combustion?—They are more liable to spontaneous combustion than the Elled vein. I should say that all coal from the ironworks collieries of Monmouthshire and Glanmorganshire is very liable to combustion. We have a great deal of iron pyrites in the coal raised from those collieries, and I consider that a very dangerous coal.

2016. Are you acquainted with the Dowlais coal?—Yes, I have shipped a good deal of it.

2017. That is a first-class steam coal, is it not?—It is a very good steam coal.

2018. That is an ironworks coal, is it not?—Yes.

2019. Would not you except that?—No. I should say that was very liable to combustion.

2020. Dowlais coal is considered at least as good as any steam coal shipped in South Wales, is it not?—It is a beautiful coal.

2021. It is the highest class steam coal, is it not?—It is as good as the others.

2022. Do you consider that liable to spontaneous combustion?—They cannot keep the iron pyrites out of it. I have to see that they do not give us too much of it.

2023. Is not it worked from precisely the same beds as the regular steam coals of South Wales?—Yes, but it has this pyrites in it which is very difficult to entirely eradicate, and there being a great deal of that amongst the coal, it makes it a dangerous coal; there is more pyrites in the Dowlais coal than any coal I have to ship.

2024. Have you ever known any instance of Dowlais steam coal igniting spontaneausly?—Never one.

2025. The Cyfarthfa, Messrs. Crawshaw's, is a first class steam coal, is it not?—Yes.

2026. Have you ever known a case of that firing?—I think I have. I have heard of several cases, I think. I have not a very good opinion of that coal.

2027. Fothergills, Aberdare, is a coal which is very largely shipped, is it not?—Yes, that is a very dangerous coal.

2028. Have you ever heard of an instance of that catching fire?—No, it is full of iron pyrites.

2029. You have never known an instance of its catching fire?—No, I have never heard of one. We have never shipped much of it.

2030. That coal is as largely shipped as any coal in South Wales, is it not?—Yes, the only cargo of that that I had to do with I had to reject in consequence of its being very dirty and unfit for steam purposes. It is full of iron pyrites.

2031. Still you have never known any case of its catching fire?—No, but I do not look after cases of casualties that occur in any other coal but my own coal.

2031*a*. It is a matter of great importance to you to keep yourself well informed as to what coals do catch fire, is it not?—No, I cannot say that it is. It is not a matter my employers want to be posted up in at all. Till I had the honour of receiving a communication from you the matter has hardly been under my consideration at all.

2032. What coals have you chiefly shipped?—The best Aberdare and Merthyr coals, Davis's Merthyr and Ferndale, and we used to ship the best of the Powell Duffryn and Insoll's Merthyr.

2033. Nixon's?—No, I have shipped some of Nixon's, but not for my employers.

2034. Have you had no experience of spontaneous combustion in shipping any of the other coals you have mentioned?—I have had Fothergill's Aberdare under my notice and several of the Dowlais coals, which I consider very valuable coal, but very dangerous coal, so far as spontaneous combustion goes, if there is such a thing as spontaneous combustion. I should think it would generate gas very quickly.

2035. That would be inflammable gas, would it not?—Yes.

2036. That is to say, it would be liable to gas explosion, but not spontaneous combustion?—Just so. My private opinion is that most of the cases that are reported as cases of spontaneous combustion are merely from ignition.

2037. Are you aware that most of those cases occur after a lengthened voyage?—No, I have not heard that.

2038. The evidence received by the Commission is that the cases of fire occur pretty generally towards the end of the voyage after the ships have been at sea a long time?—I have not heard that.

2039. By that time the volatile gases would have passed off to a great extent, would they not?—No, I think not. Probably they have kept the hatchways bunged up, and there has not been any proper ventilation, and then some fellow has gone down with a light and made the coal ignite, but, of course, they would not acknowledge that; they would rather have it spontaneous combustion.

2040. Have you shipped patent fuel?—I have shipped some fuel at Swansea, several cargoes of it.

2041. Have you ever heard of patent fuel catching fire?—I have not.

2042. (*Mr. Fenwick.*) Is the coal which is shipped at Cardiff for the Peninsular and Oriental chiefly for short voyages for the Mediterranean?—A great deal of it is shipped for India, quite half of it goes to India.

2043. Is that shipped in sailing vessels or steam vessels?—Principally in sailing vessels. We ship to

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Bombay in steamers. Formerly we used to ship by sailing vessels.

2044. In what kind of ship is the coal for the Mediterranean carried ?—Mostly in steamers.

2045. You do not advocate ventilation ?—No, not internal ventilation, but surface ventilation.

2046. (*Mr. Icely.*) Will you inform the Commission what your instructions are with regard to the shipment of coal for the Peninsular and Oriental Company ?—To see that we get coal instead of stones; that is all.

2047. Nothing beyond ?—To see that the coal is carefully shipped ?—The principal thing is to see that we get good coal according to contract.

2048. Have you any directions as to screening the coal ?—We do not screen our coal now; we formerly used to screen it.

2049. Is not it even single screened ?—It is screened at the pit's mouth; that is all.

2050. You have had no cases of spontaneous combustion in the coal you have shipped for the Peninsular and Oriental ?—None whatever. I have only had one case of ignition. In that case the ship was loaded with ocean coal at Penarth. That casualty occurred about two days before she left the dock.

2051. If there had been any such cases, would they have come to your knowledge ?—Yes. Of course inquiries would have been made as to the condition of the coal when shipped.

2052. When once the coal is shipped if any accident happens to the cargo, have the Peninsular and Oriental Company to suffer the loss ?—They insure the cargo.

2053. Do you attribute this immunity from danger in the case of the Peninsular and Oriental shipments, compared to those of other people, to the careful inspection given by you as the representative of the Peninsular and Oriental Company ?—No, I cannot say that.

2054. You do not think it is owing to their careful inspection that the immunity from danger has arisen ?—I am rather diffident in answering your question, for I might be supposed to be lauding my own carefulness, but we see that the coal is in good order before it is shipped.

2055. There is no screening, I understand you to say ?—Not lately. We ship it colliery screened.

2056. It is screened at the colliery, but not on shipment ?—No, except on certain occasions. If I see the coal has got too much small in it, they screen it again upon my suggestion, but we do not pay for it screened; we pay for it colliery screened.

2057. (*Mr. Abel.*) You referred to one case of explosion on board a coal-laden ship; could you give us any details with regard to that ?—[*]

2058. That is the only instance of explosion which has come under your personal knowledge ?—Yes.

2059. Yet you consider explosion is the main danger to be guarded against in the shipping of coal ?—Yes.

2060. You do not believe there is any great liability to spontaneous heating of coal ?—Not in the coal I am speaking of.

2061. (*Dr. Percy.*) I think you said that you reject coal containing iron pyrites in any sensible amount ?—Yes, I frequently do.

2062. Simply because the coal containing it would be of inferior value, not as I understand you, because you feared that spontaneous combustion might arise from it ?—No, merely because I considered it inferior coal, having regard to the inferior qualities required of it as steam coal; not with regard to liability to spontaneous combustion.

2063. Is not it conceivable in the case of coal inspection that there may be such a thing as a very strong temptation offered to the inspector to pass coal of inferior value ?—Quite conceivable.

2064. Where a man was not proof against temptation ?—Quite possible, probable, and very likely exists.

2065. (*Mr. Duncan.*) Quite irrespective of inspection at all, there is a large quantity of coal shipped that is in your judgment unfit to be sent on long voyages ?—A great deal.

2066. People buy a cargo of coal and take the chance of it ?—They take the chance of losing the ship and everything else.

2067. (*Chairman.*) It is your view that a coal may be an admirable steam coal and very good for shipment for a short distance that ought not to be shipped for voyages of three and four months ?—Exactly so; and no people having any knowledge of that coal would sent it on a long voyage.

2068. Do you know anything about the rate of insurance upon coal now ?—No.

2069. You said that the Peninsular and Oriental insured their coal. Do you mean they insure it in their assurance fund ?—They insure it at Lloyd's or in other offices. I think it is rather a considerable item with them.

2070. Their insurance fund only covers their own ships ?—Yes, their cargoes are insured in outside offices.

2071. (*Mr. Duncan.*) Do you think, owing to the pressure there was during the last two years 1873 and 1874 to get coals, people were tempted to send away a class of coal that would not have been sent away at other times ?—I think so, certainly. They would ship anything black.

2072. (*Mr. Vivian.*) You have stated generally that you consider ironworks' coal to be liable to spontaneous combustion and dangerous. Could you when you return home refer to the shipments of ironworks' coal and furnish the Commission with any data upon which your opinion is based ?—No, I could not. It is only my personal conviction.

2073. It is an opinion based upon no facts whatever ?—It is only my opinion.

The witness withdrew.

Adjourned to Tuesday next at 12 o'clock.

Tuesday, 6th July 1875.

PRESENT :

THE RIGHT HON. HUGH C. E. CHILDERS, M.P., IN THE CHAIR.

SIR GEORGE ELLIOT, Bart., M.P.
HENRY HUSSEY VIVIAN, Esq., M.P.
FREDERICK AUGUSTUS ABEL, Esq., F.R.S.
RICHARD CORY, Esq.
GEORGE DUNCAN, Esq.
JOHN FENWICK, Esq.
CHARLES ICELY, Esq.
JOHN PERCY, Esq., M.D., F.R.S.
WILLIAM YOUNG, Esq.

J. M. CARMICHAEL, Esq., Secretary.

MR. EDWARD BENJAMIN SCOTT examined.

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2074. (*Chairman.*) You are a partner in the firm of Messrs. Gillespy and Scott ?—I am.

2075. You have had great experience in the shipment of coal ?—Yes, for a considerable number of years.

2076. Your firm has been established some years ?—Since 1857.

2077. You have shipped largely for Her Majesty's service ?—Yes, and our predecessor also, Mr. Thomas Gillespy.

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[*] Mr. Roberts thought he would be able to furnish these details on his return to Cardiff, but he subsequently wrote :—"I regret being unable to give any particulars of the reported case of explosion in a coal-laden vessel as referred to in my evidence before the Royal Commission on Spontaneous Combustion in Coal (in No. 2057), as I find that upon the investigation held on this case, it was considered doubtful whether it was an explosion of gas or ignited by a fire (that the coal itself was ignited by a fire arising from the vessel's stores having first ignited").

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2078. From what ports have you usually shipped?—From all the coal-shipping ports, that is to say all the ports from which steam coal is shipped.

2079. Both in the North of England and in South Wales?—Yes.

2080. And from Birkenhead?—Yes, but principally from South Wales.

2081. You have paid some attention to the question which is referred to this Commission for consideration, viz. the spontaneous combustion of coal on board ships?—Yes.

2082. Will you tell the Commission from your experience what descriptions of coal appear to you to be most liable to spontaneous ignition on board ships?—I think it is more especially the smelting coals that are shipped for the west coast of South America that are liable to take fire; it is in those coals that we have noticed the greater number of burnings.

2083. Have you any details or particulars of any cases of such coal taking fire spontaneously?—Not of smelting coal; we have never shipped smelting coal ourselves; our experience has been almost exclusively in steam coal.

2084. Have you known any instances of steam coal having taken fire?—Yes.

2085. Will you give the Commission the particulars of any such cases?—The first case of which we have record is that of the "Mary Shepherd," in the year 1852. She loaded a cargo of Swansea coal. The coal was brought up to London at the time when the railway traffic in coal from South Wales to London was just beginning to be developed, and was shipped in the West India Dock. She went on a voyage to Shanghai; we traced her to the Straits of Sunda, and after that she was burnt somewhere in the China Sea.

2086. Have you any details of that casualty?—The vessel was originally classed A1. at Lloyd's for 10 years; and in 1852, when the shipment was made, she was four years old. She was a sailing vessel, built of wood, and believed to be tight. Her builders' tonnage was 550, and her registered tonnage 629. The coal was Resolven coal, from the Resolven colliery, and it was hand picked. The ships' hold was ventilated by a wooden trunk alongside the keelson, and two or three shafts thence to the hatchways. It is believed that the fire originated in the fore-hold. The coal had been on board at the time of the burning from four to five months. It was brought by the Great Western Railway from Swansea, to Bull's Bridge, near Brentford, and was conveyed thence by lighters to the West India Dock. It was a considerable time on the road; longer than would be the case at the present time. The coal was put on board from lighters brought alongside, and was lowered into the hold by baskets. The weather was wet during the loading. No record was kept of the weather during the voyage. The cargo was in the course of its transit from Swansea exposed to the weather to a considerable extent, and it was exceedingly wet at that time. With regard to indications of the cargo being heated before the fire broke out, there were hot decks for some time before the fire. With regard to whether the hold was completely filled up to the hatches, we cannot say exactly, but most probably it was not filled up to the hatches. The only suggestion that we ever heard made with regard to the cause of the spontaneous combustion was that the coal was shipped wet. At that time the subject did not attract the attention it has since done.

2087. Did you consider that the ventilation in that case had anything to do with either checking, or promoting the combustion?—At that time we did not. We thought that to ventilate was the proper thing, but it was entirely the owner's own act; that cargo was on his own account; we made no suggestion on the subject, I think.

2088. Have you any other case?—The next case we had was in the year 1855, that of a vessel called the "Robert Watson." She was built in Newcastle, in 1839. At the time of her being laden, on the 22nd October, 1855, she was classed at Lloyds Æ1. She was laden with 554 tons of Coed Talon steam coal. She put into Queenstown from stress of weather, where a portion of the cargo was landed while the damage to the ship was repaired. On her arrival at Constantinople the cargo was reported to be "heated, and "both bad and dangerous, full of iron and sulphur." This was shipped for the Admiralty, and I find that some communication was made to the Admiralty upon the report which came from Constantinople, but I cannot trace what the explanation given was. The letter-book says simply, "I have the honour to enclose "letter from shipper on the subject," without giving the particulars. That cargo was condemned and was sold there (at Constantinople). If I remember rightly the vessel was somewhat charred by the fire.

2089. Do you attribute that combustion to any special cause?—Coed Talon is a coal which we have always had some doubt about.

2090. Because it contains much pyrites?—Yes, that is the idea we have had of it always, but I could not positively say that that was the cause of the state of the Robert Watson's cargo.

2091. Was the ship ventilated?—I do not know.

Then again, during the years from 1857 to 1863, we shipped 372,000 tons of coal altogether, principally for Her Majesty's service, the Imperial Marine of France, and the Peninsular and Oriental. The bulk of the coal was steam coal, principally from Wales, but a portion was gas coal. We gave no instructions about ventilation, and we had no case of explosion or combustion reported during that period.

2092. Do you remember what coal that was?—I could not give you the names of the coals; they were the ordinary Cardiff steam coals on the Admiralty list at that time. They were all first-class coals; Nixon's Merthyr, Fothergill's Aberdare, Davis's Merthyr, Ocean, Nantmelyn, and others. During this period, from 1857 to 1863, all the ships carrying the quantity of coal I have mentioned were accounted for, except the "Marlborough," the "George Bradford," and the "Middlesex," which were missing, and the "Cassange" which was known to have been burnt. The "Marlborough" was a new high-classed British ship, which was loaded on the 24th of April 1875 with a cargo of Buddle's West Hartley steam coal for Shanghai. She is supposed to have been lost in a typhoon after passing the Straits of Sunda. The two other vessels, the "George Bradford" and the "Middlesex," were despatched to Halifax in January 1862, at the time of the "Trent" affair. They went away unusually early, and they were supposed to have foundered soon after leaving Cardiff, because another vessel named the "Force," that went away at the same period foundered also, but the crew were saved. The only case of burning that we know of out of all those shipments was the "Cassange," a vessel belonging to Messrs. James Thomson and Co., of Billiter Square. Her cargo consisted of 534 tons of Parson's Abbey Graigola coal, and it was shipped on account of private merchants for St. Helena. The bill of lading is dated Swansea, October 1863. She was 369 tons register, built at Sunderland in 1854, and classed at Lloyd's A 1 for 10 years. She was burnt on the way; I do not know at what particular part of the voyage. That carries us up to 1863. Then in the years between 1864 and 1874 we shipped 304,405 tons. That makes a total of nearly a million and a half of tons shipped over sea in the period I am speaking of; that is to say from 1854 onwards. All the vessels carrying these 304,505 tons, and also the coasting vessels, were accounted for and need no remark, except the "Princess Alexandra" and the "Pocahontas." The "Princess Alexandra" was a steamer laden in 1868 with 723 tons of unscreened Londonderry gas coal for Limerick; an explosion of gas took place in that vessel off Dover; the ship was set on fire and otherwise damaged, and some of the crew were injured and killed. About 100 tons of coal were taken out of the ship, and she went on and discharged the remainder of the cargo at Limerick.

2093. (*Dr. Percy.*) What was the name of the coal?—It was unscreened Londonderry gas coal from

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the Londonderry collieries. Then the "Pocahontas" was loaded with 1,580 tons of Hastings Hartley. That coal was shipped for Bombay under contract for the Council of India, and was shipped under the superintendence of Captain Steinson the inspector in the Tyne district. She was an American ship of 1,112 tons register, and at the time we took her she was classed in Veritas 5/6 1-1. She sailed from the Tyne on the 13th of July 1874, and was abandoned on the 10th September in latitude 20° South, and longitude 33° West. We had from the master a letter from which I have here an extract which I will read. He writes thus to Mr. David Brown who was the broker, "I arrived home all right two weeks ago. Four "Boston ships have been burnt this season within "two months of each other, and the accounts given "are about alike as to the cause of the fire. My experience, so far as regards ships taking fire is, the "coal heats, and being more or less sulphur in it of "course it ignites. In most every case ships have "taken fire in the hatchways where the fine coal is. "If ships could be loaded so as to avoid the fine "coals collecting in the hatchways, and some means "taken to prevent the coals breaking up, I do not "think fire would occur. I would not advise loading "the ship too quick; we was loaded in three days. "The coal should be perfectly dry and *not fresh* "*mined.* The only way that I know of, is to prevent "the fine coal accumulating in any place, and if "any ventilators is put in the hatches, I would "advise to put in as many as possible; say not less "than four in each hatch, in each corner; by that "way you separate the fine coal and it cools "quicker. One ventilator in my opinion is of no "account. I would platform the ship fore and aft, "so to give circulation of air under the cargo. Some "means must be taken to prevent the coals from "heating. I could not suggest anything different, "except loading the coals in large casks. Quite an "excitement is created here in the insurance com"panies; some of the offices will not take a ship with "export coal at any rate; some have raised the rates, "as it would not pay to keep ships insured carrying "coal at the present rates." Then I have extracts from the log book, which give more details. The first entry from the log book bears date July 26th (13 days after sailing), when squally weather was experienced, and the ship laboured heavily. Then, on August 30th, 48 days after sailing, there is this entry: "An unusual "strong smell of gas was noticed in the fore between "decks at 5.30 a.m. At 7.30 a.m. blue smoke was "seen coming from the port side. The hands were "set to clear away and put coal in 'tween decks. At "the depth of six feet the heat and smoke prevented "further work. Water was thrown down, and about "15 tons of the burning coal jettisoned. When about "two feet from the keelson, the smoke stopped, and "no more burning coal was found." Then, on September 8th, nine days later, there is reported "A strong "smell of burning coal and a light grey smoke. This "smell continued throughout the night all over the "ship; and then a blue smoke was observed coming "out of the after hatch. On the following day, Sep"tember 9th, the smoke increased, and more coal was "jettisoned till the hands got down to the 'tween "decks, when the heat and smoke prevented further "work. The hatches were then closed and the ship "squared away for Bahia. On the next morning the "smoke increased, and the water in the main tank "became very warm. The fire therefore must have "been in the lower hold somewhere aft of mainmast. "On the following day, September 11th, as the smoke "was coming out of the hatchway in dense volumes, "and the coals were heard 'cracking as if the flames "'might break out any minute,' the 'Pocahontas' was "abandoned. At 4 p.m. the flames burst through the "after hatch, and after burning fiercely until midnight "the vessel sank on the 60th day after sailing."

2094. (*Chairman.*) That is the last case you have?—Yes. The casualties that have happened among our shipments, which have amounted to a million and a half tons of coal since 1853, have been comparatively very few; it was not till we came to the year 1874 that we had any case of a serious character, namely, the "Pocahontas," and that ship was loaded with a coal in which we should never have expected such a thing to take place. I never heard of such a thing before as "Hastings Hartley" taking fire.

2095. In one passage of the letter which you read from the captain of the "Pocahontas" he speaks of the insurance offices at Boston being alarmed at the large number of casualties in coal-laden ships; is it your experience that of late years the percentage of cases of spontaneous combustion in coal-laden ships has increased?—Yes.

2096. And the effect has been to alarm the insurance offices both here and in America?—Yes.

2097. Have the premiums risen?—Very much. I should describe them now as being quite "fancy" rates.

2098. To what do you attribute that increase in the percentage of cases of spontaneous combustion?—I am quite at a loss to explain the cause of it.

2099. A great many causes have been suggested. By some it has been attributed to excess of ventilation, and by others to absence of ventilation. Have you formed any opinion on that point?—The opinion I have formed is that a ventilated ship is certain to be destroyed if combustion be once set up.

2100. You are averse to ventilation?—Yes.

2101. By ventilation you mean ventilation through the coal?—Yes.

2102. You would keep the surface of the coal ventilated, would you not?—As often as the weather would permit.

2103. So as to carry off the gases?—Yes, but that we have given so little heed to, that with the exception of telling the master to keep his hatches off in the first instance for 48 hours, we have never thought it necessary to give any special instructions either in the case of our own ships or in the case of any that we have chartered.

2104. As regards ventilation through the coal, you have formed a distinct opinion that ventilation through the coal is prejudicial?—Yes.

2105. Have you formed any opinion whether the increased number of these casualties is due to a different method of conveying the coal to the ship being adopted, or to a more rapid shipment of it, or any cause of that kind?—The only conclusion to which I can come, (but that to my mind is not satisfactory,) is, that in consequence of the increased cost of labour there has not been latterly the same care exercised that there was formerly in taking out the pyrites—in cleaning the coal.

2106. Do you attribute the whole or the majority of these casualties to the presence of pyrites in the coal?—I could not say that that is a positive conclusion to which I have come, but it is the only conclusion I have been able to come to.

2107. The captain of the "Pocahontas" speaks of the mischief arising from the accumulation of large quantities of small coal—is it your impression that small coal is specially liable to spontaneous ignition irrespective of the presence of pyrites?—I think it is; from its lying so much closer together—that is a condition which seems to favour the production and spread of combustion.

2108. Have you formed any view as to the special liability of particular seams of coal to casualties of this kind?—No; our opinion is adverse to North Wales coal and to the smelting coal of South Wales.

2109. But there is no special seam of South Wales coal which appears to be more dangerous than another, as far as your experience goes?—No, we do not know from what seams coals are shipped; we simply take a man's coals; he mixes them according to his own convenience. We only know the pit that the coal comes from, not the seam.

2110. Have you any experience of any special liability of north country coal to spontaneous combustion?—No; the only case we have had was the case of the "Pocahontas."

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2111. You have formed no conclusion as to the special liability of particular classes of north country coal to spontaneous combustion?—No, our evidence is rather negative than otherwise on the subject.

2112. (*Mr. Young.*) You said that you disapproved of ventilators altogether?—Yes, I think ventilation insures the destruction of the ship when combustion is once set up.

2113. You mean ventilation by wooden ventilators? —By the wooden ventilators which are usually used.

2114. Could not a ship be ventilated with metal ventilators?—Yes, she could be; but I think that the ventilation would tend to produce the destruction of the ship as I have already explained.

2115. Have you ever given any instructions, or have you ever known of any instructions being given to test the heat of the coal by the thermometer during the course of the voyage?—No, I have not. We have never given any instructions ourselves with regard to ventilation; our ships have been small ships, and we have shipped our cargoes in the usual way without ventilation.

2116. It appears that during the year 1874 there were more cases of spontaneous combustion than usual. Do you think that arose in any way from the ships not perhaps paying so well, the cost of them being greater than usual, insurances being higher, and captains not being so careful to stick to their vessels than at other times?—No, I have not the slightest idea that that would have anything to do with it.

2117. Do you find that more vessels are lost when they cease to pay than when they are paying?—That does not come within the range of my experience.

2118. In 1874 there were more cases than before—you do not think that the inference to be drawn from that is that the vessels were not paying the shipowners?—I have not any idea of any connection between the two things.

2119. (*Mr. Cory.*) Do you think, as far as your experience as a shipowner goes, that the captain of a ship knows anything about the insurance at all?—Frequently he does not.

2120. Do you yourselves inform your captains that your vessels are insured?—We say this—it is almost a stereotyped form—"The ship is insured, principally "in clubs, but on all occasions you are to please to "bear in mind that you are to act as if we were not "insured."

2121. (*Mr. Abel.*) You stated that you entertain a decided opinion that ventilation is prejudicial to the safety of a coal cargo, can you give a precise reason for that opinion?—I said this, that where combustion is set up I think the effect of ventilation is to insure the destruction of the ship; that is, it soon becomes unconquerable.

2122. You draw a distinction between the effect of ventilation as promoting combustion or heating when once it is established, and as giving rise to the first development heat?—Yes, I should say it would tend to prevent heating in the first instance, but if it is not successful in preventing the heating, then it insures the destruction of both ship and cargo.

2123. You think if it were possible to have a sufficiently efficient mode of ventilation it would be decidedly safe to adopt such a system of ventilation, and that it would tend to prevent spontaneous heating?—If you could ensure a sufficiently efficient mode of ventilation, but the question is how to ensure it.

2124. With regard to the condition of the coal as loaded, that is to say the size of the coal, do you believe that is a matter closely connected with the question of ventilation?—Where there was a large quantity of small coal I think it would be more likely that there would be impurities in the coal.

2125. It is not because you think that the coal would be less ventilated where it was small than where it was large that you consider it would be more liable to ignite spontaneously, but because the small coal contains more impurities?—I think the two things come in; there would, in the first place, be more impurities in the small coal than in the large, and next, from lying so close together, the effect would be more rapidly to increase combustion.

2126. I understand that an accumulation of small coal is caused, in a great measure, from the manner in which the coal is loaded on board ship; that it collects at the hatches, where the coals become more or less broken up?—Yes.

2127. Would there be more impurity in that coal because it happened to be in small particles than if it were in large lumps?—The impurities, in the way of pyrites, would be picked out of the large lumps when it could not be seen in the small; but the accumulation of small coal in the hatchway would arise from the fall of the coal in that particular spot, and there, I apprehend, there would be a want of ventilation, from the coal lying more closely together, and it would be there that combustion would be likely to take place.

2128. The effect due to the smallness of the coal in the hatchways would be an effect apart from that due to impurity of the coal?—Yes

2129. In the single case of explosion which you referred to, was there any damage done?—The ship was set on fire by it.

2130. Was the coal set on fire?—Yes, the coal was somewhat burnt—nothing to speak of; it was left behind in order to lighten the vessel.

2131. Was the vessel ventilated?—No, she was a steamer. She was loaded in a few hours, and there was no arrangement for ventilation.

2132. Was that vessel overloaded?—No. I forget her tonnage now, but she only carried 720 tons of coal. That was about her average cargo. She belonged to a company in Limerick, and she went round from Sunderland to Limerick. It was off Dover that the explosion took place.

2133. (*Dr. Percy.*) Are some smelting coals according to your experience more liable to spontaneous ignition than others?—We are not shippers of smelting coal.

2134. You expressed an opinion that such coal was more liable to spontaneous ignition than any other kind of coal?—Yes.

2135. You do not know whether certain kinds of that coal are more liable to spontaneous ignition than other kinds?—No.

2136. With regard to the presence of pyrites in such coal, have you any experience upon that point? —No, we have not.

2137. You spoke of a cargo of Coed Talon which was shipped in the "Robert Watson," which afterwards took fire, and which you said was full of pyrites. Was there any Admiralty inspection of that cargo?—No; that was before the Admiralty inspection took place. The Admiralty inspection was not established till 1859, and that casualty happened in 1855.

2138. Have you any personal knowledge yourself of any case of spontaneous ignition—have you watched the conditions under which such spontaneous ignition has occurred?—No, I have never had an opportunity of doing that.

2139. Have you any experience of the effect of the atmospheric temperature in promoting spontaneous ignition?—No.

2140. Do you think an elevated atmospheric temperature would be likely to promote ignition?—As a matter of theory, I should say so.

2141. With regard to the cleaning of coal from pyrites, is not it the case that very frequently coal contains pyrites in pretty large quantities which is not in lumps which could be picked out?—I believe so.

2142. Have you any experience with regard to the spontaneous ignition of such coal as that?—No.

2143. (*Mr. Duncan.*) Your experience would lead you to say that the best class of steam coal, when it is well cleaned, is not liable to spontaneous ignition at all?—That is the result of my experience.

2144. Upon the question of ventilation, I suppose your opinion is not based at all upon scientific considerations; that is to say, you do not consider whether the pyrites derives oxygen from the air

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passing through it or otherwise ?—No, it is not based upon that. We do not go into the scientific part of the question.

2145. You think if a ventilator were put into the coals, and the coals were set on fire, it would act as a chimney, and therefore would cause the fire to burn fiercer ?—Yes.

2146. You do not base your judgment as to the advantage or disadvantage of ventilation on any scientific considerations ?—No ; it is a sort of rule of thumb decision that we have come to upon the matter.

2147. (*Sir George Elliot.*) I think I remember your name as a buyer of coals for more than 20 years in the north of England ?—My firm and our predecessors have been buyers for more than 30 years.

2148. And you say you have purchased a million and a half of tons ?—Yes, since the year 1854 Mr. Thomas Gillespy and ourselves have shipped nearly a million and a half tons.

2149. What proportion of those coals would you take from the north of England ?—In the first period, 1854, 1855, and 1856, I should say fully half; subsequently it would be about two thirds Welsh coals, and one third north country.

2150. Out of all those shipments since 1854, how many cases of spontaneous combustion have you had in north country coal ?—The "Mary Shepherd" was before that date; that was 1852; that was Welsh coal. The next case, the "Robert Watson," was Coed Talon, North Wales coal, then the "Cassange," was South Wales coal. The only cargo of north country steam coal was the "Pocahontas."

2151. You have been shipping from the Tyne during a period of 20 years, about a third of that 1,500,000 tons of coals, that is to say 500,000; and you have only had one instance of spontaneous combustion in north country coal ?—Yes.

2152. And I understood you to say that you thought that casualty might have been due to the regular system of cleaning coal not having been attended to ?—That was the idea I formed about it as a matter of theory.

2153. As to ventilation, you do not seem to attach much importance to ventilation as a protection against spontaneous combustion ?—No ; we have never taken any precautions of that sort during the whole of that time.

2154. You have carried all this large quantity of coals up to 1874, shipping a great variety of north country coal from the Tyne ; and you have had no instances of spontaneous combustion except that one of the "Pocahontas ?"—That is all.

2155. And that was Hastings Hartley ?—Yes.

2156. Coed Talon you say contains rather a large proportion of pyrites ?—The report that came to us was, "Heated, and both bad and dangerous; full of iron and sulphur." That is the report of the Admiral. I believe it was Admiral Stewart.

2157. With respect to the case of the "Princess Alexandra," there was no spontaneous combustion there ?—No ; that was simply a case of explosion.

2158. In the case of the "Pocahontas," which loaded with Hastings Hartley, how many days was she out before the spontaneous combustion took place ? —48 days after she sailed there was an unusually strong smell of gas ; nine days later there was a strong smell of burning and a light grey smoke.

2159. Does anything occur to your mind that you would suggest with a view to prevent the occurrence of these casualties ?—No ; I have not been able to come to any satisfactory conclusion about the best way of preventing them.

2160. (*Chairman.*) Upon the whole you would recommend that coal cargoes should not be ventilated through ?—Judging from experience I come to that conclusion.

2161. As to the method of shipment, there are several methods in force in the north, at Liverpool, and in South Wales. There is, first, the common shipment from the tip, and, secondly, shipment by boxes ?—That is almost abandoned in Wales.

2162. Then there is a third plan of shipping by barrows ?—They ship by barrows at Birkenhead.

2163. And also in bags occasionally ?—That plan may be adopted in shipping coal for smelting purposes. I have no experience of that.

2164. Have you formed any strong opinion as to the advantage of one system over another ?—No ; I think looking at the peculiar quality of the coal, each mode seems to be well adapted to the particular coal. For instance, in the Welsh coals, there is a careful letting down of the coal by the tip, which slopes down to the ship and forms a screen when required. After the appointment of the Admiralty inspector, which was in 1862, we began to double screen, that is to say, to screen at the time of actual shipment, the small coal being taken away. That system has been given up of late and now we ship generally single screened for the Admiralty.

2165. The coal going from the tip with a heavy fall ?—No, not with a heavy fall ; it goes down the same incline, but there is no screen to take the small coal away.

2166. Do you think it desirable that there should be the least possible fall into the hold ?—Certainly.

2167. It is not very material which plan you adopt provided that result is arrived at ?—Quite so.

2168. (*Mr. Vivian.*) You mentioned one case of Resolven coal having fired, have you shipped other cargoes of Resolven coal ?—Yes, but generally speaking only coast-wise ; it is a very tender coal. We have shipped it largely principally to Devonport dockyard, and as far as Gibraltar, but I do not think any further. I believe it has been shipped over sea, but not by us.

2169. (*Mr. Icely.*) You were speaking of coals on Admiralty list. I presume the varieties on that list are coals of which the Admiralty approve ?—Yes, for their particular purposes.

2170. Have they a list of the coals of which they disapprove ?—Not that I am aware of. The condition of the Admiralty contracts was "that the coals to "be shipped were to be of such and such descriptions "at the option of the Lords Commissioners of the "Admiralty." Practically we knew that for certain purposes Resolven would do, and we knew equally that for certain other purposes we could not send Resolven, but must ship other coals named. At times the then Storekeeper-General used to come down with his big hammer, and by ordering "Nixon's navigation," for which we had to pay a heavy price, remind us that he, and not we, held the option of selecting the coal to be shipped.

2171. (*Chairman.*) That system is not in force now ?—No.

2172. (*Mr. Duncan.*) Putting coals on the Admiralty list amounted to this, did it not, that the owner of a particular colliery submitted his coals to a test, and if they went through the test satisfactorily to the Admiralty he was allowed to put the coals upon the list ?—Yes, they were then put on their list.

2173. As a rule it was the parties who owned the coals who submitted their coals to the proper test, and if they passed such test, they were allowed to be put on the Admiralty list ?—Yes, the Admiralty kept the control in their own hands as to what we should ship. Resolven would answer the purpose if it was to be used in Devonport dockyard, for instance ; but for over sea we should have had to take a higher class of coal.

2174. (*Sir George Elliot.*) The Admiralty did not shew any caprice in the matter ; any person who had a suitable coal, applied to have it tested, and if it came up to the test, it was at once placed on the list ?— Yes, there was nothing capricious about it.

2175. (*Mr. Young.*) Have you ever known a case of spontaneous combustion in a store of coals on shore ? —Yes, in the case of gas coals at Limerick, and also at depôts abroad. I have heard of it.

2176. In those cases were the coals exposed to the weather ?—Yes.

2177. I understood you to say that you thought that coal shipped wet was more liable to take fire than

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coals shipped dry?—That is the conclusion I have been compelled to arrive at.

2178. Would not it then be probable that if great quantities of coal were stored in the open air they would be liable to spontaneous combustion?—Yes, and it frequently takes place. I have frequently heard of cases of spontaneous combustion of coals stored in the open air.

The witness withdrew.

MR. JOHN GLASBROOK examined.

Mr. J. Glasbrook.

2179. (*Chairman.*) You are a colliery owner and mining engineer at Swansea?—Yes.

2180. Have you been engaged in shipping coal many years?—Yes, upwards of 30 years.

2181. What descriptions of coal have you been shipping?—Steam coal, mining coal, and house coal. We have only shipped one cargo of smelting coal, that was to the west coast of South America.

2182. Can you give us any information with regard to the liability of particular descriptions of coal shipped at Swansea to take fire spontaneously?—There are only two that are sure to go without spontaneous ignition, those are the 4-feet vein and the 6-feet vein.

2183. The other seams you look on as dangerous?—Yes, they contain impurities. We know that some coals will even take fire on the wharf if kept in too great a quantity. In fact if you have only 10 tons of coal from some of the veins it will take fire; that is the case with coal from the Cistern. I have proved it myself.

2184. (*Mr. Vivian.*) What do you say to the 5-feet vein?—The 5-feet vein is very bad.

2185. What do you say to Hughes's?—Hughes's vein is better than the rest, with the exception of the 6-feet. Hughes's is the next best to the 4-feet and the 6-feet; the 3-feet is very clean, but there is something in it that would deter shippers from shipping it.

2186. (*Chairman.*) You are speaking of Swansea coal?—Yes. This (*producing a diagram*) shows all the veins. This is the 4-feet, that is a good vein. Then here you have the 5-feet which is not a good vein. This is the 6-feet, which is a good vein; and this (*pointing to Hughes's*) is good if it is worked clean. I do not know much about the others. We own about 16 ships, and whenever No. 1, No. 2, or No. 3 Rhondda has been shipped it has taken fire.

2187. What experience have you personally of cases of spontaneous combustion?—One of our ships took fire last year. That was named the "Alpha."

2188. That is the only case of which you have personal experience?—Yes.

2189. In the evidence you have given you have been speaking generally as to your knowledge of the different coals of the district?—Yes.

2190. You also said that you had personal knowledge of coal taking fire on the wharf?—Yes, coal from the 5-feet vein. The Forest colliery has taken fire with us. Coals from the 6-feet and the 5-feet mixed together have taken fire. If you put any 5-feet with anything else there is a danger of its taking fire.

2191. You could put your finger on certain descriptions of coals and say, "These are unfit for shipment for a long voyage"?—Yes, large might go very well, but through and through will not.

2192. There are certain descriptions which you would say could not be safely shipped except for a short distance?—Yes, some of them will go a short distance safely, but some of them I would not trust for five days or seven days.

2193. From what seams would that coal be?—The seams above Hughes's, the Cistern, the Slatog, and the Bodor, that coal is very unfit to go anywhere, there is such a mixture of pyrites and shale in it, shale is the most dangerous thing of any, it contains so much oil and it gets heated. There are veins of coal containing small rods of small slaty stuff, and that breaks up with the small coal, and it is liable to take fire.

2194. What in your opinion are the causes of spontaneous combustion?—Impurities in the coal such as shale and dirt and earthy matter.

2195. Shale and earthy matter and pyrites?—Yes. and what is called "mother of coal" it is very good to burn, it makes very good coke.

2196. (*Dr. Percy.*) It is almost like charcoal?—Yes. I have some specimens of coal here. This (*producing a specimen*) contains a great deal of what they call "mother of coal" that is No. 3 Rhondda. This (*producing another piece*) is a piece of coal without any impurities whatever in it; that is sure to go without ignition, that is from the 6-feet vein.

2197. (*Chairman.*) Do you observe that these casualties have increased lately?—Yes, on account of the men not working the coal clean.

2198. Is that the only reason of the increase?—That is one of the principal reasons why there have been so many cargoes taking fire lately, that we cannot get the men to work the coal clean. They say, "If it does not suit you we will leave it."

2199. (*Dr. Percy.*) There seems to be pyrites in that coal which you say would be sure to go without ignition?—That is fat, I do not think there is any quantity of pyrites in it.

2200. (*Chairman.*) You say that one of the principal causes to which you attributed the increased number of casualties lately is the carelessness of the men in not working the coal clean?—Yes, people have been shipping coals that they would not ship at other times.

2201. The rise in the price of coal has induced the colliery proprietors to ship inferior coal?—Yes, they would ship coal which they would not have shipped at other times.

2202. Is the risk of spontaneous combustion taken into consideration, do you think, by persons shipping inferior coal?—The shippers do not care much what they take so long as they can insure the cargo, the shipowners have to look to that; we only take the 4-feet or the 6-feet in our ships.

2203. You consider that there was greatly increased carelessness in this respect while the price of coals was high?—Yes.

2204. Now that coal has fallen are shippers more careful?—Yes, they will not take any sort of coal now.

2205. Have you formed any judgment as to the advantage or disadvantage of ventilation in coal ships?—I believe separation is better than ventilation, I mean keeping the coal in compartments, small cargoes are not so liable to ignition as large ones. It is just the same as in the case of a large rick of hay, a large rick of hay is more liable to take fire than a small one when the hay is damp.

2206. You mean separation in the ship itself by the coal being placed in separate compartments?—Yes, I should divide the coals into compartments by a three-inch plank or something like that; copper ore vessels are fitted up with trunks and those trunks could be made into separating apparatus very well.

2207. Has that been practically done?—I do not think so; they take no such precautions generally.

2208. That is the precaution which you would take?—Yes, to separate the ship into three or four compartments, that would be in the case of coal which was liable to ignite spontaneously. There is some coal that does not require anything.

2209. As to the coal that would be liable to ignite, you would take that precaution of dividing the ship into compartments?—Yes, and ship the coal dry.

2210. As a shipowner you would limit your shipments to those two veins which you have described?—Yes.

2211. And you would not ship any coal from any other veins?—No, I would not take a cargo of any other description, we do not ensure our ships, and we are obliged to be very cautious.

2212. Have you studied the different methods of

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shipping coal ?—There are many modes of shipping coal. I ship my coal in boxes by the steam crane. That is a very good mode of shipment.

2213. That is an expensive method, is it not ?—No I think we save in labour after all.

2214. It does not add to the costs of loading ?—No, I do not think it does.

2215. Will you describe that method ?—Here is a box about a ton weight that is drawn up by a steam crane and dropped into the hold of the vessel; then they knock the bottom of the box off and the coal spreads away without any pressure to speak of instead of being dropped down and pounded as it is by the tipping process.

2216. So that the coal does not fall far ?—No; and the trimmers move it away as it falls.

2217. That is really on the whole not more expensive than the common method ?—No, I do not think it is.

2218. Summing up the remedies you have suggested, they are these. Only to ship coal which is not liable to ignition from the seams you have mentioned (that is speaking of Swansea coal); secondly, not to ventilate; and, thirdly, where the coal is liable at all to ignite, to divide the ship into compartments; and, fourthly, to lower the coal in boxes, trimming the coal at the bottom and so avoiding any fall ?—Yes, I would not object to those large shoots that they have for shipping coal, because the coal goes in as easily as by our boxes.

2219. That is to say the shoot which goes down actually into the hatchway ?—Yes, the coal drops by degrees.

2220. (*Mr. Vivian.*) You would fill up the hatchway by means of boxes ?—I should prefer to fill it up by boxes if possible, especially in the case of smelting coal.

2221. (*Chairman.*) Those are all the suggestions you would make ?—Yes.

2222. Do you think any of them should be enforced by law, or would you leave it to the shippers ?—I think you might very well leave it in the hands of the shipowners and the persons who ship the coal, but I believe it would be very beneficial if you could compel the men to work the coal clean. You have been legislating all for the men and the rest for the masters lately.

2223. You think something ought to be done in the other direction ?—I think so. We take care that the working people have their food unadulterated, and they ought to give us coal without adulteration where it is possible to be avoided.

2224. (*Mr. Vivian.*) You are working the 5-feet vein yourself, I believe ?—We are working it at Forest.

2225. And you contemplate working it again ?—Yes.

2226. So that the evidence you have given in regard to the 5-feet vein is against your own interest, you being yourself a proprietor of a colliery on that vein ?—Yes.

2227. Have none of the coals wrought from the 6-feet vein in collieries to the westward of Swansea, and shipped to the west coast of America fired ?—None, if they have been shipped alone. Sometimes they smuggle some of the 5-feet and some of the 3-feet vein with it, and then it is sure to take fire.

2228. (*Chairman.*) Who smuggles those coals in ?—The colliery proprietors; sometimes they are short of the other coal.

2229. (*Dr. Percy.*) Can you tell the coal when you see it ?—No; you must watch it, the 3-feet is very clean, and so is the 5-feet sometimes, but still there is a great deal of alum in the 5-feet.

2230. (*Mr. Vivian.*) The water coming from the 5-feet is very bad, is it not ?—Yes.

2231. (*Dr. Percy.*) Very bad for steam boilers ?—Yes; it will eat a hole in a shovel in a night.

2232. (*Mr. Duncan.*) That is from the alum contained in it ?—Yes.

2233. (*Mr. Vivian.*) The price of the 5-feet, the 6-feet, and the 3-feet is the same, is it not ?—Pretty much the same.

2234. So that the only object would be to fill up the ship ?—Yes, when they were short of coal. There is a great demand for the 6-feet, and it is better than the other, but they will sometimes ship the others with it. I know Mr. Sterry has been short of the 6-feet, and he has plenty of the 5-feet.

2235. You have never known a case of the 4-feet firing ?—No.

2236. There is very little of the 4-feet shipped over sea, is there not ?—They used to ship a great deal of it from Primrose to South America; there is very little of it left.

2237. You mean that quality. There is plenty of the steam coal left ?—Yes.

2238. You have shipped large quantities of the 5 feet for short voyages, have you not ?—Yes, to Cornwall for instance.

2239. It is lying in heaps in Cornwall without firing, is it not ?—Yes, mixed with other coal in layers.

2240. Have you ever known any case of patent fuel firing ?—No.

2241. Do you believe it possible for patent fuel to fire ?—No; unless it fires from an explosion of gas the same as steam coal.

2242. It will not spontaneously ignite ?—No; you can build it up in chimneys and separate it in any way you like. I never heard of a cargo of patent fuel taking fire.

2243. Nor of explosion ?—No.

2244. We have had some evidence as to Tyr Edmund taking fire ?—That is sure to take fire if mixed with Abbey Graigola.

2245. What vein is that in the Swansea coal measures ?—That is above the 6-feet.

2246. That would come under the class of veins which you mentioned as likely to take fire ?—Yes, it is sure to take fire if mixed with the 6-feet; it is full of impurities.

2247. The Abbey Graigola is much the same, is not it ?—The Abbey Graigola is the 6-feet vein; that is clean and always goes without danger.

2248. They work the Tyr Edmund with it ?—Yes, they ship it with it.

2249. So that if you heard of Abbey Graigola taking fire you would think that some Tyr Edmund had been shipped with it ?—Yes, there is no mistake about it.

2250. You would recommend that coals from veins which are known to be spontaneously ignitable should not be shipped for long voyages ?—Yes; I should keep them at home for the manufactories and ship the others.

2251. You spoke of the Rhondda veins being liable to spontaneous ignition, can you mention any specific cases of Rhondda coals having fired ?—I believe that every ship that went from Cardiff with Rhondda coal Nos. 1, 2, and 3 during the scarcity of coal took fire. Four cargoes of Cardiff coal ignited last year, viz., "Lady Heathcote," 527 tons; "Admiral Fitzroy," 550 tons; "Ermenigilda Donovara," 1,100 tons; and "Clothilde," 746 tons.

2252. Shipped on long voyages for smelting purposes ?—Yes.

2253. That is not steam coal ?—No, steam coal goes safe enough; that is shipped large, and there are no impurities in it.

2254. Does it increase the cost of shipment much to fill up the hatchway by means of boxes in the first instance ?—No, nor even with baskets.

2255. Having filled up the hatchway by means of boxes to a certain extent you go on tipping in the ordinary manner ?—Yes, because the men can shift the coals.

2256. Have you seen the mode of shipment adopted in the New Alexandra docks at Newport ?—No; I believe it is pretty much the same as that adopted at Swansea at the new drops. They have a crane at Swansea and I see the same at Newport.

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2257. Your recommendation would be that to each drop there should be a jib attached?—Yes; I would have the first 100 tons shipped by means of boxes, it would not increase the cost much.

2258. To fill up the hatchway?—Yes, that would save the coal from breaking so much.

2259. With regard to the fires which you say occurred from Rhondda coal, did those fires occur previously to the last three years or during the last three years?—I believe there was none of it shipped before the last two or three years. I believe the shipments of coal to South America were confined entirely to Swansea and Liverpool.

2260. It was the want of coal at Swansea that drove shippers to Cardiff you think?—Yes, that drove them to take Rhondda coal from Cardiff; I believe the Morfa went very well.

2261. You never heard of a case of the Morfa taking fire?—No.

2262. The other veins in the Rhondda valley except those you mentioned are not liable to catch fire?—I never heard of one.

2263. (*Mr. Cory.*) Is it a common thing in your district for the same colliery to work the coal from several seams?—Yes; they bring up the same shaft the produce of the several seams, but generally they have separate tipping places for it.

2264. The coals from all those various seams come up at the same time?—No; not in the same tram.

2265. At the same time?—No; they will raise what they call "a party," that is 10 or 12 trams in one vein, and then they will raise a "party" in another vein.

2266. Do not you think that if a colliery was in full work there would be a considerable chance of a mixture taking place under such circumstances without the coal owner intentionally mixing the coals?—They never do mix them, they keep them separate the 3-feet vein generally is all sold for the works, it works very small, and the other works very large; the large is shipped and the small is sold to the works.

2267. Do you think in the case of a considerable pressure of business the people at the colliery could succeed in keeping the produce of each vein separate?—As far as I understand they do it. I always do it when I work coal. I have been working many seams in the same pit, and always separated them.

2268. (*Mr. Vivian.*) You can not only keep the produce of each vein separate, but you know what man has worked each tram, do you not?—Yes.

2269. And you know from what portion of the ground it comes, so that if you have different royalties you can distinguish the one from the other?—Yes, the name of the cutter is on the tram, and they tip one man's coal into one shoot and tip another man's coal into another.

2270. (*Mr. Cory.*) When the coal is delivered down those shoots it goes into trucks?—Yes.

2271. What sized trucks?—10-tons trucks.

2272. You do not consider that the coal is liable to get mixed?—No, unless it is done on purpose.

2273. (*Mr. Fenwick.*) I understand that you do not insure your ships?—No, we do not insure them, we put so much by for an insurance fund; *i.e.*, the Swansea Merchant Shipowners Company's ships.

2274. Have you always done that?—Yes, after we had 10 ships. We acquired our ships gradually, and when we acquired 10 we did not insure.

2275. You have not insured since the premiums have risen?—No, nor before. We did not insure after we had the first 10. The "Alpha" luckily was insured, she was one of the first 10, the others have gone safely.

2276. As a matter of business do you gain by insuring yourselves?—Yes, at the present freight it would take all the profit. We get enough in the year out of the 16 to purchase a new ship to replace one if she is lost.

2277. (*Mr. Icely.*) What is the general impression at Swansea with regard to the ventilation of ships, is it in favour of it or otherwise?—The overlooker of our own ships, Captain Pear, thinks it is better not to ventilate, he thinks it is better to select your coal and not ventilate; if you ventilate in good coal you do no harm; if you ventilate in bad coal you assist the burning after it once ignites.

2278. You are in favour of no ventilation at all?—I am in favour of separation.

2279. Do not you consider surface ventilation at all necessary?—Yes, I think it is advisable to keep the hatches off as much as you can.

2280. Would you do anything else in the way of ventilation besides keeping the hatches off?—I would have a pipe something like what you have for the ventilation of steamers.

2281. That is only to prevent danger from explosion?—Yes, it is almost in the bottom that the spontaneous ignition begins.

2282. Do you not think that pipes passing through the cargo and leading to the surface would be an advantage?—I do not think they would do any harm. You could then ascertain if there were any heat in the coal. I should like to have one or two to test whether there was any heat. You could do that by lowering a thermometer. I do not think it would do any harm, in fact I think it would do good.

2283. Have you given any instructions to the captains of your ships with regard to taking the temperature of the cargo?—I have not the management of the ships. We keep a manager and a secretary.

2284. You leave it entirely to them?—Yes, we have an experienced man as manager. He has been 25 voyages to south America, and he is a very experienced man, and a very intelligent man. We leave it all to him.

2285. I understand you that you have had no casualties among those 16 ships except the one you mentioned?—Where there was a mixture of the 4-feet and the 5-feet coal.

2286. Your impression is that that was the cause of the casualty?—We are certain of it, because if this 6-feet is shipped by itself it never takes fire, either wet or dry. It cuts in cubes and does not produce so much dead small dust.

2287. (*Mr. Young.*) Did all your 16 ships go to the west coast of South America?—Yes, with the exception of one of them, I think, which went to China or the West Indies.

2288. You have shipped coal in those ships at all periods of the year?—Yes, every month of the year.

2289. You are very careful as to the kind of coal you allow to go into your ships?—Yes, we only allow the 4-feet or the 6-feet to go into our ships.

2290. Because you do not insure?—Yes.

2291. Is it within your experience that during the two or three years when the price of coal was high, Nos. 1, 2, and 3 Rhondda was often shipped in other vessels?—I have not heard of it.

2292. I understood you to say that those coals had been mixed with other coals?—No, I said the 5-feet has been mixed with the 6-feet in some cases.

2293. Did the shipowners know in such cases that the colliery people were putting on board the 5-feet instead of the 6-feet?—No, but they know it now. A cargo of Rhyd-y-ffad went on fire lately.

2294. Your experience has led you to this conclusion, that if everyone was his own insurer there would be no necessity for legislative interference, every man would look after himself?—Yes; we shall never ship a cargo again without seeing what we get.

2295. You say that the coal from the Cistern veins is apt to take fire even at the pits' mouth?—You might have 10 tons down out of the pit to-day, and to-morrow you could not put your hand in it, it would be so hot.

2296. If that were shipped even for a short voyage it would be liable to take fire?—It would take fire before the ship was loaded. That is the Cistern vein, the next but one above the Hughes's vein.

2297. Have you ever known great quantities of coal lying on shore to take fire?—I never saw any coal take fire but the 5-feet.

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2298. (*Mr. Vivian.*) Is anyone working the Cistern?—Yes, Mr. Daniel.

2299. He works it for his own works, does he not?—He does not ship any.

2300. (*Mr. Young.*) Supposing a lot of coal to have a certain quantity of this 5-feet in it it would probably take fire?—Yes, that is to say, supposing it had some of this Cistern in it. The 5-feet is not so bad if it is worked clean.

2301. If any of this Cistern coal got into a lot of any other coal it would be likely to spontaneously ignite?—Yes.

2302. That is your experience?—Yes.

2303. (*Mr. Abel.*) With regard to these veins which you have described as being liable to ignite spontaneously, do you ascribe that liability to the character of the coal or to the impurities it contains?—Both. The coal contains impurities not so much in the way of shale and dirt as other impurities.

2304. Are the impurities in those particular coals so distributed through the coal that no amount of cleaning removes them?—They are in the nature of the coal.

2305. What are those impurities?—I believe sulphur and phosphorus.

2306. You said that the presence of shale and porous coal like "mother of coal" was more dangerous than pyrites?—That "mother of coal" is very dangerous, because it goes small amongst the coal, and if it is wet or damp it will heat. There is a great deal of vegetable matter in the "mother of coal, and there is a great deal of oil in shale.

2307. Some of those coals which contain a large quantity of shale and "mother of coal" do not contain much pyrites?—That is so; you can hardly sit in a room where this Cistern vein is burning, there is such a strong smell of sulphur.

2308. That shows that there is a great deal of pyrites in the Cistern vein?—Yes.

2309. Is there much shaly material in that vein?—Yes, there is some of that too.

2310. In some of the other coals the pyrites is small, but the shale and porous coal is considerable?—Yes, if you look up some of the knobs you would find that almost half would be shale.

2311. (*Dr. Percy.*) You say that if you stored 10 tons of that Cistern vein coal it would fire?—Yes; I might put 10 tons of it down to-day, and to-morrow I could not put my hand in it.

2312. Is that a hard coal?—No, not very hard.

2313. It will not stand weathering I suppose?—It will get as white as lime if you put it in the atmosphere a short time.

2314. Have you put your tongue to the coal to ascertain whether it tasted of iron?—No.

2315. Have you ever examined the white stuff that appears on the coal?—Yes, there is a great deal of salt in that, I think, and alum.

2316. Have you ever put your tongue to it?—There is a great deal of alum in the 5-feet.

2317. What leads you to say that there is phosphorus in the coal?—My impression is that the Cistern vein coal contains a great deal of phosphorus.

2318. Is that an impression merely, or a conclusion to which you have come from any chemical examination?—I have not had it analysed.

2319. (*Mr. Duncan.*) You have been asked whether the shipowners could prevent these bad coals from being put on board; many of them would not have the same facility of preventing it that you have?—No, not those at a distance unless they had an agent.

2320. If they had an agent he might be a man that they could not depend on?—Yes.

2321. The underwriters sometimes raise a question with regard to the coal a shipowner is proposing to take in his ship?—Yes.

2322. You mentioned that the coals from some of those veins, for instance the 4-feet and the 6-feet, might go safely if they were clean worked?—Yes, Hughes' vein is as pure as any vein if properly worked, but there is a bed of rubbish on the coal and another in the seam, and the bottom sometimes is bad while at other times it is good.

2323. Both the colliery owners and the miners are perfectly cognizant of those bad parts?—Yes.

2324. And as a rule the colliery owner does not want to have those bad parts wrought along with the others, but the miners are not careful to avoid working those bad parts?—If we could prevail on the men to work the coal clean the coal would be marketable in any case, that is the Hughes vein.

2325. It is a fair and legitimate inference that a good deal of this spontaneous combustion has arisen from the people at the colliery mixing this 5-feet coal which you think dangerous along with the 4-feet and the 6-feet when there was great pressure to get coal?—Yes.

2326. They were of the same value, and therefore the owner without thinking of the risk of spontaneous combustion would think that he might as well have the ship filled up instead of detaining her by which he might be incurring 20*l.* a day demurrage?—That is very often the case no doubt.

2327. You think if pure steam coals were shipped at Cardiff in a proper state the risk of spontaneous combustion would be very small indeed?—There would be no risk at all.

2328. And the same with regard to coal shipped at Swansea?—Yes; I never heard of a cargo of steam coal taking fire at all.

2329. It is the smelting coal that you have been speaking of as being so liable to take fire?—Yes; I am shipping a great deal of steam coal, but I never heard of any of it taking fire.

2330. You hold that even the small coal which is shipped for smelting purposes would be safe enough if taken out of the 4-feet and the 6-feet?—Yes, there would be no danger. I would insure a ship directly that carried it.

2331. (*Sir George Elliot.*) Do you sell that smelting coal unscreened?—No, it is through and through.

2332. There is no small taken out?—No.

2333. You take care that you have nothing but the 4-feet and the 6-feet?—We will not take any coal from Swansea but the 4-feet and the 6-feet.

2334. Is the same care observed by other shippers as well as yourselves as to that?—There has been no care taken lately. When the demand for coal was so great people had to take what they could get, and I think that has been the cause of so many ships taking fire lately.

2335. You attribute the large percentage of cases of spontaneous combustion of late years first to the carelessness of the workmen in not cleaning the coals, and secondly, to the promiscuous mixing of the 4-feet, and 6 feet, and the 5-feet?—Yes.

2336. And I suppose you would say that the masters are culpable as well as the men in not taking care that the coal is wrought clean?—No doubt the masters ought not to have shipped coal from those veins where the coal was liable to ignite spontaneously.

2337. Have you considered whether it would be practicable to carry out the suggestion you have made of having the coal shipped in compartments?—I think it could be carried out.

2338. Take a 1,500 ton ship; how many compartments would you have?—900 tons would be the utmost that a ship would carry going to the west coast of South America.

2339. You are speaking of the west coast of South America trade?—Yes.

2340. What is your idea of the size of the compartments that would ensure more security?—I should think in a space the size of this room I would have four or five compartments.

2341. That is to say you would have quantities of 200 or 300 tons together?—Yes, I would put planks between the coal as it was put in.

2342. Supposing you had one or two partitions in a space the size of this room, would not the pressure be the same as you have now?—There would be a separation of the pressure just the same as in the case

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of a rick of hay. You may make a rick of hay into two or three compartments, but if you make a large rick, and if the hay is not good, it is sure to take fire; whereas if you have a small rick it is not so liable to ignite spontaneously.

2343. Shipping by boxes you would limit to merely filling the hatches with them; it is not practicable to adopt that system on a large scale where great despatch is needed is it?—For large coal I would not care to do it but it would pay the coal buyer to have it done that way because there is a great deal of breakage in dropping coal down 20 feet.

2344. Ventilation you do not attach much importance to except as a safeguard against explosion?—I would not object to it; I would prefer to ventilate the ship, though there is a difference of opinion about it; provided there was a large hole made in the cargo it would be a good thing.

2345. You have been a shipowner and sending coals to the west coast of South America for a long time?—Yes.

2346. How long is it since you arrived at the conclusion that the 4-feet and the 6-feet were the only two safe seams from which to ship coal?—It is notorious everywhere in the district of Swansea.

2347. That must have been the result of experience from other coals having been found unfit for a long voyage?—Several coals have taken fire when people have been speculating on coal from other seams. There is the Avon Vale, that is known to have taken fire. I would not ship a cargo of it. They shipped one or two cargoes, and they took fire, and I told them they would be sure to get them on fire.

2348. With regard to the Rhondda coal, there is a large quantity of Nos. 1, 2, and 3 shipped, you said that in your opinion all coal from those veins must fire?—That is from what I have heard, it is a good coal for cokeing and house purposes, but I do not think it would go on a long voyage if shipped through and through.

2349. (*Mr. Duncan.*) From what you have suggested in reference to separating coals into different parcels, I gather that you would consider it better to have a coal-carrying vessel built with two or three decks rather than having 24 feet depth of hold, the whole of the cargo lying on one bottom?—Yes, that would be as good as anything; 24 feet is too great a depth unless the coal is very clean and dry. I think what you suggest would be a very good arrangement.

The witness withdrew.

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2350. (*Chairman.*) You have been master of different merchant ships for some time?—Yes; for about 20 years.

2351. And you have had some experience in carrying coal?—Yes, I have been carrying coals 16 years in the Indian trade.

2352. Have you had any experience of cases of spontaneous combustion or explosion in coal-laden ships?—I have very little experience in that respect, it has only been in the last six or seven years that I have even seen any signs of heating.

2353. Within the last six or seven years have cases of heating come under your observation?—Yes.

2354. I think you had one very serious case in a ship you commanded?—Yes, the "Malabar."

2355. Is that the only ship you commanded in which such a casualty ever took place?—Yes.

2356. In what year was that?—In 1870.

2357. What coal was it?—It was fron Langley Mill coal-pit. We shipped it in London.

2358. You sailed from the port of London with that coal?—Yes.

2359. Do you know in what part of the country Langley Mill pit is?—I do not know. The coal was shipped from lighters which came alongside.

2360. You do not know whether it was north country coal?—No.

2361. Your ship was an iron ship?—Yes.

2362. Of 1,200 tons register?—Yes.

2363. And you carried under 1,300 tons of coal?—We had 1,274 tons on board.

2364. How many days were you out when the casualty happened?—About 36 days.

2365. Where were you bound for?—London to Batavia.

2366. Can you tell us anything about the circumstances of the casualty?—After having been out about 36 days, on one Saturday night, for about the first time all the while that we had been out, the hatches were put on. We generally had the hatches off in good weather all the way, but I told the mates that night to secure the hatches as I expected a change of weather, so they put the hatches on; and on Sunday morning about 5 o'clock the second mate took off the hatches, and he came and called me and told me that there was smoke coming up through the centre of the coals in the main hatchway. When I saw that, I altered the ship's course and put back for Rio Janeiro, and threw 50 tons overboard, and when I had put that 50 tons overboard the flames burst all over us. We set the pumps to work all that night and all the next day. We had two force-pumps and a little donkey engine, and we put the hose through under the 'tween decks; the 'tween decks were all burning, till at last the flames got halfway up the mainmast. Then we were obliged to close up the hatchways, and to put sails on the top of the hatches, and chains on the top of the sails to keep all close. That went on for two days, during which time I had the crew put in the stern. An American ship came up, and the crew said they would not stop any longer; they said that the flames might burst up at any moment. I wished them to stay that night, but they would not, so we went on board the American ship, and in about an hour after the hatchways blew right up and the mainmast went over the side.

2367. The ship was a total loss?—Yes.

2368. Do you know in what state that coal was taken on board?—I was informed afterwards by my mates that the coals were taken on board wet; that the lighters had been making water, and the coals were in a wet condition.

2369. How were they shipped from the lighters?—By baskets.

2370. Without any great fall?—They had no fall at all.

2371. Do you attribute the heating of that coal to its being shipped damp?—Yes, that is what I attribute it to.

2372. You do not know whether that particular description of coal was peculiarly combustible?—No. It was small bad coal that they were shipping at first, and I called the attention of my owner to the fact then; he made some remonstrance, and they seemed to be better after that.

2373. When you say they were bad you mean they were of inferior quality?—They were small.

2374. You had not it in your mind at the time that they were dangerous?—No.

2375. How was your ship ventilated?—Right along the keelson fore and aft, and then up and down the three hatchways.

2376. What was the size of the ventilating passage or chamber along the keelson?—About 18 inches square.

2377. Was it on both sides of the keelson?—It was put over the keelson on the keelson itself.

2378. Is that the only case you have personal knowledge of?—The only one. I have loaded coals since. I loaded coal in Dundee in another ship, and while they were loading it came on very wet and I stopped loading for two and a half days, I would not allow the coals to go in at the time it was raining.

2379. And you had no trouble in the case of that cargo?—No.

2380. You have had an experience of 20 years as master of a ship?—Yes, I have been taking coals out to the East Indies 16 or 17 years.

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2381. During all that experience the case you have mentioned is the only case of spontaneous ignition that you have had ?—Yes.

2382. What is your impression about ventilators, do you fancy it does any good ?—I fancy it does a great deal of good. In the case of the "Malabar" the ventilators were partly smashed when the coal was taken in. I cleaned the fore and aft ventilators, but the main one was not properly cleaned, or we would have observed the fire sooner.

2383. You are not one of those who look on ventilation as useless ?—No, I think it is very much required. I would put a platform underneath if I had my will in any ship taking coals on board, about 2 feet above the ceiling with holes in it, so that there might be a current of air underneath.

2384. In your 20 years' experience have you had to do with all kinds of coals ?—Yes, but they have been mostly Scotch coals shipped on the Clyde. I have taken them from Wales, but only to the West Indies and Mauritius, when they have been about two or three months on board.

2385. Notwithstanding they were all that time on board you did not find that they became heated ?—No.

2386. (*Sir George Elliot.*) You say you have been engaged for 16 or 17 years in carrying coals. For how many years have you adopted any system of ventilation in your ships ?—They never were ventilated till about 1867 I think.

2387. For about 8 or 10 years you were navigating vessels without ventilation ?—Yes.

2388. And you never had any casualty of any kind ?—No.

2389. Seeing that you went 8 or 10 years without any mishap, would not that lead one to infer that there was no necessity for ventilation ?—My opinion is that the cause of that unanimity from danger in former times was that the coals were put in bins, that is to say, large places at the coal pit, and they would lie perhaps 12 months exposed to the air before they were put on board ship, but now they take them out of the coal pits and put them on board ship directly, so that the gas does not have time to escape. The class of ships now conveying coal have been generally much increased in tonnage, consequently greater masses are carried in one bulk, formerly 500 to 1,000 tons may be taken as the extremes generally of foreign cargoes: they now range from 1,200 to 2,500 tons cargoes, and consequently require more care and ventilation through the greater mass of the cargoes.

2390. You mean that up to 1867 you used to take the coal on board your ship out of stock and not out of the pit ?—Yes.

2391. That would only be during a portion of the year; you would not always be taking out of stock, would you ?—I expect they were mostly taken out of stock in those days; they used to be in great heaps, and where those coals were lying any length of time exposed to the air the gas would escape.

2392. Did you ever know any Scotch coal to take fire by spontaneous heating ?—Yes; several of those ships that left Dundee with me last year caught fire.

2393. I mean previous to 1867 when the system of ventilation was not known ?—There was one in 1868, the "Duke of Portland" took fire; she loaded in Ardrossan, and the crew came into the Mauritius when I was there.

2394. Was she ventilated ?—No. That was the first one carrying Scotch coal that I remember.

2395. During the first 8 or 10 years of your experience you never ventilated ?—No.

2396. And you never had anything happen to you ?—No.

2397. Is that a condition that can be much improved on do you think ?—I do not know whether the different quality of coal may have anything to do with it, yet the greater bulk of the cargoes has I believe much to do with it.

2398. As to the "Malabar," which did take fire, the men lifted the coals by baskets from the barges and put them into the ship ?—Yes.

2399. When the men lifted them up to the hatchways did they drop them down ?—They capsized them down.

2400. Therefore they would fall 18 or 20 feet ?—They would at the commencement.

2401. And they were wet you say ?—Yes, the mate told me that the lighters were making a great deal of water.

2402. (*Chairman.*) Have you any general suggestion to make to the Commission on the subject of their inquiry ?—No.

2403. (*Mr. Duncan.*) With regard to the coals shipped in the "Malabar," you do not know from what district of county they came ?—No.

2404. What dock was she in when she loaded those coals ?—The Victoria dock.

2405. The coals did not come to the ship in trucks ?—No, in lighters.

2406. Do you know where the lighters loaded ?—No.

2407. Do you think they loaded close by the Victoria dock, just outside ? — I could not say where they loaded.

2408. (*Mr. Young.*) You have no personal knowledge of any cases of burning except this one ?—No.

2409. And you have carried a great many cargoes of coal ?—Yes.

2410. Mostly Scotch coal ?—Yes.

2411. Both from Glasgow and Dundee ?—On the first voyage I went with the "Malabar" I wished to get the ship ventilated. My owners said, "No, it was of no use to ventilate Scotch coal," I said I thought it was useful, so I got them to adopt the system of ventilation; it was on my third voyage bound to Batavia in her that this casualty happened. I went out and came back all right on the two first voyages, and I was taking a cargo of coals out on the third voyage when the fire took place.

2412. (*Mr. Icely.*) Speaking of your system of ventilation you described a trunk running along fore and aft near the keelson, was there any communication with the deck from the trunk ?—Yes, the up-and-down ventilators went into the horizontal trunk. The ventilators went down fore and aft.

2413. So that there were two currents of air, one coming to the foremost part of the ventilating trunk and the other to the after part ?—The horizontal ventilator went right fore and aft along the keelson; and then there were ventilators up and down in the three hatchways which went into this fore and aft ventilator.

2414. Those upright ventilators were uptakes for the gas ?—Yes.

2415. You had an in draught at the after part of that trunk that was laid horizontally and you had an in draught at the fore part ?—Yes.

2416. And those perpendicular or vertical trunks were the exits for the foul air ?—Yes.

2417. (*Mr. Fenwick.*) You said you were 36 days out when you first discovered indications of heat ?—Yes.

2418. Whereabouts were you then ?—In 29° south and 30° west.

2419. You say you had your hatches off most part of the time ?—Yes, till that night.

2420. You had ordered the hatches to be put on that night on account of the changeable weather ?—Yes, when you come outside 30° south you may expect gales of wind.

2421. (*Mr. Cory.*) Those particular coals which were shipped in the "Malabar" could not have been very fresh from the colliery, could they ?—I could not say.

2422. You loaded in London ?—Yes.

2423. There are no collieries in the immediate neighbourhood of London ?—No.

2424. The coal must have come some distance ?—Yes.

2425. It must have been shipped into the lighters in London ?—Yes.

2426. Therefore it could not have been very fresh

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from the colliery?—I have no information as to how long it was out of the coal pits, but it was taken on board in a wet condition.

2427. Those coals must have come some distance, while many of the other cargoes which you have carried safely would probably have come very quickly from the colliery?—They may have come quickly from the colliery, but they would have been put out in bins at the colliery and exposed to the air a long time before shipment.

2428. You look upon that exposure of the coal to the air at the colliery before shipment as an element of safety?—Yes.

2429. Have you any reason for coming to that conclusion?—There are so many coal ships that take fire nowadays when they do not keep the coal in bins at the collieries as they used to do before being shipped that I come to that conclusion; that is the only thing I go by.

2430. (*Chairman.*) You do not draw your conclusion from the case of the "Malabar," but from general observation?—Yes.

The witness withdrew.

Captain JAMES CAWKITT examined.

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2430*a*. (*Chairman.*) You are manager of the Salvage Association at Liverpool?—I am.

2431. And chief surveyor to the local underwriters?—Yes.

2432. How long have you held that position?—About 12 years.

2433. Previous to that you had been master of a ship in the East Indian trade?—In the East Indian trade, the China trade, and the Australian trade.

2434. In both capacities you have had some experience in connection with the shipment of coal?—My experience with regard to the shipment of coal since I left the sea has been chiefly confined to Liverpool and Birkenhead, but I have heard a great deal about the shipment of coal from other parts from time to time.

2435. You have studied the question from information that has reached you more than from your actual personal experience?—That is so.

2436. Have you arrived at any opinion with respect to the greater or less liability of particular kinds of coal to ignite spontaneously on board ship?—More particularly of late we have found that the number of cases of burnings has increased chiefly in the Lancashire coal, smelting coal going out to the west coast of South America.

2437. At Birkenhead you ship exclusively North Wales and Lancashire coal?—Yes.

2438. You do not ship any Derbyshire coal there?—No.

2439. Nor any Staffordshire? — There may be some, but it is chiefly North Wales and Lancashire.

2440. Have you noticed any increase in the casualties in the shipments of North Wales coal of late years?—Not so much of late years.

2441. But you have observed a considerable increase in the casualties in the Lancashire coal?—Yes.

2442. Could you describe to the Commission the particular seams or the particular denominations of coal which appear to you to be more liable to spontaneous combustion than others?—I do not think I could give you the seams. I think they have been chiefly Barnes' shipments of Lancashire coal and one or two others, which Mr. Rundell gives in his list. I could not tell you the particular seams or the pits. I can only say, speaking of shipments of Lancashire coal generally, that we have noticed that there have been more fires in them of late than in others.

2443. Do not the underwriters endeavour to trace home the particular description of coal in which the greatest risks seem to occur?—Yes.

2444. Have they not succeeded in that?—They have the statistics which Mr. Rundell has embodied in his pamphlet; many of his statistics are got up from our surveyors' reports, and they are recorded in the pamphlet which he has published.

2445. All you are able to say is that in coal raised from certain districts in Lancashire the risk seems to be much greater than it used to be?—Yes.

2446. How would you describe those districts?—I should call them the Lancashire coal districts.

2447. Is the result of that that the premiums charged by the underwriters on the shipment of Lancashire coal has risen?—Not alone on Lancashire coal.

2448. Is it higher than it was on Lancashire coal?—Yes.

2449. Much higher?—Yes.

2450. Have you been able to arrive at any opinion as to what the cause of the increased liability to fire may be?—My own opinion is that a great deal of it is attributable to the shortness of time that elapses between the time of the coal being wrought from the pits and its going into the ships, and perhaps from an inferior kind of coal being shipped.

2451. The shortness of time would cause the coal to be more gaseous?—The air would not have the same opportunity of percolating through it and permitting the gases to escape before the coal was put into the ship's hold?—No.

2452. That would point rather to explosion than to heating and spontaneous combustion?—This coal is of an explosive character. I do not profess to have a thorough scientific knowledge of coal; but the impression upon my mind is that the more coal is liable to spontaneous combustion the less it is liable to explosion, and the more liable it is to explosion the less liable it is to spontaneous combustion. I am not positively certain that that is so.

2453. Is it your judgment that it is rather in the direction of heating than in the direction of explosion that the recent increase of accidents has taken place?—I think so. In the whole of my experience I have only had one case of explosion, and that occurred at Birkenhead about six years ago. That was in the case of a vessel called the "Mary Amelia," which loaded South Wales steam coal from the Dyffryn steam colliery. Our surveyor had a note that she had loaded a cargo of South Wales coal. There was no ventilation. They closed the hatches on the Saturday, and on the Monday we heard that an explosion had taken place at Birkenhead, and it turned out that it was in this vessel. It appears that the hatches having been closed from Saturday night, the ship keeper went on board on the Monday morning, and went into the store-room with a naked light, and the whole deck blew off. The man was badly injured, and a paper was found in his pocket warning him against the explosive character of this coal, and telling him not to take a naked light down below. That has been the only case of explosion in the neighbourhood of Liverpool that has occurred in my experience.

2454. With reference to heating and spontaneous combustion, have you formed any opinion as to the desirability or otherwise of ventilation?—I am strongly in favour of ventilation, that is to say, ventilation carried out thoroughly and carefully.

2455. Will you describe what system of ventilation you would recommend?—If we could ventilate the whole cargo in a body, so as to admit air through the whole of it, I do not think spontaneous combustion could arise, but perhaps it might be impossible to devise any system of ventilation to do that.

2456. Even though the coal should be small and wet?—I should think if the coal was shipped damp, and it was small coal and in a confined space, firedamp must follow. That would be a very dangerous state of things I think.

2457. In any case you would be for the greatest amount of ventilation possible?—Yes; as a prevention.

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2458. What exact process of ventilation would you recommend?—What I should recommend in the first place would be a platform perforated with small holes, laid under the cargo, so as to raise the cargo and allow a current of air underneath it, and then ventilating shafts up each hatchway, communicating for and aft with the top of this platform.

2459. How high should the platform be above the keelson?—In a wooden ship it need not be as high as the keelson, but I should say it might be about 18 inches from the ceiling to the top of the platform.

2460. Carried the whole way fore and aft?—Yes; as far as the coals were carried. Those ventilators should be carried up above the deck with those bell-mouthed tubes upon them, so that they could be turned to the wind to enable a current of air to go down under the platform.

2461. Irrespective of ventilation, have you any suggestion to make with regard to precautions which are now not taken, and which you think ought to be taken?—The great point is the shipping of the coal in a good dry condition, and every care being taken that air is admitted into the hold by every possible means. I think it is a most important point that the coal should be shipped dry.

2462. (*Sir George Elliot.*) How long have you had the superintendence of the shipment of coals at Liverpool and Birkenhead?—I have not the special superintendence of the shipment of coal more than any other cargoes, but in the course of our duties we note how coal ships are loaded for the information of the underwriters.

2463. You are employed by the underwriters?—I am chief surveyor to the Liverpool underwriters, but we are daily in communication with gentlemen who superintend specially the loading of coal for the Peninsular and Oriental Company, the East Indian railways, and so on.

2464. How long have you held that office?—12 or 14 years.

2465. I suppose in your experience there has been a much larger number of cases of fire in coal-laden ships these last two or three years than there had been previously?—There can be no question about that.

2466. Has there been less ventilation the last two or three years?—I think the ventilation has been about similar.

2467. It is not due to any change in respect of ventilation?—No.

2468. What strikes you as the reason for these increased number of casualties?—As I said before, I think the shipment of small coal has a great deal to do with it. It has not been allowed to accumulate at the pits, and then it goes fresh from the pits to the ship. Of late that has been particularly so in consequence of labour strikes, and so forth, and in consequence also of the great demand for coal, and the small quantity ready to ship. That has been particularly so with regard to this smelting coal. I think very inferior qualities and small coal have been shipped.

2469. Have the coals been shipped with more despatch the last two or three years than previous to the last two or three years?—I think they have.

2470. What makes you think so?—Perhaps last year they were not so quickly shipped, but before that there was a very great demand.

2471. Was there not a great difficulty to get coals last year?—Last year there was.

2472. And consequently were not they shipped more rapidly?—The individual ships that loaded would receive the cargoes more rapidly; there were a great number of ships waiting for coal at the time.

2473. Are you aware that no more coal was raised in 1873 and 1874 than previously?—I am not aware of that.

2474. You regard the rapidity with which, during the last two or three years, coals were brought from the pit to the ship as compared with previous times, as one of the causes of the increased number of casualties?—Yes.

2475. Another cause in your opinion is that the coal has been smaller than it was three or four years ago?—I think the Lancashire coal has been smaller.

2476. There has been no change in the ventilation?—No, I think not. We believe in ventilation, and we have tried to get it carried out to the greatest extent of our recommendations.

2477. You said that Barnes' coal was more especially liable to take fire spontaneously?—He is one of the shippers to the West Coast. Several of his ships have been burnt with that description of coal in them.

2478. That is from Lancashire?—Yes.

2479. You cannot specify any particular district?—No; I do not know what mines they came from.

2480. (*Mr. Duncan.*) Did you carry coals yourself when you commanded a ship?—No.

2481. How many years back does your experience go in conexion with the shipping of coals?—12 or 14 years.

2482. Do you think there was as much ventilation 12 or 14 years ago as there is now?—No; I think there is more ventilation now.

2483. Seeing that there is more ventilation now than formerly, might not it be that ventilation does harm rather than good; has that ever occurred to you?—No; I think the number of long voyage coal ships to the east has been increasing of late, and that would account for an increased number of casualties.

2484. If you were told that scientific men who have analysed coals have found that they contain sulphur and pyrites and have ascertained that by allowing a current of air to pass over those constituents the oxygen of the air very likely tend to set the coal on fire, you would probably question the desirability of passing great quantities of air over coal when carried on board ship?—If a fire took place no doubt ventilation would tend to aggravate it, but as a preventitive we believe in ventilation, and further we think that it leads to other precautionary measures. I do not think sufficient care is taken by the captains and officers of ships that load coal. I think they are too careless about it. They do not take the danger of the cargo they are taking on board fully into consideration.

2485. In what way?—I mean in taking precautions. For instance, there are very few coal ships that go away with a thermometer to test the temperature of the coal.

2486. That would not prevent the firing of the coal?—No; but it might forewarn them of danger.

2487. You mean by their not taking care they are not careful to keep their hatchways covered in rainy weather and things of that sort?—I think the principal precautions are required when they are loading the cargo and they ought to be continued throughout the voyage.

2488. It has been stated before this Commission that there are cases where the admission of air would positively tend to put the coals on fire; that allowing a current of air to pass through them would supply to the pyrites contained in the coals the very thing to create spontaneous combustion; has that ever occurred to you?—That has occurred to me; but that is a question for scientific men to deal with.

2489. Further than your own practical experience you have no data upon which to ground your judgment in reference to ventilation being very desirable?—No.

2490. You probably consider that coals emit gases, which gases are inflammable, and that it is therefore desirable to get them out of the ship as quickly as possible. Is that the principle on which you think ventilation is desirable?—The principle on which I think ventilation desirable is that it is desirable to get a current of air through the coal; gas usually accumulates on the upper surface; if the gas accumulates on the upper surface that is a different condition of things to spontaneous combustion; that occurs from firedamp in the ship hold.

2491. (*Mr. Abel.*) You state that coals in which explosions were likely to occur would be less liable to

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spontaneous combustion than other coals; have you any facts to support that opinion?—No; it is merely my own opinion and the result of conversation with others. There have been very few cases of spontaneous combustion in shipments of coal from South Wales ports where they ship steam coal of the best character.

2492. Is that coal liable to explosion on board ship?—Yes; I believe the south Wales coal is.

2493. That is only an opinion?—It is only my own opinion.

2494. You also stated that coal coming fresh from the pits was more liable to spontaneous heating than coal which had been exposed, and you thought that the increase in the number of casualties was partly due to that. Is that based upon any facts that you can give the Commission?—That is only a general opinion I have formed.

2495. You also stated that small coal had been allowed to accumulate at the pits and that this had been shipped fresh from the pits to the ships, and you thought that that was one cause of the spontaneous heating. I do not quite understand that?—The small coal would be more liable to generate heat and damp than large coal. Large coal stowed in the vessel's hold would admit air amongst it.

2496. What you really meant was, that the increased number of casualties might be due to shipping a larger proportion of small coal which had been allowed to accumulate. If that small coal was damp it would be more liable to spontaneous heating?—Yes.

2497. You ascribe the increased number of casualties rather to the larger proportion of small coal shipped than to the shipping of it fresh from the pit?—I think it is due to both causes.

2498. Have any facts come within your knowledge which have led you to that conclusion, or is it merely an opinion?—Most of the burnings in the ships we have heard of have been under the main hatch, and the bulk of the burnings have been in vessels that have been loaded under the tips where the coal is broken, and where there has been a lot of small coal accumulating round the main hatchway, and I believe in most instances the fire has been proved to have originated about the main hatchway.

2499. Those casualties have, in your opinion, been caused by the production of small coal by the method of shipping, and not by the shipping of a larger proportion of small coal as such?—That is so to a certain extent, but the action of damp in the case of small coal would be more injurious than in the case of large coal in a confined space.

2500. (*Mr. Young.*) You are practically employed as professional assessor to the underwriters at Liverpool, and you go round to look at the ships for the purpose of seeing that the ships are in good order, and that the cargoes are properly loaded?—Our staff of surveyors do, and I used to do so till lately. I am more in the office now.

2501. Coals of course only come along with other things under your observation?—Yes.

2502. You have not paid any particular attention to coal cargoes?—Yes, as regards ventilation, storage, quality, and quantity shipped by each vessel.

2503. Has your experience led you to warn underwriters against certain classes of coal as being liable to spontaneous ignition when shipped on board vessels?—All that we have been in the habit of doing till lately has been to report that a vessel is loading with a cargo of North Wales coal, South Wales coal, or Lancashire coal, and whether by hand or by tips, without reference to the colliery; but of late, since these burnings have been so frequent, we have been taking notice of the colliery from which the coals have come.

2504. Have you found a marked increase in the number of cases of spontaneous combustion in shipments of coal from Birkenhead within the last two years?—There was a great immunity from fire in coal shipped at Birkenhead till about three years ago. We had scarcely a case of spontaneous combustion in shipments from Birkenhead till three years ago.

2505. Has there been a great increase also in the number of such cases in ships that have gone from Liverpool?—Yes.

2506. Has the increase been greater than in the case of Birkenhead?—Yes.

2507. Your advice to the underwriters would be to avoid insuring ships shipping coal from such and such collieries?—They would notice the facts given in our reports and act upon them.

2508. Has it come within your observation that the class of ships that carry coals from either of those two places has deteriorated during the last few years?—We think they have rather improved.

2509. They are mostly iron ships?—Yes, and large fine American ships.

2510. You do not find so many old-fashioned colonial-built vessels of a low class?—No, not so many.

2511. Do you find that ship owners as a general rule take great pains in the way of seeing how the coals are shipped themselves?—Yes.

2512. Irrespective of you and your surveyors?—I think so. I think every disposition is shown to carry out the recommendations of the surveyors.

2513. They make you welcome on board?—Yes.

2514. (*Mr. Icely.*) Is it one of your requirements that the temperature of the cargo should be taken?—Our surveyors who visit the ship ask the captain before the vessel loads what mode of ventilation is adopted, what coal he is taking, the quantity, and so forth. One of the questions we put is whether he is taking a thermometer with him to test the temperature, if he says, No, we recommend him to do so, and we advise his recording the temperature in the log book each day.

2515. Has it been your custom to ask those questions and to make those recommendations for a long period or a short one?—It has been our custom for several years back. We cannot insist upon their doing what we recommend. We merely advise and suggest.

2516. Within your experience for how long a period has it been considered necessary to ventilate ships?—I think they have been ventilated now as far back as I can recollect, but the mode of ventilation I think has been rather improved.

2517. A system of ventilation of some sort or other has been adopted for a great number of years?—Yes, I think so.

2518. (*Mr. Fenwick.*) You are decidedly in favour of ventilation yourself, and the Underwriters' Association at Liverpool adopt your views in regard to ventilation? — Our mode of ventilation has been adopted by them, and it is understood that most of the ships which they insure are ventilated in that way.

2519. (*Chairman.*) Was coal treated as a specially hazardous cargo till lately?—Formerly I do not think it was.

2520. Was the premium on ships carrying coal higher than upon ships carrying iron?—No.

2521. Now it is very much higher?—Yes.

2522. That is the result of the impression produced on the minds of the underwriters by the casualties in the last few years?—Yes.

2523. Is there any difference in the premium charged on ships going from Liverpool with Lancashire coal and that charged on ships going from Birkenhead with North Wales coal?—No. I think the premiums are generally equal; they have all risen.

2524. Though the risk may really be very different?—Yes.

2525. There is a sort of panic among the underwriters just at present?—There is, in consequence of the frequent burnings of late.

2526. Are the underwriters kept fully informed of all facts tending to show the comparative liability of different descriptions of coal to take fire spontaneously?—The underwriters have Mr. Rundell's statistics before them, and they are guided by those facts in taking risks. We state that a vessel is loading

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a cargo of Lancashire coal, and we may state the colliery from which the coal comes. The underwriters are now thoroughly posted in the character of the coal from the different collieries. Mr. Rundell's statistics give the number of burnings, and also the mine that the coal comes from, and the underwriters are posted up in those facts accordingly.

2527. I gather that you merely report what coal it is that is being shipped, and then the underwriters have Mr. Rundell's statistics before them, by which they are guided in fixing the premium they charge according to the denomination of the coal ?—That is so. I believe the premiums are equal all over. I do not think there is any great difference between the premium in the case of one coal and another.

2528. If the premiums are equal all over, and if it has come out before us that there is a very remarkable distinction in the liability to ignition spontaneously between different kinds of coal, one would infer that the underwriters are imperfectly informed with regard to the various characters of the different descriptions of coal ?—I do not think that. I think each underwriter will take his own view. I could not speak with regard to the premiums. I think the underwriters as a body are fully informed as to the dangerous qualities of particular descriptions of coal. Each underwriter, I presume, will act on his own judgment.

2529. (*Mr. Young.*) You say the premiums remain very much the same. Is not it the case that Liverpool underwriters do not underwrite such cargoes as you, from your reports, show to be dangerous, and that those cargoes are not taken in Liverpool at all ? —That rests with themselves. We merely report the facts for them to act on at their pleasure.

2530. They judge of them themselves ?—Yes.

2531–2. I believe that practically they refuse to underwrite risks that you report unfavourably on ?—Yes, I should think so ; some might and some might not.

The witness withdrew.

Captain W. Kennedy.

Captain WILLIAM KENNEDY examined.

2533. (*Chairman.*) You have commanded ships in the merchant service for some years ?—Yes.

2534. You have had considerable experience in carrying coal ?—Yes, a good deal.

2535. About how many voyages have you made with coal ?—10 voyages.

2536. Within how many years ?—18 years.

2537. Have you ever had a case of spontaneous combustion in any of your ships ?—Once.

2538. Will you tell us the particulars of that case ? —My first voyage with coal was in a ship of 1,100 tons, called the "Wallace." I loaded her with coals in Cardiff at the beginning of March 1852 for California. We had to call at Callao for orders, and then we went to San Francisco. The coal was on board for about seven months from the time we loaded till we began to discharge, and though we had no ventilation there were no signs of heating in the ship during the voyage. On the next voyage, in the same ship, I loaded in London, out of barges, and went to Coquimbo, and arrived all safe. On that voyage also she was not ventilated. Then I went out twice with coal to Hong Kong in another ship, a ship called the "India," and she went safe.

2539. Was she ventilated ?—No. The "India" loaded coals in the Clyde, and went to Hong Kong all safe. She was not ventilated, she was only a 550 tons ship ; on both voyages there were no signs of heat at all. I next went out in the "L'Aqouhanna." On her first three voyages we were ventilated, only we had no ventilation along the keelson. It was the stevedore's idea that the large coal might be laid in a rough way fore-and-aft along the keelson and above the keelson about two feet. He did that himself, and when the coal was about 8 feet high we had two ventilators right fore and aft, about 15 inches square, and we had uprights in the fore hatch, the main hatch, and the after hatch ; then we had cross ventilators from those into the uprights, and the ends of those fore and aft ventilators were clear from the coal ; they were away 2 or 3 feet from the coal. That was the system adopted for three voyages. The coals, in the case of those three voyages, were loaded in Birkenhead by barrow, the barrow was tipped wherever it was required without any fall at all ; and then when they came to get within 4 feet from the beams, and they could not work under the beams, they laid a plank on the top of the beams and wheeled the barrows along that way, and then there was only a 4-feet drop. On the last voyage the agent and part owner was in a great hurry, and he would have us go under the tip in Birkenhead. The tip lifts a truck about 15 or 20 feet above the ship's deck, the coal runs in an iron shoot, and by the time it gets into the hold it is smashed up very much, and a large body of small coal always lies where it falls, for the trimmers never think of trimming that, it is only the round coal that they trim. When I saw this I objected to it, and I said "I do not like this small coal accumulating here, and we ought to be careful now that we hear of so many ships taking fire." The agent said "Do you think the ship will be lost through it ?" I said "I would not go so far as to say that, but still I did not like it." We had an extra ventilator alongside the keelson. We had pieces of wood in this way ∧ on each side of the keelson, the lower board was about 3 inches off the skin. Then there would be a ventilator down each hatchway entering into the fore and aft ventilator in the ship's bottom. On this voyage, on account of so much small coal that went in with the tip, I had two tiers of ventilators, one under the beams, and another tier above the beams, there being ventilators into the uprights in each hatch from each of those tiers of ventilators. I could not say exactly the date we left Liverpool, I think it was the 10th or the 15th of October that we left. We found no heat till we got within 500 miles of Bombay. The first indication we had of the heat was seeing the paint beginning to get black, and there was a smell as if coal-tar had been capsized, though there was no such thing in the ship at all. We got the fire hose to work and threw in water, and pulled to in the best way we could. After we had got 2 feet of water in the ship, we got the hose attached to the main pumps and pumped the water out, and then put water down the hold again so as to saturate the coal, but it was all of no avail. After about 50 hours of it we were at last obliged to put the boats out, at about 4 in the morning. We got them victualled and watered, and by the time they were ready it was time to go out.

2540. The ship was a total loss ? — Yes. We were not a mile away from her when the flames burst up the after hatch and the main hatch, and 10 minutes after the mainmast fell carrying with it the mizenmast.

2541. What coal was that ?—I could not say. Our agents could tell probably. It was called North Wales coal.

2542. Considering your immunity from accidents of this kind in your previous voyages, to what do you attribute the casualty which you met with on that occasion ?—To the coal being dropped down from such a height and its being smashed up so that there was so much small coal lying under the main hatchway.

2543. The accumulation of so much small coal produced an unfavourable impression on your mind at the time ; you did not expect to lose your ship, but still you thought it was dangerous for the small coal to be lying there in such a mass ?—Yes.

2544. Do you think that the thorough ventilation which you seem to have exceptionally given to this ship increased the danger, or do you think that it diminished it ?—When the fire did take place it was worse ; it acted as a fan to blow it up.

2545. Do you think it tended to diminish the risk of taking fire originally ?—I doubt it, because I have

Captain W. Kennedy.

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often carried coals without ventilation at all and no accident has happened with them.

2546. Do you take it to be the bent of your mind from experience that ventilation does more harm than good ?—I do not think it does much good.

2547. What do your owners say about ventilation ?—When I came home one of my owners, Mr. William Ross, of Glasgow, was quite astonished that I did not turn to and bore holes in the ship and let the water run into her in that way, and then pump it out. I said any person while the ship was burning could go down and bore holes, but after the water came to the fire we should get no one to go down to plug the hole.

2548. Do you know what the opinion of your owners is on the question of the ventilation of a coal ship ?—My owners are of opinion that ventilation is beneficial.

2549. What are their instructions ?—It is left to my own judgment. Messrs. Robert Young and Company, of Glasgow, were the principal owners of the ship, and they left it to me to do everything I thought was necessary with the advice of any older hands in the coal trade.

2550. Do you consider it to be the general opinion of those of your experience that in a trade like your own ventilation is beneficial ?—There are a great many that I have known who have had their coal on fire, and whose ships were well ventilated, who have said that they did not think ventilation was of any use.

2551. They would push the hatches off as often as possible, so that any gas might escape ?—We never had the hatches on except when it was raining. Salt water will not make the coal take fire ; it is the fresh water that heats the coal.

2552. When you speak of ventilation, you mean ventilation through the coal ?—Yes.

2553. You do not mean ventilation on the surface ?—I believe it is of great benefit on the surface, because whatever gas there may be it will evaporate through the small openings in the coal.

2554. I take it to be the general effect of your evidence that you doubt the efficacy of ventilation ?—I doubt the practical worth of ventilation at all. The last voyage I took with coals was in the "America" of Glasgow, 1440 tons register, built of iron. We sailed from the Clyde 11th April 1873 with 2000 tons of steam coal, and landed the same at Coconada, without any signs of heating during the whole voyage. No ventilators were used on this voyage ; the owner, John H. Watt, Esq., did not approve of them.

2555. You are strongly of opinion that the lower the fall of the coal when it is put into the ship the better ?—Certainly, you get the coal rounder.

2556. It is your opinion that a large accumulation of small coal in the ship's hold particularly under the hatchways is very dangerous ?—Yes, because on one of my voyages when I got to Bombay there were four ships on fire, they had all got in in time to get assistance and they got the steam pumps from shore to put out the fire, and it was in the after part of the main hatchway that those ships were burning.

2557. Did they load under the tip ?—Yes, those four ships were all from Birkenhead.

2558. (*Mr. Duncan.*) What was the name of those coals which you loaded on this voyage when they took fire ?—I cannot say. They were North Wales steam coal.

2559. Were they as round coals, generally speaking, as you had been accustomed to load from Birkenhead ?—Something the same.

2560. As a rule the coals from Birkenhead are large coals ?—Yes, some of them are very large.

2561. This cargo was not quite so large ?—No, and the coals being so small and having to fall a great height there was a great accumulation of small coal round the hatchways.

2562. Do you think it possible that the coals might have come from East Cheshire and come round to Birkenhead to be shipped ?—They told me that they were North Wales coal.

2563. (*Mr. Young.*) Probably what you would do in future would be to adopt the plan you referred to as having been so successful on board one or two of your ships, that is to say putting the largest pieces of coal all along the keelson, and so creating an internal ventilation ?—That is the way it was done for three voyages.

2564. And it was most successful ?—Yes.

2565. You had no wooden ventilators at all in that case ?—Not to the keelson ; 7 feet above the keelson there were wooden ventilators right fore and aft.

2566. Probably you will continue to adopt that plan in future ?—It was through the advice of older hands in the coal trade that I adopted it, and I may mention that I never found any heat at the bottom of those ventilators.

2567. Did you put the thermometer down from time to time ?—Yes.

2568. Did you register the temperature in the log book ?—Yes ; the greatest heat I have ever felt in all the coal carrying in my experience has been just at the hatchway. You will always feel the heat there, but if you go down you will feel no heat.

2569. Do you mean that you will feel the heat whether the cargo is in a state of spontaneous combustion or not ?—I have always experienced heat at the hatchway even when I have carried the cargo safely and well, and it is the same with a cotton ship. The heat has been so great at the hatchway that you would think the ship was on fire.

2570. It is not peculiar to coal ?—No.

2571. (*Mr. Icely.*) Was this ship when she took fire ventilated in the same way as she had been when she went safely with her cargo ?—She was better ventilated according to the opinion of older hands in the trade than myself ; we had this roofing of wood over the keelson and about 10 inches above the keelson there was a free passage of 3 feet on each side of the keelson.

2572. Did the ends of that horizontal shaft extend beyond the coal on each side ?—They could not go exactly beyond the coal because of the shape of the ship, but round knobs of coal were placed at the ends ; I paid attention to that ; all the large coal was placed round them.

2573. Where was the air admitted to that horizontal shaft ?—From the main hatch and the fore hatch ; upright ventilators went down into this shaft.

2574. Did the upright ventilators have a direct communication fore and aft with the shaft ?—Yes.

2575. (*Mr. Cory.*) I understood you to say that you thought wet would tend to promote heat in a coal cargo ?—Fresh water.

2576. But not salt water ?—No.

2577. So that you would not close the hatches to prevent a little salt water going down ?—No ; unless there is heavy weather we never think of putting the hatches on. If I am not wrong I think the Peninsular and Oriental Company tried an experiment with salt water at Bombay ; they got water carried up by the natives and they saturated the coal with salt water ; they put no ventilators in the stock and there never was the least heat out of the coals at all, but they are very much afraid of their taking fire with fresh water, and I believe they have taken fire on shore at times.

2578. (*Chairman.*) Have you any suggestion to make to the Commission on the subject referred to them ?—I only speak from what I know by experience. I may say that if cotton gets wet with salt water it will heat and be apt to set the ship on fire, but fresh water will not heat it. I was at Bombay when two ships were coming home with cotton bales ; while they were loading the cotton was exposed to heavy rain ; that did no harm, but afterwards when the cotton had got saturated with sea water both those ships took fire.

2579. (*Mr. Young.*) Was there any oil or seed on board either of those ships ?—No ; oil is a bad thing to have on board. I never allow a drop of oil below because there is nothing worse than oil.

The witness withdrew.

Adjourned.

Tuesday, 16th November 1875.

THE RIGHT HON. HUGH C. E. CHILDERS, M.P., IN THE CHAIR.

PRESENT:

FREDERICK AUGUSTUS ABEL, Esq., F.R.S.
RICHARD CORY, Esq.
GEORGE DUNCAN, Esq.
JOHN FENWICK, Esq.
JOHN FERGUSON, Esq.
CHARLES ICELY, Esq.
WILLIAM YOUNG, Esq.
J. M. CARMICHAEL, Esq., Secretary.

Mr. ARTHUR OWEN DAVIS examined.

Mr. A. O. Davis.

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2580. (*Chairman.*) You attend from the Peninsular and Oriental Company to give us what information the company have as regards spontaneous combustion of coal shipped for their service abroad?—Yes.

2581. Will you tell us in the first instance as to the year 1874, what were the total shipments of different kinds of coal?—In 1874 we shipped by 213 colliers 287,003 tons of coal to various foreign ports. To Alexandria we shipped 6,151 tons, which was carried by nine colliers. To Aden we shipped 60,755 tons by 36 colliers.

2582. Through the Canal?—No, round the Cape.

2583. (*Mr. Duncan.*) Those are coal-laden ships properly speaking?—Yes. To Brindisi we sent 28,862 tons by 27 ships; to Bombay we sent 38,164 tons by 24 ships; to Calcutta we shipped 6,794 tons by 4 ships; to Hong Kong we shipped 19,073 tons by 11 ships; to Malta we shipped 50,932 tons by 47 ships; to King George's Sound we shipped 15,096 tons by 8 ships; to Point de Galle we shipped 28,802 tons by 25 ships; to Shanghai we shipped 4,106 tons by 3 ships; to Singapore we shipped 18,834 tons by 13 ships; to Suez we shipped 4,217 tons by 3 ships.

2584. (*Chairman.*) Did those go through the Canal?—No, round the Cape for ports to the eastward of Suez. To Yokohama we shipped 5,217 tons by three ships.

2585. Can you give the class of coal in each case?—I have not the respective quantities of each description of coal in every case, but I can give the descriptions of coal conveyed to the principal ports. To Alexandria we shipped of Bebside West Hartley (that is Newcastle coal) 1,363 tons, and to the same port of Insole's Merthyr (which is a Cardiff coal) 2,301 tons. To Aden of Bebside we shipped 4,019 tons, of Howard's West Hartley 8,917 tons, of Battersby's North Wales Main 14,316 tons, of Scott's Brymbo (a North Wales coal) 14,937 tons, of Powell Duffryn (a Cardiff coal) 5,992 tons, of Insoles Merthyr 7,136 tons. Then to Brindisi we shipped 19,011 of Powell Duffryn and 6,474 of Buddles' West Hartley (a Newcastle coal); to Bombay we shipped 10,732 tons of Scott's Brymbo, 11,416 tons of Battersby's North Wales Main, and 3,785 tons of Insoles' Merthyr; to Calcutta we shipped 2,186 tons of Powell Duffryn and 1,804 tons of Insoles; to Hong Kong we shipped 3,261 tons of Battersby's North Wales Main, 3,146 tons of Scott's Brymbo, and 5,079 of Powell Duffryn; to Malta we shipped 33,044 tons of Powell Duffryn, and 2,927 tons of Davis's Maestag; to King George's Sound we shipped 5,895 tons of Battersby's, 3,750 tons of Powell Duffryn, and 3,846 tons of Scott's Brymbo; to Point de Galle we shipped 12,822 tons of Insoles' Merthyr, 3,657 tons of West Hartley Main, 4,243 tons of Howard's West Hartley, and 6,005 tons of Scott's Brymbo; to Shanghai we shipped 2,966 tons of Powell Duffryn, and 1,140 tons of Insoles' Merthyr; to Singapore we shipped 12,837 tons of Insoles' Merthyr, and 1,775 tons of Scott's Brymbo; to Suez we shipped 2,860 tons of Powell Duffryn, and 1,357 of West Hartley Main; to Yokohama we shipped 3,345 tons of Powell Duffryn, and 1,872 tons of Insoles' Merthyr.

2586. Will you tell us what casualties you have had, and with what class of coal in each case?—During the year there were three ships which loaded Howard's West Hartley (a Newcastle coal) for Aden. Those three were burnt.

2587. Have you the names of the ships?—The "Euxine" (an iron ship) the "Oliver Cromwell," and the "Calcutta." The "Euxine" had been a Peninsular and Oriental steamer, and after we sold her she was converted into a collier.

2588. (*Mr. Cory.*) At the time she was burnt she was a sailing ship?—Yes.

2589. (*Mr. Duncan.*) Can you tell us in what month of the year those vessels sailed?—The "Euxine" sailed on the 12th of June 1874. The "Oliver Cromwell" sailed on June the 18th, and the "Calcutta" on June the 12th. Then of the ships carrying coal to Bombay there is one missing, the "Nora Græme." She sailed from Liverpool.

2590. (*Chairman.*) In what month did she sail?—She sailed on the 23rd of September.

2591. (*Mr. Icely.*) Have you the description of coal she was laden with?—Scott's Brymbo.

2592. (*Mr. Duncan.*) You have no evidence that she was burnt?—No, we only know that she was lost; at least the underwriters have paid for a total loss. Then in the case of the ship "Ontario," which went with coals to Bombay; her coals took fire when she was just outside Bombay. The Government sent a tug out to bring her into the harbour and it was found that about 100 tons of her coal had been burnt or damaged. At all events we have claimed for 100 tons so damaged. The tug towed her into Bombay.

2593. (*Mr. Icely.*) When did she sail?—I cannot give you the date of her sailing.

(*Mr. Ferguson.*) She sailed on the 6th August 1874.

2594. (*Chairman.*) You may say there were four cases of actual combustion, and one of a ship missing, cause unknown?—Yes.

2595. Do you know anything about the state of the weather when the coal was shipped in any of those cases?—Generally more or less wet. It frequently happens that between the time when the coal leaves the pits and the time it is put on board some of it gets wet.

2596. Do you happen to know how the coal was put on board those ships, whether by tip or barrow? At Birkenhead the coal is invariably loaded by barrows into our colliers.

2597. (*Mr. Duncan.*) When the coal is being shipped have you any one representing you to see that no coal containing pyrites or shale is shipped?—Yes; we have an inspector at the ports of Newcastle, Birkenhead, and Cardiff.

2598. (*Chairman.*) Was the "Nora Græme" loaded at Birkenhead?—Yes.

2599. Where were the "Euxine," the "Oliver Cromwell," and the "Calcutta" loaded?—At Newcastle.

2600. How were they loaded?—I think they were loaded from tips, but I will not be certain. They were all loaded in the Northumberland Dock, and I believe from tips.

2601. Does the Peninsular and Oriental Company impose any conditions as to ventilation?—No.

2602. It is left to the shipowners to ventilate as they please?—The underwriters stipulate in some cases that the keelson shall be ventilated with vertical shafts or with Venetian ventilators to hatches.

2603. But in the tenders the company itself does not make any condition?—No; in fact our inspector at Newcastle, Captain Steinson, who has had a good many years experience, and is a thoroughly practical man, is quite against the system of ventilation from underneath.

2604. You have given us your statistics for the year 1874, have you any similar statistics for previous

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years ?—I have drawn up a memorandum which I will read :—

"Memorandum relative to spontaneous Combustion of Coal on Shore and in Bunkers of the Company's Steamers :

"Cases of spontaneous combustion in the company's coal stacks have taken place, but taking into consideration the heavy stocks kept up, and the extremes of heat and moisture to which they are exposed, such instances are very rare. About 10 years ago, a case of firing occurred near the bottom of a large stack of coal at Calcutta, when about a thousand tons of coals had to be removed ; it was then discovered that for a space of about 6 feet square the coal was in a very heated and charred condition. At Aden, in July last, two cases of heating occurred, one in the heart of a stack of North Wales coal, and the other near the surface of a heap of Bebside Hartley (Newcastle) coal. In the course of the last 10 years three or four cases of explosion in the bunkers of the company's steamers have taken place, owing to the *close* covers of the bunkers having been kept on too long ; it is, however, one of the company's standing orders to their chief engineers, that the close covers of the bunkers must only be put on when the decks are being washed, or in very bad weather ; *grated* covers being used on all other occasions, so as to admit of the escape of the gas generated in the coals—(for the same reason it would be well, were the hatches of colliers lifted on all occasions where practicable). In the company's experience cargoes of the South Wales coal are much less liable to spontaneous combustion than either Newcastle or North Wales coal. The arrangements for stacking the company's coal are as follows :

" Malta	In sheds, in the open, and in barges, ready for shipment.
" Brindisi	In the open.
" Aden	Do. do.
" Bombay	Under sheds.
" Calcutta	Do. do.
" Galle	In sheds and in the open.
" Singapore	In sheds.
" Hong Kong	Do.
" Shanghai	Do.
" Yokahama	In floating hulk and in sheds on shore.
" King G. Sound	In floating hulks and in the open on shore."

2605. It would appear that in some places the coal is stacked in the open, while in others it is under cover, the climate in each case being very nearly the same. Is there any particular reason why coal should be stacked differently in the one case to what it is in the other ?—At Aden the land on which it is stacked belongs to the Government, our tenure of the land is very uncertain, and therefore we have not erected any sheds there for coal ; but at Bombay, and Calcutta, and Shanghai, where we have our own property, where the land is either freehold or leasehold, we have run up sheds.

2606. The company do put the coal under cover where they can ?—Yes ; particularly at places like Bombay and Calcutta where the monsoon rains are very heavy.

2607. Can you give us any information for the years before 1874 similar to what you have given us for 1874 as to the number of casualties at sea ?—I have not such statistics here.

2608. Could you furnish us with a table giving the amount of coal shipped to places beyond the equator, the number of casualties, and the description of coal in the carriage of which the casualty occurred, say for a few years past ?—I will do so. I know that 1874 shows a far larger percentage of cases of spontaneous combustion and casualty than any previous year.

2609. (*Mr. Fenwick.*) Are you able to assign any cause for there being so many cases in 1874 as compared with other years ?—I do not know the reason of it. The fact has caused a considerable amount of attention. Some people say the coal is not so well cleaned as it used to be, and so a greater amount of pyrites gets into it.

2610. You are not able from your own judgment to draw any conclusion upon the matter ?—No.

2611. (*Mr. Young.*) You said that you inspected the coal as it went on board, but you did not take any care about ventilation, that that was left to the shipowners ; practically, then, you contract with the shipowner to deliver so many tons of coal at a certain place and you do not care how it gets there ?—Very often it is a matter of difficulty to get tonnage at all, and if we hamper a man with all kinds of restrictions as to ventilation he might not be inclined to take our coals. We naturally conclude that a shipowner will take as much care of his ship as he can.

2612. You do not insure the coal yourselves ?—Yes, we insure our coal and shipowner insures the ship.

2613. You only take such steps as are necessary to satisfy the underwriters ?—Just so.

2614. (*Chairman.*) You do not ensure the coal in a fund of your own ?—No, we ensure it in the ordinary way.

2615. (*Mr. Duncan.*) Probably one of the reasons which would prevent you interfering with the owner as to ventilation wovld be that yoor own inspector in whom you have great confidence thinks that ventilation does harm rather than good ?—Captain Steinson thinks so undoubtedly.

2616. On the other hand others might think that ventilation would do good ?—Yes.

2617. Being regarded as an uncertain matter even by your own men, you do not interfere ?—Yes, some are for it and some are against it. Captain Steinson has given strong evidence against ventilation from the bottom, he prefers ventilation from the top by opening the hatches and if possible by having other means of access to the cargo from the deck.

2618. (*Chairman.*) Have the rates you have had to pay for insurance risen much lately ?—Yes.

2619. By about how much ?—It differs to various ports.

2620. Can you give us the percentage of increase ?—I cannot give you that, but I know that after the losses of those three ships on their voyages to Aden, and other ships by fire, the rates did go up.

2621. And they have not fallen since ?—I do not think they have fallen since.

2622. So that the company is very much interested in the best information on this subject being obtained as soon as possible ?—Yes ; because though we insure our coals, still we do not get anything like the value of coal when it has been lost ; we merely get the invoice value ; we lose the interest of the money and various other matters, and perhaps we have to pay a higher rate of freight on the cargo sent out in substitution of the lost cargo.

2623. You do not insure the coal at the port of arrival ?—No ; it is only the invoice price of the coal with a per centage added to cover contingencies.

2624. (*Mr. Duncan.*) Have you ever considered whether your casualties were less than those of other parties exporting coals ?—We have never discussed the subject so far as I know. We take all the means we can to get the best ships and to have them examined and so on, but it never struck us to inquire whether our per centage of losses was greater than that of others.

2625. You are not able to say whether your vigilant supervision in the shipment of your coals has produced a smaller per-centage of losses as compared with the losses in the case of other ships carrying coal ?—No.

2626. (*Mr. Ferguson.*) Those three ships the "Oliver Cromwell," the "Euxine," and the "Calcutta" were loaded under Captain Steinsons superintendence ?—Yes.

2627. At the same time that they were loaded was not there another vessel loaded called the "Corah" which was not ventilated and which went safe ?—Yes.

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2628. So that the supervision of Captain Steinson being the same over the four ships three were burnt and one not ?—Yes the captain of the "Corah" who was an American, objected to ventilation altogether; he said it was a mistake.

2629. The same description of coal was shipped in the "Corah" as was shipped in the other vessels ?—I believe it was the same coal and the "Corah" arrived at her destination safely.

The witness withdrew.

Adjourned to Tuesday next at 12 o'clock.

Tuesday, 23rd November 1875.

The Right Hon. HUGH C. E. CHILDERS, M.P., in the Chair.

PRESENT:

Frederick Augustus Abel, Esq., F.R.S.
Richard Cory, Esq.
George Duncan, Esq.
John Fenwick, Esq.
John Ferguson, Esq.
Charles Icely, Esq.
William Young, Esq.
J. M. Carmichael, Esq., Secretary.

Captain A. P. Sandeman.

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Captain Alfred P. Sandeman examined.

2630. (*Chairman.*) You were, I think, for many years connected with the East India navy ?—Yes, in Bengal.

2631. For more than 20 years ?—In active service 23 years.

2632. On shore as well as at sea ?—Entirely afloat during that period.

2633. Afterwards you were surveyor to the London Salvage Association ?—Yes, for two years, during the years 1871 and 1872. I was their surveyor in Liverpool.

2634. As such you had a good deal to do with the ventilation of coal-laden ships ?—I had special instruction to take an interest in the ventilation of coal ships, and to report the particulars.

2635. Subsequently you have been surveyor in the Marine Department of the Board of Trade ?—Since 1873.

2636. In that capacity have you had to turn your attention to the question of ventilation in any way ?—No; the Board of Trade take no action in respect of ventilation.

2637. Your official connexion with that question has been as surveyor to the Salvage Association ?—Yes.

2638. In that capacity you have formed very strong opinions on the subject, I believe ?—Yes, I have had occasion to form a very strong opinion on the subject.

2639. And you wrote a paper on the subject for the Board of Trade, not long ago ?—In the early part of this year.

2640. About the time when this Commission was appointed ?—Yes.

2641. In that paper you have expressed a very strong opinion in favour of a thorough ventilation of coal cargoes ?—Either a thorough ventilation, or no ventilation at all.

2642. But of the two you consider, I believe, that a thorough ventilation is desirable ?—I think it would contribute to overcome the difficulties.

2643. Will you state to us, in general terms, what your views of proper ventilation is, and the reasons you would give for it ?—The ventilation that used to be adopted varied in its character. One plan of ventilation was this. A platform would be built above the skin of the vessel and from the platform there would be vertical shafts at each hatch, and, in cases where there was no permanent 'tween deck fore and aft trunks on or under the lower beams all running into each other. That ventilation, I think, so far as my knowledge goes, has been very effective in keeping the cargo from combustion. There may be isolated exceptions, but I believe, as a rule, all ships that have been ventilated in that way with coal have carried their cargoes to their destination without any spontaneous combustion.

2644. That practice was in force when you went to Liverpool ?—Yes; it had been recommended and encouraged by the underwriters for some time previously, and my instructions were to give it all the encouragement I possibly could. Then a second style of ventilation was merely running an alley-way by means of dunnage wood or boards placed at an angle on the keelson: that gave a fore and aft current along the middle line on each side of the keelson, and into that the vertical hatch shafts were introduced, all the shafts communicating with each other. I do not think that was so good a plan, for it did not give that amount of ventilation under the body of the coal that the platform did, the platform extending, as it did, into the bilges of the vessel.

2645. That was a fixed platform solidly constructed ?—A fixed platform solidly constructed, bearers being laid athwart-ships, and heavy deals laid fore and aft, so that there was a current of air passing from the bilges into the midship body of the vessel.

2646. You consider the second plan of ventilation in which dunnage boards were laid along the line of keels on inferior ?—I consider it inferior inasmuch as it was rarely that the boards were laid with care—they were generally put up and allowed to shift for themselves after they were up; and in many instances to my knowledge they gave way, and the result was that the coal found its way down on to the skin of the ship, and destroyed the current of air, the ventilation being thereby entirely checked and stopped.

2647. Did the failure of that system lead to any further change ?—The next style of ventilation was simply introducing vertical shafts, many shipowners in Liverpool hold that ventilation is of no advantage whatever, and it was only under pressure that they were disposed to adopt it; and in many cases they would merely introduce vertical shafts in the hatches carrying them down as far as the keelson, but there was no current of air kept up by means of those shafts, for they stepped on to the solid body of the coal.

2648. That system failed ?—That system was a failure. My experience is that where the platform system was adopted it was evident that the owner of the ship was interested in ventilating his ship as well as he possibly could, and where the simple vertical shafts were introduced, it was simply that he ventilated his ship under pressure to comply with the wishes of the underwriters having no faith in ventilation himself.

2649. Has the first system which you described as being formerly in force, that is to say, the platform system, been practically abandoned lately for the other system ?—No, it is still adopted in ships sailing from Liverpool and Birkenhead.

2650. Would you advise its general adoption ?—My opinion always has been that ventilation tended to keep the cargo cool, that is if the ventilation was thorough.

2651. You think that imperfect ventilation does not do so ?—Imperfect ventilation would do no good whatever, I should say. Sometimes ventilation is very expensively introduced into a ship, but no interest is taken in that ventilation afterwards to protect it from damage when ships are loading, and particularly where ships load their cargoes under the high level or coal tips, eight or ten tons of coal will be shot out of a waggon into the ship's hold where those vertical shafts are placed and little or no precaution will be taken to protect the coal from falling on the shafts and damaging them, and I have found in many instances that the vertical shafts have got damaged, and have become choked with coal and been rendered simply useless.

2652. Do you consider ventilation necessary because coal is generally put on board damp, or do you insist upon it in the case of coal which has been shipped thoroughly dry?—I should say it would be practically impossible to ship coal thoroughly dry in Liverpool. It is brought from a distance, in waggons and sometimes it is exposed in those waggons for days. It is very rarely that you have a week of dry weather in Liverpool, and my opinion is that the coal is always wet before it is put into the hold of a ship.

2653. In dealing with the question we must assume that the coal is shipped usually in a wet state?—Certainly some portion of a large cargo.

2654. Is it your opinion, as the result of your experience that where coal is shipped dry ventilation is desirable?—I should say under all circumstances it is desirable. There is a very large amount of pyrites in some of the coal shipped from Liverpool, more in some coal than others. There are some coal owners who hold that their coal is non-combustible and that there is nothing in it to cause heating. On one occasion when I was the surveyor for the London underwriters in Liverpool I had to meet Mr. McCulloch, the inspector for the Government of Madras, regarding the cargo of a ship put back from bad weather. The owner was anxious to condemn it, but though we knew there had been seven feet of water in that ship's hold at one time Mr. McCulloch held that there would be no danger in taking that cargo forward. It was a coal shipped from the mines of Pearson and Knowles.

2655. Containing no pyrites?—Very little or none. I may mention an instance where a cargo of that coal did ignite afterwards. That was in the case of the "Palmyra," last year, which carried a cargo of the same coal to Madras. She had merely a vertical shaft in her hold; she had no fore and aft ventilators whatever, and when the latches were opened at Madras, the cargo was found to be in a very heated state and on fire in one part.

2656. You do not attribute combustion solely to moisture in contact with pyrites?—No, the confined air would conduce to combustion also. I have seen some coal shipped in Liverpool which was heavily charged with pyrites. I have seen veins of brass substance three quarters of an inch to an inch thick.

2657. Is it your experience that it is to the pressence of that brassy substance that combustion is solely due?—I should say so.

2658. You think if coal could be carefully picked so as to contain no pyrites you would have no combustion?—I would not like to say that, but I should say there would be much less liability to combustion. I had to do with a cargo of coal on one occasion in India, a cargo of English coal, loaded in Calcutta for the Persian Gulf during the Persian war in 1855 or 1856, and that cargo was hermetically sealed as effectually as any cargo could be on board ship; it was carried in an American ship, and American ships generally are very strongly sealed. The ship put back from stress of weather, having made a great deal of water, and it was my duty to take charge of her as pilot, and I took her to Calcutta. On the passage there we had some very heavy weather, and having occasion to get out some fresh ropes we had to lift the after hatch, and when we lifted the after hatch we found the cargo on fire. The hatches were put on at once, and we tried to get the vessel into a dock to sink her in the dock, but opening the hatches had given vent to the fire below, and at last the smoke came out to such an extent that after consultation it was found necessary to scuttle her. We accordingly cut holes in her side to scuttle her, having put her on shore, but on account of a falling tide were obliged to cut a hole three feet long before we could get the water in to act on the coal, yet no smoke found its way through the hole, showing how closely the vessel was ceiled. The fire was entirely confined to the midship body of the vessel and aft below; there was no presence of fire above. We found that low down the coals had been on fire.

2659. How many days had she been out?—About a month.

2660. It is very unusual for spontaneous combustion to take place so soon after the cargo had been loaded, is it not?—I do not know of any other such case.

2661. (*Mr. Duncan.*) Were those English coals?—Yes.

2662. (*Chairman.*) Re-shipped in India?—Yes.

2663. (*Mr. Young.*) Had they been lying long on shore before they were shipped?—I think not. I think they were shipped from the stores of the Peninsular and Oriental Company as an accommodation to the Government.

2664. (*Mr. Duncan.*) I gather from what you have said in answer to the Chairman's question that you think spontaneous combustion in coals arises from the pyrites that is found in them?—I think it contributes largely to it.

2665. You do not think spontaneous combustion wholly due to the presence of pyrites?—I have not sufficient chemical knowledge to enable me to say definitely that that is so. I know that cargoes of pyrites after being on board only a few days will frequently be landed in a very heated state. I know that in the case of cargoes coming from the Spanish Main where they have only been a few days on board the steamers, when there has been very heavy weather and when the water has got down into the cargo that it became heated, and the stuff of which those cargoes consist is very much of the same character as that which you will find in coal, it being more soft in the coal than in the Spanish pyrites.

2666. I suppose you consider on that account it is more liable to ignite spontaneously?—Certainly.

2667. Have you considered at all what would be the effect of giving an abundant supply of oxygen to pyrites?—The effect would be to prevent its heating.

2668. But if it had got heated what would be the effect?—If it had got heated I should say the effect would be very bad. The supplying of a superabundance of air would prevent its heating I should say.

2669. If it became heated would it be better to supply air?—If I found a cargo on fire I should certainly batten down the hatches, but it is to prevent the possibility of heating that I would recommend ventilation.

2670. Seeing that pyrites even in the course of a few days, if there is any quantity of it on board the ship, gets heated in some way or other whereby it ignites spontaneously, is it not questionable whether in a cargo of coal containing a large proportion of pyrites, and therefore likely to get heated under any circumstances, you could by any system of ventilation make the air so permeate through the cargo that no part of it could get heated?—I think it has been done; very large cargoes of coal have been carried forward in ships which have been thoroughly well ventilated, there are lines of vessels that have had no casualties whatever in carrying their coal cargoes.

2671. Are you aware whether there are any lines of vessels which have never ventilated at all and yet have carried their coal cargoes safely?—Yes, I believe there are lines of vessels in which a system of ventilation has not been adopted, and which have carried their coal cargoes without any accident.

2672. There are a number of such vessels, are there not?—I believe there are.

2673. You have no statistics to give us showing the number of ventilated ships which have carried their cargoes safely in comparison with the number of unventilated ships which have also carried their cargoes safely?—No, I have not, but I have remarked that some vessels that were well ventilated carried their cargoes without any accident, and I have remarked that others which were indifferently ventilated met with accident.

2674. You recommend ventilation for the purpose of preventing the cargo from getting heated, because

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if the cargo got heated, by supplying air you would be providing the elements for spontaneous combustion?—Yes.

2675. Are you aware that it is oxygen that the pyrites requires to cause it to ignite?—If it is excluded entirely from air it will heat, but if air is supplied to it in sufficient quantities I should say it would keep it from heating; confinement causes it to heat and then oxygen causes it to ignite. I would simply recommend ventilation with that view, to prevent its heating. If ventilation will not prevent its heating, then of course ventilation is not desirable.

2676. (*Mr. Ferguson.*) On what ground have you come to the conclusion that ventilation is necessary at all?—From noticing that ships that have been ventilated have carried their cargoes safely.

2677. You are aware that a great number of ships that have not been ventilated have carried their cargoes safely?—I believe that some large ships have gone away without ventilation and have carried their cargoes safely; but I am not aware that a great many have done so.

2678. Is it from statistics or from observation that you come to the conclusion you have with respect to ventilation?—For two years it was my duty to board every coal-laden ship and to report the particulars as to whether she was ventilated or not; it was very rarely that I came across ships that did not ventilate in some way or other, and in many cases I found that the shipowners took a very large amount of interest in ventilation. I do not know that those shipowers who took that large interest in ventilation were careful to prevent the ventilators from being damaged, but I believe they carried their cargoes safely. The conclusion I have formed is that ventilation keeps the cargo from heating and prevents spontaneous combustion.

2679. Could you give the Commission any statistics with respect to those vessels which you supervised for two years, showing the number of vessels that were ventilated and the number that were not ventilated?—I could name some firms that took a large interest in the ventilation of their ships and whose ships were navigated by men who took a great interest in seeing that the ventilation was properly carried out, and I believe that those vessels were in consequence enabled to carry their cargoes without spontaneous combustion.

2680. Have you any statistics to give us showing the number of vessels that carried their cargoes safely that were not ventilated?—I have not.

2681. What you say about ventilation is a sort of general opinion?—It is more the result of my observation whilst I was occupied as surveyor to the Salvage Association.

2682. We have had the cases of four ships mentioned to us which were all loaded at the same port with the same coal, three of them were ventilated, the fourth was not; the one that had no ventilation took her cargo out with safety, while the other three were burnt; would that fact alter your opinion in regard to ventilation?—In the cases of those three that were burnt it is possible that the ventilators may have become damaged.

2683. Your experience is confined to shipments of Liverpool and Birkenhead coal?—Yes.

2684. You have had no experience of shipments on the east coast?—No. What I had to do with was coal from the Midland districts, from Wigan and from Wales.

2685. Are you aware that in a majority of the ventilated vessels that have been burnt, the fire has occurred near the trunk ventilators which have been put in the hatchway?—I am not aware of that.

2686. Was the coal which was shipped at Liverpool and Birkenhead from various pits?—Yes; I had nothing to do with the shipping of the coal, I had merely to report to the underwriters that such and such coal was being put on board.

2687. You did not investigate the quality of the coal?—No, not beyond looking at it in the waggons.

2688. You only made a superficial observation of it?—That was all.

2689. Did you observe that the vessels took fire which carried coal with a large amount of pyrites in it?—I cannot say that I did.

2690. (*Mr. Duncan.*) What is the result of your observations with regard to the quantity of pyrites contained in coal shipped on the Liverpool side of the Mersey as compared with that shipped on the Birkenhead side?—The coal shipped on the Liverpool side has less pyrites in it.

2691. (*Mr. Abel.*) From your evidence I gather your view of the effect of proper ventilation to be this, that by passing sufficiently rapidly and in sufficient quantity cold air over the heated coal you prevent an accumulation of heat, and you even remove the heat which has been generated?—That is my view.

2692. Do you think it is possible to stow coal on board ship with such care and with such arrangements for natural ventilation as to ensure the passage of air thoroughly through anything like every portion of the mass of coal on board ship?—Not through every portion, but you could carry these trunk ways through various sections so as to ventilate a larger cubical space. At present you have merely two fore and aft ventilators on the middle line below, and probably one or two in the middle body of the vessel, but I would increase those fore and aft ventilators, and so operate on a larger space.

2693. With the very best possible arrangement of natural ventilation, would it not be the case that you would still only ventilate portions of the cargo?—No doubt, but you would reduce the amount of solid coal unventilated. As things stand now there is a very large body of coal that has no ventilation whatever, and the suggestion I make is with a view to carry ventilation through that mass of coal.

2694. Do you think any arrangement of shafting could ensure the carrying of air through the main body of the coal?—I do not see why it should not in the same way as it is carried through a body of rice.

2695. Under any circumstances the ventilation would only be partial ventilation, would it not?—If you arranged the ventilators so as to operate at intervals of 9 or 10 feet I should say, as all the coal does not lie quite solid, that it would tend to reduce the liability to heat.

2696. (*Mr. Young.*) I have a paper before me signed by you dated March 3rd, 1875, at the beginning of which you say, alluding to vessels burnt at sea, "We may with truth say a week now seldom "passes without its sad tale of loss in many "instances of life, as well as of valuable ships, "and all attributable to this cause," and in another place you say, "The sooner it is insisted upon" (that is to say ventilation) "and made compulsory, "the sooner will we hear of the sad and revolting "catastrophes which have lately resulted from spon-"taneous combustion with coal cargoes on foreign "voyages." Are you aware of the number of vessels that have been lost from spontaneous combustion of coal as compared with the number that have gone with safety in 1874?—No. I do not know that there was a large proportion lost, but the losses were very serious.

2697. Should you be surprised to learn that the proportion of losses was about 1 in 2,000?—I am surprised to hear it.

2698. Do you know how many lives were lost in those "revolting catastrophes" that you allude to?—I was alluding then particularly to the loss of the "Euxine" and one or two other vessels; the cases were very revolting, I thought.

2699. Do you know how many lives were lost?—There was a large loss of life.

2700. Should you be surprised to learn that there were only 27 lives lost altogether in 1874?—I should have thought the number of lives lost would have been greater, but you have the data; I have none.

2701. In your paper you appear to point to a compulsory inspection by the Board of Trade, or by the Government, of all ships loading with coals. Do you still adhere to the opinion that such an inspection

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would be advisable?—If ventilation is necessary, it ought to be carried out effectively. Many ships are ventilated and the ventilation is of no use whatever.

2702. Do you think Government interference would would make it effective?—It would contribute to it.

2703. You think ventilation of coal-laden ships could be effectively carried out only by a Government inspection of every such ship?—I do not say that. In my paper I merely said that so far as I knew, the ventilation that was adopted in coal laden ships was often seriously damaged from want of care and want of interest in it.

2704. You recommend that all coal-laden ships should be inspected?—I confess that was my view, but what I recommended in my paper about supervision applied specially to boats.

2705. Have you considered the question how it would be possible for the Government to inspect every coal-laden ship?—I know many instances where, if I had been a Government inspector, I could have been instrumental in keeping the ventilators from being damaged which were damaged and not put to rights.

2706. Do not you think the owner, the captain, and the crew would naturally take care of those things?—There may be no captain with the ship.

2707. Then there would be the mate?—Frequently the mate does not take interest in such matters.

2708. Do you think the supervision of a Government surveyor better than the care which men directly interested in the matter may be supposed to take?—I think it just possible that a Government surveyor's inspection would be better. He might see that it was done.

2709. Are you aware that in 1874 there was shipped from England 12,000,000 tons of coal?—No doubt you are correct.

2710. How many thousand inspectors would that require?—Not many. It does not take a man long to see that the work is being done properly; if he had to direct it himself it would take a very large number.

2711. If the object was to prevent the ventilators being damaged an inspector would have to be always present when a ship was loading?—Under those circumstances it would take a very large staff, but I never intended to suggest any such measure.

2712. (*Chairman.*) Coming to the last point first, there are two ways in which the law might interfere; it might simply require a certain system of ventilation to be applied to all coal-carrying ships, or it might in addition to that requirement direct that inspectors should be appointed to see that it was carried out. Is it your advice to us to recommend that a law should be passed enforcing a certain system of ventilation?—Yes, if it is considered that ventilation is desirable. If my opinion is not justified, of course such a law should not be passed, but if ventilation is desirable it ought to be carried out effectively. Certainly some interest should be taken in ventilation, and my experience is that no interest as a rule is taken in the ventilation of ships, though there are exceptions; there are many owners who take a very large interest in the ventilation of their ships, and there are others who take no interest in it whatever, though ventilators are put into their ships.

2713. You would recommend us to advise Parliament to pass a law enforcing a certain system of ventilation?—Yes, being of opinion that ventilation is desirable.

2714. Would you in addition to that, as an administrative arrangement, not only require that ventilation, but appoint officers to see that the requirement was enforced?—My feeling is that it would be necessary that some one should be present at the loading of the ships to see that the requirement was carried out.

2715. You would not leave it to the ordinary process of law to punish the owner who did not comply with it, but, as I understand, you would require that inspectors should be appointed to see that the law was properly carried out?—I cannot say that I am able to offer a decided opinion upon that point. I should suppose that inspectors would be appointed.

2716. You have not considered the administrative point sufficiently to be able to give us any distinct recommendation in regard to it?—No; I quite feel that there is very little interest manifested in ventilation, and if ventilation is of any advantage whatever possibly it might be necessary that inspectors should be appointed.

2717. You said in reply to Mr. Young that the strong language you used in your paper was induced by the loss of the "Euxine" and one or two other ships about that time. There were three ships lost about that time, viz., the "Euxine," the "Oliver Cromwell," and the "Calcutta." Are those the three you refer to?—Very likely. The "Euxine" I know was one.

2718. They were loaded in the same month and were lost about the same time?—Yes.

2719. Are you aware that those three are the very ships to which Mr. Ferguson referred when he said that out of four ships which sailed on the same voyage, for the same company, with the same coal, three of them being ventilated and one not, the three that were ventilated were lost, while the one that was not ventilated (a vessel called the "Corah") reached her destination safely?—To my knowledge on one voyage the "Euxine" had her ventilators damaged. Referring to my reports I find I reported that the after ventilation was choked with coal. I think she is a ship that generally loads under the tip in the West Float, and it is a questionable thing whether the vertical shafts of the ships are not in many cases damaged in loading under these tips.

2720. Would not it shake your impression, which was strengthened at the beginning of the year by those three casualties happening just together, to learn that those three were distinctly cases of ventilated ships, those cases having been brought before us pointedly by witnesses as evidences of the impolicy of ventilation, seeing that those vessels were lost while another ship, loaded with coal in the same dock (the Northumberland dock in Newcastle) at the same time and for the same owners (the P. and O. Company), arrived quite safely though not ventilated?—That is a very strong case certainly.

2721. With regard to the cost of ventilation. You have compared the ventilation of rice cargoes with the ventilation of coal cargoes. I suppose the freight of rice is a very much better freight than the freight of coal?—Yes.

2722. So it does not at all follow because a particular system of ventilation could be applied to rice that it could be applied to coal?—An increased freight would have to be charged if an improved system of ventilation were adopted.

2723. What would it cost to thoroughly ventilate on your plan a cargo of 1,000 tons of coal?—I do not think it would cost a great deal. The ventilators are made of rough deals, and rough deals are to be had very cheap. I give you an idea of the expense in my paper, viz., 1*s.* 6*d.* a ton.

2724. 1*s.* 6*d.* a ton is a very serious per-centage, is not it?—It is very trifling when weighed against the additional premium that is frequently charged.

2725. Which has been charged within the last year or two?—Yes.

2726. Have you observed that the premium is apparently out of all proportion to the risk, looking at the per-centages before us?—Yes; the per-centage of loss appears to be very small.

2727. The 1*s.* 6*d.* is a very serious addition to the other charges, is it not?—Yes; it would be an additional expense, no doubt.

2728. When you estimate 1*s.* 6*d.* you take it as the minimum; it might be considerably more?—No, I do not think it would be more.

2729. (*Mr. Fenwick.*) You say some shipowners are very particular with regard to ventilation, and some take no interest in ventilation whatever?—Yes.

2730. To what do you attribute that?—To want of confidence in ventilation, I suppose.

The witness withdrew.

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MR. MATTHEW ANDERSON examined.

2731. (*Chairman.*) You are a shipowner at Leith?—Yes.

2732. And you have been for some time, we understand, superintending engineer there?—Yes, for the last 12 years.

2733. In that capacity what have been your duties?—I have had to do with the loading of ships and looking after the engines, boilers, the coal, and so on.

2734. The subject of coal has come especially under your attention?—Yes; I was born close to a coal pit.

2735. Have you had any experience in the loading of coal ships?—I have not had to do with any ships taking entire cargoes of coal; but I have seen scores of ships loaded with entire cargoes of coal.

2736. You have not had the superintendence of the loading of those ships?—No; I have merely seen them.

2737. You have had the superintendence of the loading of coal in ships' bunkers?—Yes.

2738. You have arrived at some conclusions on the subject of the proper ventilation of coal?—Yes.

2739. Will you tell the Commission what those conclusions are?—I think that spontaneous combustion always arises from the presence of pyrites—what we call "brasses"—and when the water comes in contact with it it dissolves the sulphur of the pyrites and heat is evolved, and hence the coal ignites after a certain time. I know that from experience. In the district in which my father was a colliery engineer, formerly, when there was no demand for waste coal it was all put into great heaps, and after a certain time the rain or the snow came on to it, and spontaneous combustion took place. I could see from where I lived no less than 20 or 40 of those heaps on fire by spontaneous combustion. My opinion therefore is that when coals, if they contain pyrites (and very few coals are free from it), are put into a ship's hold, as soon as water is brought in contact with them spontaneous combustion takes place.

2740. What is the remedy you would apply to prevent it?—You cannot prevent it, do what you like, only if you could put the coals in perfectly dry and keep them dry there would be no fear of spontaneous combustion.

2741. It is your impression, that whether ventilated or not, coal will take fire at a certain time, if it is wet?—Yes, decidedly so. Ventilation, in my opinion, is only useful when coals give off a lot of gas.

2742. To prevent explosion?—Yes.

2743. But it has no effect in preventing spontaneous combustion?—No. For instance, there was a colliery in the West of Scotland, that was a fire for months and they had no means of putting it out, but a Dr. Gurney took it in hand and he excluded all the atmospheric air, where he could and pumped carbonic acid into it for weeks, and eventually put it out. My idea of the best way of preserving coals from spontaneous combustion is to put perforated pipes down each side, to divide the ship into two halves, each side of the keelson, and to have perforated pipes leading down from the deck. Then I would take a daily record of the heat by lowering a thermometer so as to see whether the heat rose or not. If the heat rose, you might be sure that the first stage of spontaneous combustion had commenced; if it went on increasing, I would exclude the atmospheric air, if it still went on increasing, then carbonic acid gas would be the best thing to put it out, because carbonic acid expels all the atmospheric air. If the atmospheric air were entirely excluded, there would be no oxygen to feed the coals, and hence the fire would so go out.

2744. Then so far as prevention is concerned, you do not think that either ventilation or the absence of ventilation makes any difference?—No.

2745. You think that coal of a certain description, with a certain amount of pyrites in it, and with a certain degree of dampness will take fire whatever yon do?—Yes.

2746. And that all we have to look to is to prevent that kind of coal from being put on board in that state, and to provide the best way of putting the fire out when it came?—Yes, that is my opinion, to discover the first stages of it, and check it in its first stages

2747. But you think that no artificial contrivance in the character of ventilation will do any good?—Not when a large body of coals is put on board ship in a damp state.

2748. Your belief is that coals never take fire except they are put on board in a damp state?—No. As a proof of that, I remember the colliery where my father was engineer for, the small coal which was thrown aside and exposed to the wet took fire, but the other coal, which was protected from the weather did not take fire. At times there was not a sufficient demand for the coals to keep the collieries employed, and they used to bring the best coals to the surface, and, to protect them, they used to be put in sheds where they could be kept dry.

2749. And you never knew a case of dry coal taking fire spontaneously?—No.

2750. Your opinion is, that the presence of pyrites, and the presence of moisture are the sole causes of spontaneous combustion?—Yes.

2751. And you are of opinion that under those conditions spontaneous combustion infallibly follows, whatever precautions you take?—Yes.

2752. (*Mr. Duncan.*) Then you disapprove of the idea of ventilating ships by shafts through the hatchways?—To prevent spontaneous combustion, certainly.

2753. You would allow the gases that escape from the coals to get out?—I would make provision for that.

2754. Your opinion clearly is that it is not judicious to put ventilators through the coals?—Except to ascertain the temperature, which I would do by means of those pipes.

2755. Are you of opinion that ventilation might do harm?—If the coals are put in damp, and therefore liable to spontaneous combustion, it does do harm, inasmuch as atmospheric air is one of the elements that is required to dissolve the sulphur; and therefore the more air you introduce, the worse it becomes; the more atmospheric air you add, and the more hot it becomes, so as to give off flame or heat the worse it becomes.

2756. Irrespective of that, in the first stages of spontaneous combustion, is there any element in atmospheric air which would have a tendency to combine with pyrites, and so produce combustion?—The atmospheric air and the water dissolve the pyrites and liberate the sulphur, and then the action takes place upon the coals. That is the first stage of it.

2757. For the purposes of igniting the sulphur, you do not think a gas is required to be combined with it?—No; the gas is the product of the decomposition of the iron pyrites in the coal.

2758. There would be gas evolved if water was decomposed and put into a gaseous state?—Yes. Where do you derive the gas from, if it is not from the decomposition of the coal?

2759. You do not think that thorough ventilation throughout the bulk of the coals would be a matter that this Commission should recommend to be introduced in every ship?—If I had a ship loaded with coals, the plan which I have suggested is the plan I would adopt: that is the only one I recommend.

2760. That is to put perforated tubes down and ascertain whether the temperature was rising or not, and if you found that the coals were on fire, you would force down carbonic acid in order to put the fire out?—Yes. If carbonic acid was not available, those pipes would convey water down below.

2761. Those pipes would only convey a limited quantity of water down, would they not?—If you had a 4-inch pipe that would convey an unlimited quantity.

2762. You would flood the ship with water?—Yes, and that would put it out. If those pipes were like telescopic tubes it would be a very simple process to put them in.

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2763. (*Mr. Ferguson.*) We have had it in evidence that coals carried in iron ships, which were loaded in perfectly dry weather, took fire; how would you account for that?—I would not believe it.

2764. Is it a fact that some coals contain water in themselves?—No; coals contain no water, but coals will absorb water.

2765. Supposing they have not been exposed to any surface water, is not it the fact that there is a certain per-centage of water in some kinds of coal, and more in some kinds than others?—No.

2766. It has been attempted to account for the fact of coals shipped dry taking fire by the theory that they contained a certain quantity of damp in themselves, and that that, along with the pyrites, caused spontaneous combustion?—There are certain elements which go to form coal, I cannot tell you the constituent parts of coal, but I never heard of water being one of them.

2767. I think some of your Fife coal contains a great deal of water?—Simply for this reason, in the Fife coal field there is water adjoining the pits; there is water constantly in the pits, both at the top and the bottom of the seam, but that is an absorption of the water into the coal.

2768. You think that the coal that is said to have been shipped dry may have contained water in it which it had absorbed?—Yes; it very likely may have contained water that it absorbed, but of the elements that go to make up coal, water is not one.

2769. If there was water in the ship's hold and the coal contained iron pyrites, you probably think that that would account for the coal that was shipped in dry weather taking fire?—The water which the coals had absorbed previous to their being put into the ship, and the pressure of iron pyrites, would conduce to generate heat.

2770. Are you familiar with the Scotch coal shipped at Glasgow and Leith?—Yes; I know the Bonnie Hill, the Rough Rigg, the Stan Rigg, and some of those other Wishaw coals.

2771. Are they all equally liable to spontaneous combustion; is the pyrites contained in the coal wrought from all those pits about the same?—In the Rough Rigg and the Stan Rigg it is the same. I cannot go through the whole catalogue of the various descriptions of coal.

2772. Your idea is that pumping in carbonic acid is the proper way of putting out fire which has taken place in coals from spontaneous combustion?—That is the quickest method of stopping the fire.

2773. Do you consider it better than pumping in water?—In the first instance, certainly, because you do not injure the coals to the extent that you do by pumping in water.

2774. Would the carbonic acid injure any other cargoes?—Certainly not; the carbonic acid being heavier than atmospheric air, it has a natural tendency to descend and expel the atmospheric air.

2775. Supposing it was put in at the bottom of the vessel, at the keelson, would it rise and expel the air?—No.

2776. It would lie at the bottom of the ship?—Yes.

2777. (*Mr. Young.*) Where were those heaps of coal which you say you have seen on fire?—In the county of Durham, at the Marquis of Londonderry's collieries.

2778. Were those coals stacked for the purpose of preservation?—No; at that time, 40 years ago, there was no demand for coke, for railway or for other purposes; iron smelting was in its early stage, and the coal was all screened. That was the small dust put away as being useless. They used to make the tracks for the railways with those small coals.

2779. Practically nobody cared whether they were burnt or not?—No, they took no interest in them whatever; they did not take the trouble to set fire to them.

2780. (*Mr. Icely.*) You said that your experience with regard to the stowage of coal was confined to ships' bunkers?—Yes, and what I had seen shipped on the Tyne.

2781. Have you any experience of spontaneous combustion taking place in coal taken into a steamer's bunkers?—Yes, I had one case of a fire.

2782. Could you give the Commission any information with respect to that case?—In that case the fire arose from the coals being put into the bunkers damp, and the bunkers being a little too near the boilers.

2783. You think possibly the communication of heat from the boilers had something to do with it?—The coals had only been in the bunkers a few days when they took fire; the heat was the originator of the combustion.

2784. The heat being communicated by the boilers?—Yes.

2785. At what distance were those bunkers from the boilers?—12 inches.

2786. Do you think it was possible that the heat could be communicated. The boilers would have some non-conducting substance on the outside of them; do you think it would pass through that non-conductor and then through a space of 12 inches and affect the coals?—It did so; and as a means of prevention I put another plate a couple of inches apart from the boilers, so that there might be a current of air between.

2787. It was your impression that the heat was communicated to the coal by the boilers?—Yes; we never had the same casualty again, though we used the same coals afterwards.

2788. Can you tell the Commission whether those coal boxes were ventilated or not?—They were ventilated in the usual way, by taking the lid off when the men went down.

2789. It was simply an exit for the gases that might have accumulated?—It had always been found sufficient.

2790. You say the coals were only a very short time on board?—Yes; they got wet coming off in the craft that brought them to the ship; they caught fire after being out 11 days.

2791. Was it the custom to take the temperature?—No.

2792. How was the fire discovered?—By the trimmers going in to trim the coals. The coals had not got on fire to such an extent as to set the ship on fire, but they were smouldering and hot and ready to burst into a flame.

2793. You were convinced that the fire had arisen from the moisture of the coal?—The coals had gone in damp, and coming on board wet, together with the heat from the boilers, combustion was generated a great deal sooner than it would have been if the coals had been put in dry and there had been no heat near.

2794. That is your only experience of any casualty in the case of coal stowed in bunkers?—Yes.

2795. (*Chairman.*) The pipes which you would pass down in order to take the temperature would admit of air going down too?—Yes.

2796. Would you put taps on them?—Yes; I would screw taps on them, I would make use of them either to put the thermometer down or to put down carbonic acid, or water if carbonic acid was not available.

The witness withdrew.

Mr. William Darley examined.

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2797. (*Chairman.*) You were formerly foreman of the yard at Chatham?—Yes.

2798. And lately you have been employed as a Nautical Assessor under the Board of Trade?—Yes.

2799. In that capacity you have had some experience of cases of explosion or spontaneous combustion in coal ships?—Yes, such cases have come under my notice in the inquiries on which I have sat.

2800. How many such cases have you been concerned with?—The "Zeno" was one.

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2801. Was that a case of explosion or spontaneous combustion ?—Explosion of gas. Then there was also the case of the "Berwick."

2802. Was that a case of explosion or spontaneous combustion ?—That was a case of explosion. Then there was the "Maria Lee." I think that was not an explosion from coal, but ignition from spirits. Then there was the "Herbert Graham," that was a case of spontaneous combustion.

2803. So that in those inquiries two cases of explosion, and one of spontaneous combustion have come under your notice ?—Yes.

2804. Have you any other experience of spontaneous combustion on board ship ?—When I was foreman afloat at Sheerness, and had charge of the ships in the harbour there, I found there were cases of spontaneous combustion on board ships in which the coal was stored. Those were cases of extreme heating of the coal, and there was one case occurred in the dockyard, where the coal took fire.

2805. To begin with the causes, what is your impression with regard to the cause of spontaneous combustion of coal ?—I believe spontaneous combustion of coal to arise from various causes ; one cause is the shipment of "green" coal. I do not consider it safe to ship very green coal.

2806. What do you mean by "green" coal ?—Coal taken from the pit's mouth and transferred directly to the ship.

2807. (*Mr. Duncan.*) Would you call it fresh wrought coal ?—Yes, fresh wrought coal taken from the pit, and put at once on board ship. In the next place it is not safe to ship coal in rainy weather. If you ship coal in rainy weather it is more liable to ignite spontaneously.

2808. (*Chairman.*) With regard to "green" coal you consider that shipping the coal in a "green" state is a cause of danger whether shipped in a damp state or not ?—Yes.

2809. Danger in the sense of spontaneous combustion or of generation of gas leading to explosion ?—Both.

2810. Have you had any experience of spontaneous combustion in dry coal ?—No ; in the case I referred to which occurred in the dockyard the coal was shipped in very wet weather.

2811. So far as spontaneous combustion goes your experience is only of coal which has absorbed moisture ?—Coal which has been exposed to moisture.

2812. As to "green" coal, how does that produce spontaneous combustion ?—When it is wrought from the pit it is full of gas ; if it is exposed to the atmosphere previously to being shipped some of that gas is emitted that would be otherwise retained, but when confined it becomes dangerous and ultimately heats.

2813. Does the presence of that gas in dry coal lead to heating or only to explosion ?—I judge that the "green" coal is never very dry. I think there is a certain amount of humidity in it.

2814. So that in the long run it comes to moisture being the cause of spontaneous combustion ?—Yes. The tendency of wet is to cause a decomposition of the coal I believe.

2815. It comes therefore to this, that the cause of spontaneous combustion in your opinion is moisture, however that moisture may be generated ?—Yes. In the case to which I referred as having occurred at Sheerness I had a correspondence with Mr. Rogers who had charge of the coal depôt there. I wanted to satisfy my mind upon it, and he told me that the coal was shipped in wet weather, and in each case where I have come across coals that have been destroyed by fire I have found on inquiry that wet has had something to do with it.

2816. That being so, I suppose your opinion is that the first precaution which should be taken is not to ship wet coal ?—Not to ship wet coal.

2817. That would be the best of all remedies ?—Yes.

2818. What is the next preventive measure you would take —In the next place I would put it into a dry ship.

2819. Assuming coal to be shipped which was not perfectly free from moisture, what appliance would you use in order to prevent spontaneous combustion ?—You want to keep it dry. There are some coals that do not require so much ventilation as others ; it depends on the quality of the coal. In some it is necessary, I believe, that you should have no ventilation at all (or at least only surface ventilation), or the tendency would be to cause spontaneous combustion. As far as my experience goes I would not offer an opinion whether earthenware tubes introduced into coal liable to spontaneous combustion would not produce a beneficial effect in the earlier stages, because when the gases are compressed and confined the tendency, I believe, is to heat the coal, and then danger arises ; but perhaps if the gases were allowed to escape from perforated pipes that might be prevented ; but that is a matter of speculation.

2820. You are not able to give us any distinct opinion of your own upon the question whether ventilation does or does not obviate the danger of spontaneous combustion ?—No, I have never heard of ventilation generally injuring coal. I have found in the generality of cases where coal has sustained injury that it has been in the absence of ventilation.

2821. In the case of the "Herbert Graham," was the ship well ventilated ?—I do not think she was properly ventilated, and I am doubtful whether she was a very dry ship. I cannot remember positively whether the coals were shipped wet in the "Herbert Graham." If they were shipped wet it would account for the spontaneous combustion.

2822. You do not remember whether there were any appliances for ventilation in that ship ?—From my inquiries I was not perfectly satisfied with the ventilation.

2823. What is your recommendation as a whole upon the subject of ventilation. I gather that in the case of some coal you would not ventilate it at all ?—In the case of some coal you need to be very cautious in ventilating it. The fact is on that point I am hardly competent to give a decided opinion ; there are some cases that are a little doubtful, and they want experiment.

2824. This Commission being appointed specially to report upon that question, you do not feel able to give us any very distinct recommendation upon the subject of ventilation ?—I think that surface ventilation cannot do any harm. I think it would be well to test, by experiment, the effect of the introduction of earthenware tubes into cargoes of coal known to be liable to spontaneous combustion. It might probably be found that the introduction of such tubes would have a beneficial effect ; that is to say "before" heat has been generated. I believe, as others have observed, that the effect of introducing atmospheric air afterwards would be only to increase the combustion.

2825. Do I rightly gather that you would recommend that experiments should be made to test these matters ?—Yes.

2826. So far as explosion is concerned you are of opinion that every facility should be given for carrying gas off ?—Yes.

2827. Would you do more than that ?—Yes ; I would insist that the coals, after being taken from the pit, should be exposed a certain time to the atmosphere in order to allow for the escape of the gas.

2828. After they were on board all you would do would be to ventilate the surface ?—After they were on board they should be kept dry and be properly ventilated. I know of no better system of ventilation than that introduced in the Royal Navy, and which I explained to the court at Hull in the case of the "Zeno." There are some matters of detail which I could add to it, and particular modifications might be applied to particular sorts of vessels. The system to which I refer is a system of pipes and cowls—a system which has been introduced into the Royal Navy with the greatest possible success. Some men would object to going to the expense of large iron tubes, and in some cases copper tubes, where they come near the binnacle, but

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wooden trunks might be used with advantage, and in some cases wind sails.

2829. That is for surface ventilation ?—Yes; and I would recommend that you should carry it into the body of the coal also. Take the case of Cardiff hard brittle coal, you may ventilate it to any extent. I believe the principal danger that you find in coal of that description is that when a naked light is brought into proximity to it an explosion takes place. In each case of explosion that I have come across in that coal the explosion has taken place in consequence of a man taking a light below. Then I think the system of fitting up a ship should be such as to prevent the gases accumulating, so as to prevent parts of the ship where coal is not stowed acting as receptacles for gases.

2830. I think M. Vassard, whom you examined in the "Herbert Graham" case, in answer to a question put by you, said that in the case of coal containing pyrites there should be no ventilation, as it was liable to ignite spontaneously, and that ventilation was only useful in the case of coal containing no pyrites, in which case it was of use for carrying off the gas and to prevent explosion. Is that your view ? — I should defer to the opinion of M. Vassard on that question; he is a chemist and more competent to give an opinion on such a point than I am.

2831. Do you agree with M. Vassard ?—Yes. In addition to a system of ventilation for the coal, it is necessary that precautions should be taken for the ventilation of the ship. This is a drawing of the "Zeno" (*producing it*). This (*pointing to the compartment the fore side of the coal hold*) became a receptacle for the gas generated from this coal. The bulkhead not being watertight or not being airtight, the gases that were generated here passed through, and the result was that this was made a receptacle for the gas. This (*pointing to it*) was used as a water ballast tank, and there was a pipe leading from here into the hold to let the water off, and I believe there were a couple of holes in the deck, to allow the gases to escape and to run off any water that might be here into the ballast tank. That answered two purposes; whilst it allowed any wash of water to run down it also allowed the air to arise; consequently this (*pointing to the compartment the fore side of the coal hold*) always became a receptacle for gas. When one of the men went down through the forepeak with his light the gas exploded, blowing him into the bows of the vessel, and in 50 minutes there was 33,000*l.* worth of property gone, and 29 hands had to take to an open boat in the Bay of Biscay.

2832. (*Mr. Duncan.*) You say that there is greater danger of spontaneous combustion if the coals are shipped wet. Have you formed any definite opinion as to the effect of water upon the coals in producing spontaneous combustion ?—A chemical action goes on, I believe, on the introduction of water into the coals, tending towards spontaneous combustion; and there is a demonstration of that in the circumstance that in those cases where spontaneous combustion has taken place the coal has been brought in contact with the wet either by being shipped wet or by being put in a wet ship.

2833. Supposing you were told that in numerous cases where the coals had been shipped as dry as possible those coals had taken fire, and in addition to that, as appears from the statistics which have been taken, it turns out that the coals in which the greatest number of cases of spontaneous combustion took place were coals shipped in the three driest months of the year, would that have any effect upon your judgment ? —It would lead me to hesitate in giving a definite opinion with regard to wet; but of course there are different descriptions of coal, and what would affect one would not affect the other.

2834. Did I rightly understand you to say that water does not affect Cardiff coals ?—I believe that water does affect them, that is that they are better kept dry, but I have come across no cases of spontaneous combustion in Cardiff coals that have been wet.

2835. Has it ever occurred to you to inquire whether the component elements of Cardiff coals, and of those other coals that have taken fire frequently are different from one another ?—Yes, I believe they are different from one another as shown by the different effects produced, because one will explode and the other will ignite. The coal from Cardiff, particularly the Cardiff hard brittle will explode. I do not know (because I have not got extensively into it) whether you will find that it is liable to ignite spontaneously, but in some correspondence which I have had with people at Cardiff and which is contained in a paper which I beg to hand in (*handing in the same*), they tell me that they have come across no cases of spontaneous combustion in that coal.

2836. Do they say what spontaneous combustion arises from ?—Yes, the presence of a large quantity of iron and sulphur in the coal which, when acted on by water, produce a chemical action, causing spontaneous combustion.

2837. It is clear that there are two elements of danger in coals. Is your recommendation to the Commission that different courses of action should be taken with coals that have those different dangers in them ?—Yes, decidedly.

2838. That is to say that those Cardiff coals that are liable to explode should be differently dealt with from those that have large quantities of pyrites in them ?—Yes, just so. My experience has been more particularly in cases of explosion. I have only sat as nautical assessor on one case of spontaneous combustion, that of the "Herbert Graham." Then there was the case that occurred at Sheerness where the coal took fire, in that case the coal had been exposed to wet.

2839. In the case of the "Herbert Graham," was the quality of the coal shipped on board a subject of inquiry ?—Yes. Mr. Vassard was present and gave his report. I think you will find the report in the evidence.

2840. Do you remember whether the coals that that vessel had on board were more dangerous than the coals ordinarily shipped ?—I cannot charge my memory with that.

2841. You are not prepared to speak definitely as to what precaution should be taken in the case of ships having on board these coals which are highly charged with pyrites and liable to spontaneous combustion ?—No; I would not give a positive opinion upon that. I would suggest that a great deal might be gleaned by the Commission from those who are constantly shipping the coal. No doubt also coal owners at different ports for their own interest would be glad to give you information upon the point.

2842. (*Mr. Ferguson.*) You say that some coal requires ventilation, will you tell the Commission on what grounds you come to that conclusion ?—From the terrible explosions that have taken place where ventilation has not been attended to.

2843. That is in reference to Cardiff coal that explodes, not coal that is liable to spontaneous combustion ?—Just so.

2844. Is that system of ventilation by which the gas is taken from the top of the coals to the deck by pipes and cowls the system which you say has been adopted in the Navy with success ?—Yes.

2845. In the coal-carrying vessels of the Navy have there been fewer explosions than in other vessels carrying coals ?—I believe you will find that to be the case.

2846. Has the system to which you refer been adopted in the vessels of the Navy as well as in vessels carrying coals for the Navy, and does the answer you give with respect to comparative immunity from accident apply to both classes of vessels ?—Yes.

2847. Do they keep some vessels of their own to carry coals in ?—I believe in some cases they do. At all events they have vessels in which their coals are stored as coal depôts, where they remain for a considerable time.

2848. Your experience has been more in the explosive coal ?—Yes.

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2849. You cannot say whether ventilation is good or whether it is bad in the other kind of coal?—I cannot say positively.

2850. (*Mr. Abel.*) Your experience with regard to explosions on board coal-laden vessels is limited to two instances?—Yes; it is limited to two instances.

2851. You have no idea what proportion of casualties is ascribable to explosions on board coal-laden ships, and whether it is large or small?—I believe there is a great number of coal-carrying ships in which explosions take place, many that you hear nothing of. When we were sitting at Hull on the "Zeno," a vessel exploded, I could not tell you her name, and there was a loss of life. There was one case I read about in which 22 men out of 23 were blown into the air. Another where 8 were blown into the air, 4 fell overboard and were drowned, and 4 fell dead upon deck. Such cases are of common occurrence. I have not the slightest doubt that a great many cases have occurred, in which vessels have been lost from explosion, and all hands sacrificed. In this case of the "Zeno," 29 people had to put to sea in an open boat, and if there had been the usual weather in the bay, they would have been all lost, no doubt. The evidence that that vessel was not properly fitted was very clear, and in fact, it was admitted by the owner of the vessel, for when I was in the court I expressed an opinion that the vessel was built for one purpose and used for another, and the representative of the owner rose and admitted that that was the fact, that she was built for a general cargo, and in that exceptional case she was used for a coal cargo, that coal being a dangerous coal.

2852. (*Mr. Icely.*) With regard to the casualty which occurred at Sheerness, that was in a hulk?—Yes.

2853. Did any case of spontaneous combustion or explosion ever come under your notice in the case of any ship afloat having coal on board?—I have never known any such case in any vessel belonging to the navy.

2854. Are you aware whether it is the custom to keep coal in navy ships in harbour for any length of time?—No.

2855. Is not there a First Division of Steam Reserve, consisting of vessels having coals and other stores always on board?—There is a certain quantity of coals on board such vessels, but I do not think the bunkers are all filled up.

2856. Those ships are supposed to be ready for sea, and they have all their stores on board, coal included?—I am not able to speak confidently on that point.

2857. Are you aware that some of those ships will remain probably a year or two without being employed the coal still remaining on board?—I have not known any vessels remain in the First Division, at Sheerness, so long as that.

2858. Not for a twelvemonth?—No, I do not think so.

2859. For six months?—I should think they have perhaps for six months.

2860. During that time you never heard of any accident occurring?—No, I have heard of cases of the coals becoming heated and in those cases the engineers, who are very careful men, watch the coals; they daily test the heat of the coals by the thermometer tubes, and if they have any doubt of their safety they have them removed either to another bunker, or on deck.

2861. You attribute the immunity from accident in those ships to those precautions being taken by the engineers?—Yes, and the system of ventilation that is generally adopted.

The witness withdrew.

Adjourned.

Tuesday, 15th February 1876.

PRESENT:

GEORGE DUNCAN, Esq., IN THE CHAIR.

FREDERICK AUGUSTUS ABEL, Esq., F.R.S.
RICHARD CORY, Esq.
JOHN FENWICK, Esq.
CHARLES ICELY, Esq.
WILLIAM YOUNG, Esq.
J. M. CARMICHAEL, Esq., Secretary.

MR. WILLIAM JACOB PRIM examined.

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2862. (*Chairman.*) You are resident engineer to the Houses of Parliament?—Yes.

2863. And you were formerly chief assistant to the Manchester Steam Users Association?—Yes.

2864. How many years were you there?—About eight years.

2865. And that was about the year 1865?—Yes.

2866. Was that when you left the Association?—No, I left in 1868. I have been at the Houses of Parliament since 1868.

2867. And I believe you became acquainted with a patent for extinguishing fire by means of steam while you were at Manchester?—Yes; patented by a Mr. Robert Hollingdrake for mill purposes.

2868. What was he?—An ironfounder in Stockport.

2869. And can you tell us anything about the successful application of this patent?—It answered the purpose to a certain extent, but it gave a good deal of trouble from the fact that the steam spread about so much that it rusted the machinery; so that if there was a fire in one corner of the room all the machinery throughout the room was rusted; and consequently I believe it fell into disuse. Another difficulty, and a serious one in my estimation, was that in a large room where there was a very large area of cooling surface the steam condensed very quickly, I consider that in any place where there is a large number of rooms, or a large cooling surface such as skylights or open iron roofs, that would be fatal to the system, because the steam would condense readily and not have any effect on the fire.

2870. And I suppose if the windows were knocked in, it would have the same effect in that respect?—Yes, or if there were staircases open that would convey the steam away.

2871. You are not able to tell us of this invention having been used for any length of time successfully?—I cannot say that it has been used successfully. I have lost sight of the matter for some years; but it struck me that it might be used successfully on board ship where the hatchways can be battened down. In a confined space it might be used successfully.

2872. Then how would you propose to fit a vessel with it?—By running pipes in various places.

2873. With holes perforated in them?—Yes, and commanding cocks, so that steam would only be turned into such places as was necessary.

2874. I suppose you would hardly expect that the steam would permeate through a large stock of coals very far, particularly if there were much small among them?—No; but if the pipes were carried in various directions I am of opinion that the fire might be held in subjection for a considerable time, instead of breaking out and destroying the whole cargo.

2875. I see that you suggest our examining the patentee?—I thought that he might give more information about it, and might tell you where the difficulties were in applying it.

2876. I suppose you are aware that there are a good many patentees that have ideas about putting fires out ?—I am aware of that.

2877. You have no particular knowledge of your own in reference to the application of steam in putting out fire ?—Excepting on a very small scale. I have tried it on a small scale and found it very successful. In fact the small scale on which I tried it was with a kettle and a candle and also a gas burner. I found it very successful when applied to a strong flame of gas. I have also tried it in a chamber where the steam was at a very low pressure so that I could bear my hand on the windows, and successfully put the fire out. This chamber was 16 feet long by 6 feet wide, and 10 feet high.

2878. You have never had any risk of the Houses of Parliament being burnt down from spontaneous combustion in your stock of coals, and used this means of putting the fire out ?—No.

2879. Or have you on any other occasion dealt with fire in masses of coal, putting it out, whether by steam or otherwise ?—No.

2880. (*Mr. Cory.*) I suppose the steam would require to be of some considerable pressure in order to produce an effect on fire ?—No.

2881. Supposing there was a fire in this room, and it was filled with steam, would it have any effect on the fire ?—Yes, I think that at a very low pressure it would answer.

2882. Such a pressure for instance as would not break the windows ?—Yes ; the pressure of the steam was scarcely anything above the atmosphere.

2883. (*Mr. Icely.*) It would not be hermetically sealed ?—No. Of course steam would escape through the keyholes and various chinks.

2884. (*Mr. Abel.*) The arrangement which you have referred to, the system of perforated pipes, seems to be rather an obvious mode of applying the steam. Could you point out any special feature in this particular patent ?—As it is necessary in cotton mills to keep the temperature pretty high so as to prevent the oil in very fine machinery from clogging, heating by steam pipes running all round the rooms is adopted, and the invention consisted in making those pipes with a diaphragm through the centre, so that one half acted as the ordinary steam or heating pipe, and the other half was perforated so that at any time steam could be turned into this perforated pipe and diffused through the room.

2885. The invention was chiefly applicable to cotton mills, or factories of that kind ?—Yes, where steam was already in existence.

2886. Probably a much more simple arrangement would apply the steam in the hold of a vessel ?—Yes, much simpler.

2887. (*Mr. Young.*) Have you ever reflected, with regard to this invention as applied to coal-laden ships, on the cost and trouble that it would give ?—Very much would depend upon the construction of the vessel and the nature of the bulkheads. If you get the steam into small spaces it would be very much simpler than having one large hold. I should consider that in a steam collier, where boilers are constantly under steam, the cost of fitting up an apparatus of this description would be, perhaps, such a matter as 5*s*. per ton burthen.

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2888. Unfortunately from all we have seen and heard it is not on board steam colliers and in short voyages that the danger is apprehended, but on board large vessels and vessels going on long voyages, and not merely destined for coal, but going for the purpose of bringing home other cargoes ?—It that case it would be necessary to have a boiler on board ; that boiler could be used for cooking and general work on board.

2889. With a donkey-engine, you mean ?—Yes ; for working capstans and hoisting in and out cargo, &c.

2890. (*Mr. Fenwick.*) Have you any proposition to make as regards the fitting of a coal-laden vessel with regard to these pipes of which you have spoken ; do you propose running them through the centre of the ship or the sides ?—I would keep them as low as possible (as the steam will naturally rise) in the bilges, and in places where they would be accessible when the cargo was out, and in some cases, if possible, accessible when the cargo was in ; and I would suggest having cocks so that steam could be turned on in sections. If it was known where the fire was located then the nearest section to that could be turned on.

2891. By means of pipes leading up to the deck ?—Not necessarily to the deck ; to places where they would be accessible, to such parts of the ship where a person could get down to the bottom without going down through the cargo.

2892. (*Chairman.*) When a vessel is loaded with coals she is nearly as full as she can hold, you would therefore have to make tunnels to get down to the coals ?—In some cases there are portions of the vessel running very fine aft and forward where there are no coals.

2893. As a rule they are filled up within a foot or two of the deck all over the body of the ship. You would see that there would be considerable expense if you were to make tunnels to get to the cocks ?—Decidedly, if it were necessary.

2894. (*Mr. Icely.*) There would be no difficulty in fitting the cocks with spanners leading to the upper deck, would there ?—Not the least.

2895. (*Chairman.*) You would see the difficulty there would be in preventing the coal dust from getting into your pipes ; you would have to cover them with something in order to prevent that ?—Yes. Some provision should be made in the nature of a shield, so that the steam would have to pass some short distance before it came into contact with the cargo.

2896. You would have a wooden box and holes in the sides of it, and the top covered, I suppose ?—Yes, something of that sort. With a properly constructed shield any little dust that there was would be blown away, so long as it was not forced or pressed into the orifice of the steam pipes. The pressure of steam would be sufficient to blow that away.

2897. Have you anything else to say on this subject ?—I would observe that I consider that a hose could not be worked very well with steam in it, even if you had a hose on deck. A leather or canvass hose could not be worked advantageously with steam, because if there were the least hole in the hose the steam would escape rapidly ; and in binding up the hose steam would not be so easily dealt with as water.

2898. Have you anything further to add to your evidence ?—I think not.

The witness withdrew.

Adjourned.

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Is employed at Portsmouth; was formerly inspector of coal for the Admiralty in Northumberland and Durham district, 973–976.
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Tilleary, 1451.
Townhill, *App.* pp. 87, 92, 103; Q. 83, 84, 555.
Tredegar, *App.* p. 115.

Coals—*Classes—continued.*
Tron, *App.* p. 82.
Trycher, *App.* p. 107.
Tryddyn, *App.* p. 105; Q. 77.
Tylacoch, *App.* p. 115.
Tyne, *App.* pp. 75, 78, 103; Q. 64, 1184.
Tyne Fine Merthyr, 596.
Tyr Edmond, 920, 2244, 2248, 2249.
Tyrfilkin, 1482.
Wallsend, 85.
Watson's Hartley, *App.* pp. 75, 78, 104, 107, 138; Q. 83, 213, 248.
Wayne's Merthyr, *App.* p. 100.
Wear, *App.* p. 115.
Welsh, *App.* pp. 74, 77, 78, 87, 103; Q. 47, 2149, 2164.
West Hartley, *App.* pp. 6–9, 66, 69–71, 76, 77, 83, 86, 93–98, 103–105, 107, 116, 117; Q. 85, 1638, 2585.
Westminster, *App.* p. 77.
Westminster Brymbo, *App.* pp. 86, 98, 103, 105, 115, 131; Q. 77. See *Brymbo.*
West Riding Hartley, *App.* p. 138.
West Yorkshire, 85.
White Moss, *App.* pp. 70, 96, 105.
Wigan, *App.* p. 74.
Wilkesbarre, *App.* p. 115.
Wills' Merthyr, *App.* p. 115.
Wishaw, *App.* pp. 69, 75, 88, 89, 92, 103; Q. 83, 2770.
Wishaw Ell, 83.
Woodside Field, 83.
Wrexham, *App.* p. 115.
Wyndham Merthyr, *App.* p. 115.
Yorkshire, *App.* pp. 91, 103; Q. 46.
Yorkshire Hartley, 85.

Commended in Evidence.
Cowpen Hartley, 634, 717, 832.
Cyfarthfa, 2025.
Dowlais, 2020.
Elled Vein, 2013.
Monmouthshire, 1450.
Morfa, 2260.
Orrell, 52.
South Wales Steam, 1432.
Swansea, 4-feet and 6-feet veins, 2182.
West Hartley Main, 1642.

Mentioned in Evidence as Dangerous.
Avon Vale, 2347.
Barnes's, 2442, 2477.
Bodor, 2193.
Cistern, 2183, 2193, 2311.
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Fothergill's Aberdare, 2027.
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Lancashire (some), 51, 2441, 2477.
Monmouthshire, 2012.
North Wales, 2108.
Parson's Abbey Graigola, 919, 2244.
Rhondda (Nos. 1, 2, 3), 2010, 2196, 2251, 2348.
Slatog, 2193.
South Wales (some), 2004, 2108, 2492.
Swansea, 1565, 2182.
Town Hill, 563.
Tyne, 1188.
Tyr Edmond, 919, 2244.

Coal Cargoes.
Paper respecting, *App.* p. 139.
See *Shipment of Coal, &c.*

Coal Mines Regulation Act.
Made one source of cheap labour less available, 137.

Coal-shippers.
Information afforded to Commission by, *App.* p. 82.

Coal-shipping Ports.
Visit of Commission to, *Rep.* p. x.
See *Shipment of Coal.*

Coal Ships.
See *Ships carrying Coal.*

Coal Stores on Shore.
Admiralty papers respecting, *App.* p. 60.
Papers and statistics relating to, *App.* p. 115.
Spontaneous combustion of, *App.* p. 63; Q. 620, 654, 663, 680, 690, 853, 897, 900, 917, 966, 979, 1005, 1100, 1105, 1532, 1980, 2175, 2297, 2604, 2739, 2777.

Coal Trade, Export.
General condition of, *Rep.* p. vi.

Conclusions of the Commission.
Summary of, *Rep.* p. xxiv.

Correspondence.
Between Board of Trade, Lloyd's, and London Salvage Association, *App.* p. 1.
Relative to spontaneous combustion in the "Persia," *App.* p. 25.
Relating to spontaneous combustion of coal, *App.* p. 32, 109.

Covey, Mr. William. (*Analysis of his Evidence.*)
Is Chief Inspector of Machinery, and has charge of the steam reserve, 1097, 1099.
Storage of coal ashore at Devonport; some is housed, and some is not, 1100–1103, 1129–1134.
Has never seen any case of heating of coal, 1105, 1107, 1128.
Approval of ventilation of coal cargoes; there should be ventilators in the bunkers, 1110–1127.
Case of explosion of gas in the "Agincourt" and the "Devastation;" if the bunker-lids had been taken off every day explosion would not have occurred, 1112–1127.
An order has been given to the whole service that coal-boxes are to be ventilated, 1119–1123.
Has never known cases of explosion in wooden ships, where there is more likely to be admission of air, 1135.

Crane for Loading Coals.
See *Shipment of Coal.*

D.

Dangerous Coal.
See *Coal; Coals—Classes.*

Darley, Mr. William.
On prevention of gas explosions, *App.* p. 53.

——— (*Analysis of his Evidence.*)
Was formerly foreman of the yard at Chatham, and has been lately Nautical Assessor under Board of Trade, 2797, 2798.
Has had experience of cases of explosion and combustion in ships; cites instances, 2799–2804, 2850–2852.
Spontaneous combustion of coal; it arises from shipping "green" and wet coal; coals should be exposed after being taken from pit, to allow escape of gas; has not known a case in a vessel belonging to the Navy, 2805–2817, 2827, 2832, 2833, 2853–2861.
Moisture is the cause of spontaneous combustion of coal, 2815.
Coal should be put into a dry ship, 2818, 2819.
Ventilation of coal cargoes; ventilation on surface cannot do harm; only useful for coal containing no pyrites; some coal must be ventilated; approves of Royal Navy system, 2818–2831, 2842–2849.
Cardiff coal; its danger of explosion, not of spontaneous combustion, 2829, 2834–2838, 2843.
Different kinds of coal require different courses of action, 2837.
Navy engineers test their coal cargoes with the thermometer; immunity from accident attributable to this, and to their system of ventilation, 2860, 2861.

Davis, Mr. Arthur Owen. (*Analysis of his Evidence.*)
Attends from Peninsular and Oriental Company, 2580.
States number of shipments, and classes of coal shipped for Company in 1874, 2581–2585.
In 1874, had four ships burned, three of them loaded with Howard's West Hartley; and one missing, loaded with Scott's Brymbo; coal was mostly shipped in wet weather, 2586–2600, 2626.
The Company has inspectors to see to shipping of coal, 2597.
The Company prescribes no conditions as to ventilation; their inspector at Newcastle objects to it, 2601–2603, 2611, 2615–2617, 2626.
Reads memorandum relative to spontaneous combustion of coal on shore, and in bunkers of the Company's steamers, 2604.
Coal stacks of the Company; mostly covered; firing very rare; instances cited, 2604–2606.

Davis, Mr. A. O. (Evidence of)—*continued.*
Three or four cases of explosion in bunkers of Company's steamers, owing to "close" covers being kept on too long, 2604.
South Wales coal less liable to spontaneous combustion than Newcastle or North Wales coal, 2604.
Larger per-centage of casualties in 1874 than in any previous year, 2607–2610.
The Company insures its coal; rates have risen, 2612–2614, 2618–2623.
Has never compared Company's casualties with others, 2624, 2625.
Allusion to the "Corah," which went safely, unventilated, 2626–2629.

Derry, Mr. Samuel E. (*Analysis of his Evidence.*)
Was formerly inspector of coal shipped for the Admiralty in South Wales; is now a retired chief engineer in the Navy, 891–893.
Has known three cases of spontaneous combustion, all with Parson's Abbey Graigola coal; they occurred at Jamaica and Barbadoes; states particulars, 894.
Parson's Abbey Graigola coal; it is a smokeless coal from Llansamlet; iron pyrites is so intimately mixed with it as to make it liable to catch fire; it is from one particular vein (the Tyr Edmond) that the dangerous coal comes; it was simply inspected, before being ordered for the Admiralty; witness ceased to ship it after the casualties; 895–925, 936–940, 950–957.
Iron pyrites in coal; if localized in large masses, it is less liable to take fire than where it is intimately mixed; where much mixed, either damp or friction will cause spontaneous ignition, 905, 908, 923, 942–949, 962–965.
Coal should be free from small at the time of shipment; small coal assists spontaneous combustion, but it is not generally liable to fire, 909, 941, 942, 966–972.
Has never known a case of good South Wales coal catching fire spontaneously in the main body of the coal, 923, 928, 929.
Has never known a case of spontaneous combustion in patent fuel, 926, 927.
Has known cases of explosions of gas in vessels; this occurs when the hatches are put on before the cargo has given off its gas; ventilation is absolutely necessary, 929–935.
Coal was sent for trial before being admitted for Admiralty use, but the trial might have been fallacious, 953–957.
Coal shipped wet would be liable to spontaneous combustion, 958–961.

Devonport.
Storage of coal at, 1100.

Drops for Loading Coal.
See *Shipment of Coal.*

Durham.
Combustion of coal-stores at, 2739, 2777.

E.

Equator.
Analysis of results in coal-laden ships crossing the, 36–147.

Explosions of Coal in Ships.
No reliable statistics concerning, *Rep.* p. xxiii.
Surface ventilation requisite for guarding against, *Rep.* p. xxv.
Owing to marsh gas, *Rep.* p. xxviii.
Board of Trade inquiries respecting, *App.* p. 41; *Q.* 2800.
Papers from Admiralty relating to, *App.* 60.
On the Tyne, 444.
In vessels of the Navy, 581, 605, 1112.
Of South Wales coal, 929, 1942, 2492, 2829.
Not common in wooden ships, 1133.
At Cardiff, *App.* p. 59; *Q.* 1491.
In the Northumberland Dock, 1658.
Explosion mistaken for spontaneous combustion, 1959, 2036.
Of Londonderry gas coal, 2091, 2129.
At Birkenhead, 2453.
In bunkers of steamers, 2604.
Ventilation is only useful against, 2741.

Export of Coal.
From chief British ports, in 1873–75, *Rep.* p. vii.

F.

Farrer, Mr. Thomas Henry. (*Analysis of his Evidence.*)
Is Permanent Secretary to the Board of Trade, 14.
This Commission was instituted because Board of Trade's inquiries (under Merchant Shipping Act) into cases of burning of coal-laden ships failed in regard to scientific and complicated cases. Lloyd's Committee and the Salvage Association also desired inquiry by a special body, 16, 17.
Puts in papers respecting the "Herbert Grahame" and "Minnie Grahame," Abstract of Wreck Register, and two Returns, 18–25.
Board of Trade takes cognizance only of British ships, 27–30.

Fawcus, Mr. G.
On prevention of explosions in coal ships, *App.* p. 32.

Ferguson, Professor.
Chemical analysis of coal by, *App.* p. 109.

Finlay, Mr. James. (*Analysis of his Evidence.*)
Is master of a ship, and has been constantly carrying coals, 472–474.
Describes case of combustion in the ship "Alfred;" the coals were from the Tyne; they became heated when within a few days' sail of Valparaiso; the ship was lost; there was no ventilation whatever, 475–486, 508–514, 539–542.
'Tween decks a great help to a ship's ventilation, 486.
Has carried coal for 20 years in ships that were ventilated; they had wooden ventilators running fore and aft through the coal in the lower hold; attributes freedom from casualties to proper ventilation, 487–507, 515–536.
Great diversity of opinion exists about ventilation; the majority are in favour of it, 537. 538.
By applying above system of ventilation, by visiting the hold every day, and keeping off the hatches, coal may be safely carried, 543–545.

Fire in Coal-laden Ships.
Water and steam for extinguishing, recommended by Commission. *Rep.* p. xxiv.
Report on, *App.* p. 32.
Use of water for extinguishing, 761.
Use of carbonic acid, 761, 763, 1424, 1469, 1817, 1823, 1843, 1901, 2743, 2772.
Use of steam, 1933, 2867.

G.

Galloway, Captain John. (*Analysis of his Evidence.*)
Has been master of merchant ships, and used to carrying coal (mostly Scotch coal) for about 20 years, 2350, 2351, 2378–2381, 2384–2392, 2409–2411.
Has known but one case of spontaneous combustion of coal on board ship, 2352–2396, 2408.
Case of heating on board the "Malabar;" loaded with Langley Mill coal, in a wet condition; fired when 36 days out; ship was ventilated; totally lost; casualty owing to coal being damp. 2352–2378, 2381, 2382, 2398–2407, 2411, 2417–2430.
Approves of ventilation of coal cargoes; would have a platform, with a current of air underneath, 2382, 2383, 2386–2389. 2411–2416.
Casualties are increased now by coal being taken direct from pits to ships, the gas not having time to escape; also by vessels being larger, and carrying greater masses of coal than formerly, 2386–2391, 2397.
Witness had carried coal for eight or ten years without ventilating his ship, and had no casualty, 2386–2389, 2395, 2396.
Instances of Scotch coal firing, 2392–2394.
Exposure of coal to the air before shipment is an element of safety, 2428.

Gases.
Occlusion of, in coal, *Rep.* p. xxvii.
Marsh gas the source of explosions, *Rep.* p. xxviii.

Gases in Coal Cargoes.
Paper on, *App.* p. 31.
Prevention of, *App.* p. 53, 60.
See *Explosions of Coal in Ships.*

Gasworks.
Combustion of coal in, *App.* p. 109–112.

Glasbrook, Mr. John. (*Analysis of his Evidence.*)
Is a colliery owner and mining engineer at Swansea; has shipped coal for upwards of 30 years, 2179, 2180, 2345.
Swansea coals; only two are not liable to spontaneous ignition, viz., the 4-feet and the 6-feet vein; some will take fire on the wharf; some may go safely a short distance; objection to seams above Hughes's, and to the Cistern, Slatog, Bodor, Rhondda coals, &c., 2181–2186, 2189–2196, 2199, 2210, 2211, 2224–2239, 2244–2249, 2251, 2259–2262, 2285–2336, 2345–2348.
Has had one case only of spontaneous combustion—the "Alpha," 2187, 2188.
Mixing coals causes spontaneous combustion, 2190, 2244–2249, 2263–2272, 2285, 2286, 2335.
Spontaneous combustion of coal arises from its impurities, as sulphur and phosphorus, shale, earthy matter, pyrites, and "mother-of-coal," 2193–2197, 2303–2310.
Casualties occur through men not working the coal clean; desirable if they could be compelled to do so, 2197, 2198, 2200–2204. 2222, 2223, 2335.
While coal was high-priced inferior qualities were shipped; this was a cause of firing, 2201–2204, 2334.
Coal cargoes should be separated, and kept in compartments; this would be better than ventilation; a hole made in the cargo might be good, 2205–2209, 2277–2282, 2337–2344, 2349.
Coal should be shipped dry, 2209.
Shipping of coal in boxes, by steam crane, is a good mode, and does not increase cost, 2212–2222, 2254–2258.
To avoid casualties, ship coal not liable to ignite; do not ventilate; divide ship into compartments; and lower coal in boxes, 2218–2223, 2250.
Has never known patent fuel to take fire, 2240–2243.
Abbey Graigola coal takes fire if mixed with Tyr Edmond, 2244–2249.
Several cases of coal cargoes igniting when carrying Rhondda coal, 2251, 2252, 2259–2262.
Steam coal travels safely, 2253, 2327, 2328.
Coal is not liable to get mixed in working, unless it is done on purpose, 2263–2272.
Witness does not insure his ships; belongs to an insurance fund, 2273–2276. 2290, 2294.
Desirable if temperature of coal cargoes were tested by a thermometer, 2282–2284.

Glover, Mr. John. (*Analysis of his Evidence.*)
Is a chemical manufacturer at Newcastle, and a member of the Chamber of Commerce of Newcastle, 1807, 1808.
Ventilation of cargoes of north country coal; it should rather be avoided than sought for; the surface of the coals should be ventilated; iron gas pipes might be inserted in the coal for ascertaining the temperature, 1810–1814, 1817, 1821–1823, 1870–1873.
Witness's experience is confined to north country coal, 1812–1814.
Spontaneous combustion of coal; chiefly owing to its containing bisulphide of iron (in form of brasses or pyrites) with moisture, and also bituminous shale; and to the presence of finely-divided coal; pyrites finely divided favours combustion, 1815–1820, 1827–1842, 1847–1868, 1884–1900, 1923–1926.
Temperature of coal cargoes should be examined every day with a thermometer, and entered in a log, 1817.
Coal may take fire at very low temperature; some coals more likely to fire than others, 1817, 1824–1826.
No one mode of loading coal is better than another as respects combustion, 1818–1820.
Spontaneous ignition of coal is only a question of time, 1818, 1824.
Application of carbonic acid gas to heated coal cargoes; apparatus described, 1817, 1823, 1843–1846, 1865–1867, 1901–1922, 1927–1932.
Cannel coal finely divided would be likely to take fire, 1862–1864.
Moisture is one of the main conditions of the firing of coal; coals shipped wet might perhaps have a current of hot air injected through them, 1869–1876, 1916, 1935, 1936.
North of England small coal is dirtier than formerly, 1879–1883.
Heaps of coal must be large (perhaps 200 to 500 tons) to ignite spontaneously, 1886–1891.
Objection to application of steam for extinguishing fire, 1933, 1934.

Goose-neck Drop.
See *Shipment of Coal.*

Graham, Professor.
Allusion to his report on the firing of the "Amazon," 1817, 1934.

Great Western Railway.
Box system of loading coal used by, 798.

"Green" Coal.
Danger from, 2805.

Greenough, Mr. W. W.
On coal used by Boston Gaslight Company, *App.* p. 114.

H.

Hancock, Mr. Robert John. (*Analysis of his Evidence.*)
Is shipping manager to Messrs. Charles Hill and Sons, of Bristol and Cardiff, 1309.
Has shipped about 75,000 tons of coal (both steam and smelting coal) in the last ten years, 1310–1312, 1343, 1350.
Has never had a case of explosion, or of actual spontaneous combustion, 1313.
Has had one case of coals becoming heated, in the ship "Her Royal Highness;" heating resulted from the coals being placed on the bottom of the ship, and from their having got wet on shore, 1313, 1314, 1324–1330, 1344, 1345, 1354–1358.
Carries his coal on a platform 16 to 20 inches from bottom of ship; this promotes coolness and dryness; uses ventilators fore and aft, and in each hatchway; platforms are used by guano vessels in Callao trade, 1315–1319, 1337–1340, 1346.
Wet is a cause of spontaneous combustion, 1315, 1341, 1351, 1352.
Shipping coal; danger would be lessened by taking only large coal on board at first, and putting large coal in each hatchway; has generally shipped by tips, but sometimes by box-drops, 1320–1323, 1329, 1331–1336, 1353.
Understands iron pyrites to be the cause of spontaneous combustion of coal, 1327.
His captains are instructed, in a general way, to watch heating of cargoes by thermometer, and look to the ventilators, 1347–1349, 1359.
Has given particular attention lately to the subject of spontaneous combustion of coal in ships; the Chamber of Commerce of Cardiff has taken up the subject, 1359–1368.
Has found the per-centage of loss by spontaneous combustion of coal in ships to be not over one-half per cent. over the whole kingdom, 1367, 1368.
Ventilation could not be dangerous to coal, but after a fire occurred it would tend to feed it, 1369.
Pays very little extra (not more than one per cent.) for insurance now than formerly on coal cargoes, 1370–1380.

Harwood, Mr. James. (*Analysis of his Evidence.*)
Is employed at Portsmouth, 974.
Had one case of heating at a coal-store in Jamaica; the coal had got wet; it was Parson's Abbey Graigola coal; had it taken out, and, when replaced, put iron tubes into it to carry off the gas, 1004–1012.

Hatchways of Coal Ships.
Fire chiefly occurs there; should be filled with large, not small, coal, 1228, 1283, 1320–1323, 1329–1336, 1353, 2124–2128, 2498, 2556, 2561.

Hill, Mr.
Statistics by, relating to casualties to coal ships, 1365.

Hoist for Loading Coals.
See *Shipment of Coal.*

Hollingdrake, Mr. R.
His method of extinguishing fire by steam, 2867.

Hosier, Captain Henry M. (*Analysis of his Evidence.*)
Is Secretary to Lloyd's, 1.
Puts in lists of coal-laden vessels lost or damaged, and of missing coal-laden vessels; and a letter from the coal inspector of the Peninsular and Oriental Company to the Committee of Lloyd's, *App.* p. 104–108; Q. 4–13.

Howe, Mr. E.
On spontaneous combustion in coal-stores, *App.* p. 113.

I.

Icely, Mr. C.
On combustion of coal in Plymouth Gasworks, *App.* p. 109.

Inman, Dr.
On spontaneous combustion, *App.* p. 26.

Inspectors of Mines.
Should inquire into cases of spontaneous combustion of coal, *Rep.* p. ix., xxv.

Insurance of Coal Ships.
Statistics prepared for Underwriters' Association of Liverpool, 32.
Premiums; rise of, &c., 154, 1244–55, 1270–1274, 1294–1308, 1370–1380, 1441–1449, 1483, 1484, 2052, 2097, 2273–2276, 2290, 2294, 2612, 2618, 2724.
Shippers' insurance fund, 2273.

Iron Pyrites in Coal.
See Coal.

Ironworks' Collieries.
Coal from, liable to spontaneous combustion, 2015.

J.

Jamaica.
Spontaneous combustion of coal-store at, *App.* p. 63, 64; Q. 620, 917, 979, 1004.

Jettisoned.
Heated coal cargoes jettisoned, 105.

Jeula, Mr. Henry. (*Analysis of his Evidence.*)
Is an underwriting member of Lloyd's; was formerly Honorary Secretary to the Statistical Committee of Lloyd's, 2, 3.
Puts in paper of suggestions for inquiry, *App.* p. 140: Q. 12.

K.

Kennedy, Captain William. (*Analysis of his Evidence.*)
Has commanded ships, and had experience in carrying coal, for 18 years, 2533–2536.
Has known one case of spontaneous combustion, 2537.
Instances of voyages without casualty made by witness in coal-carrying ships not ventilated, 2538, 2539, 2554.
Case of burning of "L'Aquohama;" loaded by tip; coal much broken; ventilated; totally lost; due to small coal lying under main hatchway, 2539–2545, 2571–2574.
Doubts practical worth of ventilation through coal cargoes; his owners approve of it; surface ventilation desirable, 2546–2554, 2565, 2566.
Salt water will not make coal take fire; fresh water promotes heat in coal; effect of water on cotton, 2551, 2575–2579.
Great danger arises from small coal in the hatchways; due to shipping by tip, 2555–2562.
Instances four ships firing in the hatchways after being loaded under the tip, 2556.
Registers temperature of cargo in log-book; has always experienced heat at the hatchway, even when cargo has been carried safely, 2568–2570.

Keyham.
Combustion of coal-store at, 900.

L.

Legislation.
Statutory inquiries respecting burnt coal ships, *Rep.* p. ix.; Q. 16.
Suggested only in regard to inquiry by inspectors of mines and as to specification of coal entered outwards, *Rep.* p. xxv.
Desirable for inspection of coal ships, 424, 1438.
For causing temperatures of coal cargoes to be taken, 1797.
For making men work coal clean, 2222.
For enforcing ventilation of coal ships, 2713.

Lignite.
Casualties in shipment of, from Marseilles, 106.

Limerick.
Combustion of coal-store at, 2175.

Lists of Coals.
Mr. Rundell's, *App.* p. 127, 130–138; *Q.* 2442, 2527.
Brown's, 100.
Admiralty's, 2164.
See *Statistics of Coal.*

Liverpool.
Statistics of coal vessels sailing from, *App.* p. 127.

Liverpool Salvage Association.
Report by, on ventilation of coal ships, *App.* p. 32.

Lloyd's.
Correspondence between, and Board of Trade and London Salvage Association, *Rep.* p. v.; *App.* p. 1; *Q.* 16.
Return of coal shipments by, *Rep.* p. vi.
Statistics relating to coal shipments in 1874 from, *App.* p. 101.
Lost or missing coal-laden vessels posted at, 4–12.
Letter to, from coal inspector of Peninsular and Oriental Company, 13.
Desired [further inquiry into cases of burnt coal-laden ships, 16, 21.
See *Tables.*

Loading of Coal.
See *Shipment of Coal.*

Local Marine Boards.
Correspondence of Board of Trade with, arising out of the case of the "Persia," *App.* p. 25. From—
Abergavenny, *App.* p. 33.
Cork, *App.* p. 26.
Dundee, *App.* p. 29.
Glasgow, *App.* 32.
Greenock, *App.* p. 31.
Leith, *App.* p. 29.
Liverpool, *App.* p. 26.
London, *App.* p. 26.
Newcastle, *App.* p. 30.
North Shields, *App.* p. 32.
South Shields, *App.* p. 31.
Sunderland, *App.* p. 28.
West Hartlepool, *App.* p. 33.

London Salvage Association.
Correspondence of Board of Trade, &c. with, *Rep.* p. v.; *App.* p. 1; *Q.* 16, 21.

M.

Madras Railway.
Ignition of coal shipped for, 700.

Marine Boards.
See *Local Marine Boards.*

Marseilles.
Shipment of coal from, 106.

Marsh Gas.
The source of explosions, *Rep.* p. xxviii.

Masters of Vessels.
Information afforded by, *App.* p. 76, 85.

Mather, Mr. J.
On gases in coal cargoes, *App.* p. 31.

Mauritius Marine Board.
Report of, on loss of the "Persia," *App.* p. 25.

McConnochie, Mr. J.
On systems of coal-shipping in South Wales, *Rep.* p. x.

McCulloch, Mr. Henry Johnson. (*Analysis of his Evidence.*)
Is Consulting Coal-engineer to the Navy, and has been connected with the coal trade more than 30 years, 696–699.
States particulars of three cases of spontaneous combustion, one with coals shipped for the Admiralty, and two with coals shipped for Madras Railway; the first casualty was owing to something external to the coal, and the others to the coals having become wet, 700–724, 848–852, 857–866, 879.
Cowpen Hartley coal; it is very free from sulphur, and one of the best coals on the Tyne, 713–717, 832, 833, 848.
Casualties in the "Numea" and "Palmyra," 713–724.
Iron pyrites in coal; all coal contains it, 717, 748, 818–833, 839–842, 865, 866.

McCulloch, Mr. H. J. (Evidence of)—*continued.*
Per-centage of casualties with which witness is acquainted is very small, 726.
Casualties in the "Rattlesnake," "Racoon," and "Megæra" were cases of heating from external causes, 727–730.
Methods of shipping coal; the best is that on the Tyne, and at Blyth and Sunderland, 731–742.
Coal should not be broken in shipping; the larger the coals the greater their value, and the less danger there is from fire, 735, 741.
Methods of transporting coal from pit to place of shipment; waggons, swing-drops, &c.; the larger the waggon the worse for the coal, 743–747, 763–817, 873.
Spontaneous combustion of coal (except anthracite) is caused by moisture, 748–750, 827, 867, 870, 872.
The increased number of cases of spontaneous combustion is owing to increase in exports, and to carelessness in shipping and ventilating coal, 751–758, 855, 856, 890.
Thorough ventilation would save coal-laden ships from danger; ventilator described, 759, 760, 834–838, 874–878, 883–890.
For extinguishing fire in coal-laden ships, water is more likely to be useful than the extincteur, 761, 762.
Objection to the system of double-screening of coal, 790, 791.
North of England system of shipping coals should be introduced into South Wales, 812.
Careful inspection of coal at the ports; comparative safety is owing to this, 843–847.
Has known many cases of spontaneous combustion of coals stored on shore; more than afloat, 853, 854.
Every coal (except anthracite) is liable to spontaneous combustion, 869.
There is not sufficient water in coal internally to make it liable to spontaneous combustion, 872.
Coals lose but little of their combustive property in the course of a voyage, 880–882.

Merchant Shipping Acts.
Unsatisfactory inquiries under, respecting burnt coal-ships, 16.

Mersey.
Letter, &c. respecting sailings of coal ships from, *App.* p. 127.

Mersey Docks and Harbour Board.
Papers furnished by, *App.* p. 118.
See *Tables.*

Messent, Mr. P.
Sketches of coal-shipping on the Tyne sent by, *Rep.* p. xv.

Mines' Regulation Act.
Has caused removal of boys who cleaned coal, *Rep.* p. ix.; *Q.* 1710, 1737.

Mixture of Coals.
In relation to spontaneous combustion, 641, 642, 660, 661, 670, 671, 2190, 2244–2249, 2263–2272, 2285, 2286, 2335.
For Navy use, 642, 670.

Moisture in Coal.
See *Coal.*

Montreal.
Spontaneous combustion of coal at, *App.* p. 113.

Moore, Mr. Robert. (*Analysis of his Evidence.*)
Is chief engineer, 1098.
Has never seen any heating of coal; mentions a case of spontaneous combustion in the coal bunkers on board the "Gladiator," 1108, 1109.

N.

Navy.
Instructions respecting carriage of coal in vessels of the, 616, 623.
Cases of heating of coal in ships of, 581, 701.
Approval of system of ventilation adopted by, 2828, 2844.

Newcastle.
Temperature of coals of, *App.* p. 112.

New England Association of Gas Engineers.
Paper read before, *App.* p. 111.

Newton, Mr. John Paul. (*Analysis of his Evidence.*)
- Is commander of a coal-carrying ship; has made voyages as a captain for 22 years, and met with one case of spontaneous combustion, 546–551.
- Describes case of combustion in the vessel "Lochee;" she was loaded in the Tay; the coals were saturated with rain and sea water; they were from Town Hill Colliery, Fifeshire; heating discovered after about three months; when in sight of Galle she blew her decks out from end to end; she had had her hatches off every day, and was well ventilated; only 24 hours before the explosion was anything wrong suspected, 551–556, 567–[illegible].
- Attributes casualty to coals having been shipped wet, 554.
- Has known other cases of Town Hill coal taking fire, and destroying vessels, 558–565.

New York Gas Company.
- Statement from, *App.* p. 112.

Nova Scotia.
- Papers from, respecting spontaneous combustion, *Rep.* p. [illegible].
- Temperature of coals of, *App.* p. [illegible].

O.

Oils, Hydro-carbon.
- In connexion with heating of coal, 1815, 1817.

P.

Paraffin.
- In coal, 1815.

Patent Fuel.
- Spontaneous combustion not liable with, *App.* p. 116; *Q.* 1547, 2240.

Peninsular and Oriental Steam Company.
- Paper and statistics furnished by, *App.* p. 114.
- Letter from coal inspector of, to Lloyd's, 13.
- Gives no instructions respecting ventilation of coal cargoes, 1669, 1946, 2601.
- Particulars as to shipment of coal of, 1786, 1967.
- Insure coal cargoes, 2062, 2612.
- Spontaneous combustion and explosion of coal shipped for, 2580, 2604.
- Inspection of coal under, 2597.
- Spontaneous combustion of coal of, on shore and on sea, 2604.
  - See *Tables.*

Percy, Dr.
- Scientific paper by, relative to spontaneous combustion and explosion, *Rep.* p. vi., xxvi.
- Allusions to his work on fuel, 1815, 1820, 1834, 1925.

Perry, Mr. W. C.
- On causes of spontaneous combustion in coal ships, *App.* p. 33.

Plymouth Gasworks.
- Combustion of coal at, *App.* p. 109.

Poole, Mr. Henry S.
- Letter from, respecting spontaneous combustion of Nova Scotian coals, &c., *App.* p. 110.

Portsmouth.
- Combustion of coal-stores at, [illegible].
- Storage of coal at, 1105.

Premiums on Coal Ships.
- See *Insurance.*

Price, Mr. W. R. (*Analysis of his Evidence.*)
- Is a shipowner in London, 1136, 1242, 1243.
- Has had experience of two cases of spontaneous combustion of coal, in the "Calcutta" (77 days out), and the "Oliver Cromwell" (80 days out), both wooden ships; believes the coal was Howard's West Hartley; it was put on board by tips; it was much broken; it was ventilated by a horizontal shaft near the bottom; both ships were lost; report on the loss, 1137–1168, 1185, 1186, 1191–1196, 1201, 1202, 1222–1224, 1228–1231, 1256–1261, 1264.
- Burning of the "Euxine," an iron ship, loaded at the same time as the above, 1169–1172, 1186–1188, 1197, 1198.

Price, Mr. W. R. (Evidence of)—*continued.*
- Safe passage of the "Cumb." loaded about same time as above, and not ventilated, 1173, 1174, 1199, 12[illegible], 1262–1266.
- Shipping of coal by barrows is the best way; boxes the next; there is less breakage, and more ventilation through the cargo; it takes longer, and costs more, 1175–1183, 1203–1221, 1288–1292.
- Witness has carried principally from Birkenhead; method of shipping coal there is by tips and by barrows; to the character of the coal shipped there may be attributed its immunity from accident, 1178–1185, 1275–1277.
- Numerous cases of burning of vessels from the Tyne; objections of witness to experimenting with Tyne coal, 1188, 1267–1269.
- Burning of the "Berenice," having Tyne coal on board, 1188.
- If witness had to carry coal from the Tyne he would adopt the same mode of ventilation which he has applied to vessels going from Birkenhead, 1184, 1226, 1234, 1235, 1267.
- Objection of Mr. Steinson to through ventilation of coal in ships; it should be above the coal, and the hatches should be kept open when possible, 1202.
- Spontaneous combustion of coal takes place in the fine slack, 1227–1233.
- Has always instructed his captains to keep the hatches off for fear of explosions, 1236.
- Gives instructions now to try the heat of the coals with the thermometer, and record it in the log, 1236–1241.
- High rate of insurance on coal; it prevents its being carried in high-class ships; witness contemplates taking his own insurance risk, 1244–1255, 1270–1274, [illegible].
- Has carried Cardiff coal, and always ventilated it, 1278–1282.
- As an insurance broker, witness has insured ships in which cases of combustion have occurred, 1294–1301, [illegible].
- Danger of heating of small coal in the main hatchway, 1151, 1166, 1228–1233, 1283–1287.

Prim, Mr. William Jacob. (*Analysis of his Evidence.*)
- Is Resident Engineer to the Houses of Parliament, and was formerly Chief Assistant to the Manchester Steam Users' Association, 2862–2866.
- Describes Mr. Robert Hollingdrake's patent for extinguishing fire by means of steam; it might be successful on board ship; the vessel might be fitted with perforated pipes, &c., 2867–2898.

Pyrites in Coal.
- See *Coal.*

Q.

Questions by the Commission.
- Proposed to be put to masters of coal vessels in which casualties have occurred, [illegible].

R.

Resiniferous Shale.
- In Tasmania, *Rep.* p. xxvi.

Richters, M.
- Experiments by, on weathering of coal, *Rep.* p. xxvii.

Roberts, Edmund Squire. (*Analysis of his Evidence.*)
- Is inspector of coal shipments at Cardiff for the Peninsular and Oriental Company; has inspected about 1,000,000 tons of coal and 700 ships, 1937–1941.
- Has had one case of explosion, none of spontaneous combustion, 1942, 1943, [illegible], 2057–2060.
- Best steam coal is not subject to spontaneous combustion, 1944, 1945.
- Objection to through ventilation of coal cargoes; coal stores on land should be ventilated; ventilation should be on the top, [illegible].
- Does not believe in spontaneous combustion, 1959–1963, [illegible].
- Shipment of coal; is particular with regard to avoiding breakage; system described, 1967–1979, 2042–2056, [illegible]–2071.
- Storage of coal at Alexandria; each stack was ventilated by a tunnel through the coal to prevent spontaneous combustion, [illegible].
- Only the best coals should be sent on long voyages, [illegible].

Roberts, E. S. (Evidence of)—*continued.*
South Wales coals; some are dangerous from containing iron pyrites, as No. 2 Rhondda, most Monmouthshire coal, &c., 2004–2039.
Ironworks coal is liable to spontaneous combustion, 2015–2035, 2072.
Has never heard of patent fuel catching fire, 2040, 2041.

Royal Mail Steam Packet Company.
Information furnished by, *App.* p. 114.
See *Tables.*

Rundell, Mr. R. Cooper.
On sailings, &c. of coal ships from the Mersey, *App.* p. 127.
——— (*Analysis of his Evidence.*)
Is Assistant to Secretary of Underwriters' Association of Liverpool, 31.
Has prepared statistics with a view to discover probable cause of spontaneous combustion of coal in ships, and afford means of estimating the increased risk, 32–36, 100-105.
Greater number of fires occur about the 60th day after leaving port; fires have occurred in most of the varieties of coal exported; fires occur in the cargoes whether coal be ventilated or not, and whether loaded in summer or winter, by tip or basket; statistics in illustration of foregoing, 37–57.
Certain amount of moisture present in all coal, 37.
Classification of coal ports, and comparison of sailings and casualties, 46–57.
Cannel is a safe class of coal, 51, 52.
Orrel coal bears a high character, 52.
Crow Orchard coal thought to be liable to spontaneous combustion, 53.
Bituminous coals are avoided by some underwriters; accidents with them, 54, 55.
Casualties in connexion with coal shipped from Lancashire and South Wales ports, 55, 73, 76.
Tonnage of ships (sailing and steamers) in which casualties have occurred; time between sailing of ship and indication of heating, 58–64.
Summer sailings are the most dangerous; most casualties have occurred in September and October, 65–69, 94–99.
Effect of temperature; when and where heating first discovered, 69, 70.
Selected sailings, and number of heatings to number of voyages, 71–73.
Shipment of coal by hand and tip; shipment by tip breaks the coal, and increases the danger; shipment by barrow preferable, 74–76, 107, 108, 114–117, 157–168.
Coal with which casualties have occurred, and ports from whence shipped, 77–86.
Casualties do not show clearly the comparative liability of coal to spontaneous combustion, 87.
Accidents in iron and in wooden vessels, 88–93.
Ventilation of vessels in which casualties have occurred; difficult to say whether a vessel is sufficiently ventilated; certain system recommended by underwriters' surveyors; shutting up coal would conduce to its combustion, 90, 91, 118–120, 141.
Casualties in shipment of lignite from Marseilles, 106.
Casualties, other than burning, occurring to coal-laden ships, are rare, 109–111.
Nothing to show whether double-screened coal would fire sooner than single-screened, 112, 113.
Cases of coal becoming pasty from exposure, while waiting for ships, 120, 121.
Dampness of coal, and spontaneous combustion, 122–125.
Great increase of casualties from combustion of coal afloat in 1874; casualties not on the increase generally; connexion between casualties and increased demand for coal, 126–136.
Casualties proceed from condition in which coal is shipped, 135.
Connexion of casualties and the labour question; increased cost of labour; pitmen not fined now for sending up bad coal, 137–139.
Casualties are to be attributed to what occurs before the coal is shipped, 140, 145.
Coal is carried generally in a better class of vessel than formerly, 142–144.
Comparison of longer and shorter voyages; longer voyages afford a good specimen, 146–153.
Premiums have risen for short, as well as for long voyages, 154, 155.

Rundell, Mr. R. C. (Evidence of)—*continued.*
Coal agents who have a name to keep up are particular as to their shipments, 169.
Allusion to Coal List of, 2442, 2527.
See *Tables.*

S.

Salvage Association.
See *Liverpool Salvage Association; London Salvage Association.*

Sandeman, Captain Alfred P.
On coal cargoes, *App.* p. 139.
——— (*Analysis of his Evidence.*)
Was connected with East India Navy; afterwards Surveyor to London Salvage Association; subsequently Surveyor in Marine Department of Board of Trade, 2630–2637.
Has specially attended to question of ventilation of coal ships; approves of thorough or no ventilation; describes various systems; would ventilate to prevent cargo from getting heated; approves of ventilation being enforced by law; increased freight might have to be charged, 2634, 2641–2654, 2669–2730.
Board of Trade take no action in respect of ventilation, 2636.
Many shipowners object to ventilation, but some are in favour of it, 2647, 2729.
Impossibility of shipping coal thoroughly dry, 2652.
Large amount of pyrites in some coals; this induces combustion, 2654–2658, 2664–2670, 2689, 2690.
Instance of heating in a cargo hermetically sealed, 2658–2663.
Liverpool coal; impossible to ship it thoroughly dry; some sorts heavily charged with pyrites; less pyrites in coal shipped in the Liverpool than on the Birkenhead side, 2652–2657, 2683–2690.

San Francisco.
Casualties in coal-ships bound to, *Rep.* p. vii.

Schist in Coal.
Danger from, 1815.

Scott, Captain Henry F.
On a method of preventing spontaneous combustion of coal cargoes, *App.* p. 78.

Scott, Mr. Edward Benjamin.
Is a partner in the firm of Messrs. Gillespy and Scott; has shipped largely from all steam-coal shipping ports, principally from South Wales, 2074–2080, 2091–2095, 2147–2154.
Spontaneous combustion; danger of smelting coals; instances of steam-coal firing, 2081–2095, 2133–2135, 2138–2140.
Case of heating of coal on board the "Mary Shepherd;" carried Swansea (Resolven) coal; fired after four or five months; coal understood to have been shipped wet; it was ventilated, 2084–2087, 2168, 2170.
Liability to fire of coal shipped wet, 2086, 2177.
Case of the "Robert Watson," loaded with Coed Talon coal, containing much pyrites, 2088–2091, 2137, 2156.
Cases of the "Marlborough," "George Bradford," and "Middlesex," missing; and the "Cassange," burnt; the latter loaded with Parson's Abbey Graigola coal, 2092.
Cases of the "Princess Alexandra," carrying Londonderry gas coal, explosion; and of the "Pocahontas," carrying Hastings Hartley, burnt, 2092–2095, 2107, 2110, 2129–2132, 2154, 2157, 2158.
Insurance of ships; increase of premiums, 2096, 2097, 2116–2120.
Ventilation of coal cargoes; objection to through ventilation; where combustion is set up it would destroy the ship, 2099–2104, 2112–2115, 2121–2123, 2144–2146, 2153, 2160.
High cost of labour may induce careless picking of coal for pyrites, and so increase casualties, 2105, 2106, 2141, 2142, 2152.
Small coal is especially liable to ignition, irrespective of the presence of pyrites, 2107.
Opinion adverse to North Wales coal and South Wales smelting coal, 2108.
Has formed no conclusion as to liability to spontaneous combustion of special classes of north country coal, 2108–2111.
Danger of small coal in the hatchway, 2124–2128.
Best class of steam coal, well cleaned, is not liable to spontaneous ignition, 2143.

Scott, Mr. E. B. (Evidence of)—*continued.*
Shipment of coal; no special approval of any one system, 2161–2167.
Coals on Admiralty list, 2169–2174.
Spontaneous combustion in coal-stores, 2175, 2178.

Screening of Coal.
Disapproval of double screening, 791.

Scurr, Captain John. (*Analysis of his Evidence.*)
Is commander of the "Brenhilda;" describes the heating of coals when between the tropics and the equator; the coals were turned over and cooled; ship had good ventilators, always open; at Calcutta the coal ignited; water was pumped on it; cause of casualty most probably in the coal itself; it was called "Watson's coal;" it was tipped, but little broken, and was dry; the ship might have been as well without ventilators; heating of the coal was probably owing to its being brought too rapidly from the bottom of the colliery into the ship, 170–349.
The "County of Perth" was loading at the same time as the "Brenhilda;" she was loaded by barrow; she took fire near Calcutta, 240–245.
The "Blair Athol," loaded just after, took fire at Bombay, 241, 246–248.

Separation of Coal.
Better than ventilation, 2205.

Sheerness Yard.
Trial of Hirwain coal, &c. at, *App.* p. 62.

Shipment of Coal.
To European, as compared with more distant ports, *Rep.* p. vii.
Shipment of small coal, *Rep.* p. ix.
Systems of loading in relation to breakage, *Rep.* p. ix.
In South Wales, *Rep.* p. x.; Q. 50, 1388, 2080, 2182.
By balance tips (or drops), *Rep.* p. x.; by hydraulic tips, *Rep.* p. xi.; by hand tips, *Rep.* p. xiv.; objection to tips, Q. 76, 739; approval of them, 1418.
Anti-breakage box used in, *Rep.* p. xii.
Box system for, *Rep.* p. xii.; Q. 1331, 2212.
Crane used for, *Rep.* p. xii.
Hydraulic hoist for, *Rep.* p. xiv.
By barrow, *Rep.* p. xiv.; Q. 1180, 1203, 2539.
By spout and drop, *Rep.* p. xv.
By goose-neck drop, *Rep.* p. xviii.
Danger of shipping certain coals, *Rep.* p. xxiv.
Shipments in 1874, *App.* p. 101.
Classification of ports for, 46.
At Liverpool and Birkenhead, *Rep.* p. xii.; *App.* p. 124, 125; Q. 46, 74, 157, 1180, 2434, 2539, 2690.
From Scotch ports, 83, 2767.
In dry weather, 122.
Danger of shipping in haste, 348, 422, 1572.
Navy instructions regarding, 616, 623.
At Cardiff, 703, 1312, 1937.
On the Tyne, at Blyth, Sunderland, &c., *Rep.* p. xv.; Q. 733–817, 1138, 1580, 1647.
Waggons for, 733, 763, 873.
By swing drops, 780.
In casks or barrels, 1423.
Carelessness of officers in regard to, 2484.
At Newport, 1312, 1504.

Shipowners.
Information afforded by, *App.* pp. 66, 85.

Ships carrying Coal.
Report on fires in, *App.* p. 32.
Admiralty papers respecting fires in Government ships and stores, *App.* p. 60.
Statistics of sailings of coal ships, *App.* p. 119–138, 146, 1584–1591.
Lloyd's lists of lost or missing coal-laden ships, *App.* p. 108; Q. 4–12.
Board of Trade takes cognisance only of British ships, 27–30.
Statistics respecting casualties from combustion in, 32.
Class of ships in which casualties have occurred, 58, 88.
Might be built with two or three decks, to separate the coal, 2349.
Steam fittings for extinguishing fire in, 2872.
Case of unventilated ship "Corah" travelling safely, 1173, 1184, 1199, 1262, 1629, 2628, 2717.

Names of Ships.
"Admiral Fitzroy," *App.* pp. 70, 95; Q. 2251.
"Admiral Lyons," *App.* pp. 77, 86.
"Agincourt," 1113.
"Alexander," 564.
"Alexandre," *App.* pp. 70, 94.
"Alfred," *App.* pp. 77, 58.

Ships carrying Coal—*Names of Ships—cont.*
"Alpha," *App.* pp. 69, 90; Q. 2187.
"America," 2554.
"Anna," *App.* pp. 76, 93.
"Annie Laurie," *App.* pp. 67, 87.
"Arracan," *App.* pp. 78, 92.
"Atalanta," *App.* p. 59.
"Aurelie," *App.* p. 33.
"Baringa," 1297.
"Battalion," *App.* p. 99.
"Beacon Light," *App.* p. 9.
"Berenice," *App.* p. 10; Q. 1188, 1596.
"Berwick," *App.* p. 49.
"Beucleuch," *App.* p. 40.
"Bivouac," *App.* pp. 69, 90.
"Blair Athol," 240.
"Brenhilda," *App.* p. 94; Q. 170.
"Broomhaugh," *App.* pp. 71, 99.
"Broughty Castle," 1637, 1663.
"Calcutta, *App.* pp. 69, 94; Q. 1143, 1160, 1191, 1202, 1222, 1228, 1629, 1666, 2587, 2599, 2626, 2717.
"Cambria," 431.
"Cassange," 2092, 2150.
"Cattofield," *App.* p. 4.
"Centaur," *App.* p. 131.
"Cereal," *App.* pp. 68, 90.
"Channel Light," *App.* pp. 69, 91.
"City of Richmond," *App.* pp. 71, 98.
"Clevedon, *App.* p. 36.
"Clothilde," 2251.
"Columbia," *App.* pp. 66, 78, 85.
"Commodore Perry," *App.* p. 8.
"Corah," 1173, 1184, 1199, 1262, 1629, 2628, 2717.
"County of Nairn," *App.* pp. 12, 69, 90.
"County of Perth," *App.* pp. 69, 90; Q. 240.
"Crocodile," 1028.
"Crystal," *App.* pp. 70, 97.
"Daphne," *App.* p. 66; Q. 601.
"Devastation," *App.* p. 65; Q. 605.
"Druid," 431.
"Dumbartonshire," *App.* pp. 70, 95.
"Eeta," *App.* pp. 70, 94.
"Eliza Rulcke," 1594.
"Enthusiast," *App.* pp. 70, 97.
"Ermenigilda Donovara," 2251.
"Ettrick Dale," 1658.
"Euphrates," *App.* p. 60; Q. 583, 631.
"Euxine," *App.* p. 71, 93, 131; Q. 1169, 1186, 1197, 1202, 1622, 1629, 1718, 2587, 2599, 2626, 2717.
"Fanny," *App.* p. 40.
"Fedalma," *App.* p. 98.
"Force," 2092.
"Foundling," *App.* pp. 71, 98.
"George Bradford," 2092.
"George Douthwaite, *App.* 91.
"Gladiator," 1109.
"Glaramara," *App.* pp. 68, 89.
"Glasgow," *App.* pp. 63, 596.
"Glenlyon," *App.* pp. 77, 86.
"Glen Tilt," *App.* pp. 70, 96.
"Glory of the Seas," *App.* p. 131; Q. 60.
"Hannah Law," *App.* pp. 77, 86.
"Henry Bath," *App.* pp. 69, 90.
"Herbert Grahame," *App.* p. 20; Q. 18, 23, 1386, 1404, 1419, 1450, 1480, 2802, 2821, 2830, 2838.
"Her Royal Highness," *App.* pp. 68, 87; Q. 1313.
"Holyrood," *App.* p. 99.
"India," 2538.
"Indian Empire," *App.* pp. 69, 92; Q. 1654.
"Industry," *App.* p. 95.
"Isabella Kerr," *App.* pp. 69, 92.
"Janet Cowan," *App.* p. 88.
"J. H. Lorentzen," *App.* p. 100.
"Joanna," *App.* pp. 67, 87.
"John," *App.* pp. 78, 93; Q. 395.
"Khersonese," *App.* pp. 69, 97.
"Lachee," 552.
"Lady Heathcote," 2251.
"Lady Louisa," 64.
"La Escocesa," *App.* pp. 70, 97.
"L'Agouhanna," *App.* pp. 6, 78, 89.
"L'Aquohama," 2539.
"Le Baron," *App.* p. 40.
"Levant," *App.* p. 98.
"Lochiel," *App.* p. 87.
"Louisa Theresa," *App.* p. 59.
"Maassluis," 64.
"Mabel," *App.* p. 40.
"Malabar," *App.* pp. 63, 77, 89; Q. 591, 600, 2354, 2398, 2421, 2430.

Ships carrying Coal—*Names of Ships—cont.*
"Manfred," 431.
"Maria Lee," 2802.
"Marlborough," 2092.
"Mary Amelia," 2453.
"Mary Shepherd," *App.* p. 86; *Q.* 2085, 2150.
"May Queen," *App.* pp. 67, 87.
"Megæra," *App.* p. 61; *Q.* 586, 631, 729
"Merom," 61.
"Meuse," *App.* p. 99.
"Middlesex," 2092.
"Mindora," *App.* pp. 68, 89, 95.
"Minnie Grahame," *App.* p. 23; *Q.* 18, 23, 1386, 1404, 1425, 1450, 1480.
"Minotaur," *App.* p. 60; *Q.* 605.
"Miranda," *App.* p. 55.
"Mogul," *App.* p. 131.
"Monmouthshire," *App.* pp. 70, 96, 131.
"Moorsfort," *App.* p. 69, 93; *Q.* 1615.
"Morning Glory," 388.
"New Lampedo," *App.* pp. 70, 97.
"Nora Græme," 2589, 2598.
"Northbrook," *App.* pp. 80, 96.
"Numea," 848.
"Oliver Cromwell," *App.* pp. 69, 94; *Q.* 1143, 1193, 1202, 1216, 1222, 1228, 1256, 1629, 2587, 2599, 2626, 2717.
"Oneiza," 1611, 1663.
"Ontario," *App.* p. 96; *Q.* 1643, 2592.
"Palmyra," *App.* 67, 87; *Q.* 849, 2655.
"Persia," *App.* pp. 5, 25, 68, 69, 88, 91.
"Pocahontas," 1645, 2092, 2107, 2150, 2158.
"Prince Alfred," *App.* pp. 15, 69, 91.
"Princess Alexandra," 2092, 2157.
"Rattlesnake," 597, 631.
"Respigadera," 61.
"River Boyne," *App.* p. 39.
"Robert Gilroy," 559.
"Robert Watson," 2088, 2137, 2150.
"Royal Family," *App.* pp. 68, 77, 87.
"Saint Mungo," *App.* pp. 16, 92.
"Saint Nicholas," *App.* p. 131.
"San Rafael," *App.* p. 40.
"Sea Pink," 1297.
"Senator," *App.* p. 35.
"Serena," *App.* pp. 77, 89.
"Shalimar," 1613, 1663.
"Sidra," 1650.
"Staffordshire," *App.* pp. 70, 96, 131.
"Thames," *App.* p. 100.
"Theresa," *App.* p. 40.
"Thomas Parker," *App.* p. 59.
"Trent," 2092.
"Try," *App.* pp. 70, 97.
"Unione," 64.
"Usk," *App.* pp. 67, 86; *Q.* 1416, 1457.
"Vanda," *App.* pp. 68, 89.
"Victorine," 64.
"Wallace," 2538.
"Walter Baine," *App.* pp. 69, 92.
"Wanderer," *App.* p. 67, 87.
"Waverley," *App.* p. 18.
"William Batters," *App.* p. 100.
"Zeno," *App.* p. 41; *Q.* 2800, 2828, 2851.

Specification for Coal Cargoes.
Legislation recommended with regard to, *Rep.* p. viii., xxv.
Form of, *App.* p. 109.

Spontaneous Combustion of Coal.
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LONDON:
Printed by GEORGE E. EYRE and WILLIAM SPOTTISWOODE,
Printers to the Queen's most Excellent Majesty.
For Her Majesty's Stationery Office.

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REPORT

OF THE

ROYAL COMMISSION

ON THE

PRACTICE OF SUBJECTING LIVE ANIMALS TO EXPERIMENTS FOR SCIENTIFIC PURPOSES;

WITH

MINUTES OF EVIDENCE

AND

APPENDIX.

Presented to both Houses of Parliament by Command of Her Majesty.

LONDON:
PRINTED BY GEORGE EDWARD EYRE AND WILLIAM SPOTTISWOODE,
PRINTERS TO THE QUEEN'S MOST EXCELLENT MAJESTY.
FOR HER MAJESTY'S STATIONERY OFFICE.

[C.—1397.] *Price* 4*s*. 4*d*. 1876.

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ROYAL COMMISSION

ON

THE PRACTICE OF SUBJECTING LIVE ANIMALS TO EXPERIMENTS FOR SCIENTIFIC PURPOSES.

.s.) (Signed) VICTORIA *R.*

Victoria, by the Grace of God, of the United Kingdom of Great Britain and Ireland Queen, Defender of the Faith. To Our right trusty and well-beloved cousin and councillor Edward Viscount Cardwell, Our right trusty and well-beloved councillor John Baron Winmarleigh, Our right trusty and well-beloved councillor William Edward Forster, Our trusty and well-beloved Sir John Burgess Karslake, knight, Our trusty and well-beloved Thomas Henry Huxley, esquire, professor of natural history in the Royal School of Mines, Our trusty and well-beloved John Eric Erichsen, and Our trusty and well-beloved Richard Holt Hutton, esquire, greeting.

Whereas We have deemed it expedient that a Commission should issue to inquire into the practice of subjecting live animals to experiments for scientific purposes, and to consider and report what measures, if any, it may be desirable to take in respect of any such practice.

Now know ye, that We, reposing great trust and confidence in your zeal, ability, and discretion, have authorized and appointed, and do by these presents authorize and appoint you, the said Edward Viscount Cardwell, John Baron Winmarleigh, William Edward Forster, Sir John Burgess Karslake, Thomas Henry Huxley, John Eric Erichsen, and Richard Holt Hutton, to be Our Commissioners for the purposes aforesaid.

And for the better enabling you to fulfil the objects of this Our Commission, We do by these presents give and grant unto you, or any three or more of you, full power to call before you, or any three or more of you, such persons as you shall judge necessary, by whom you may be the better informed on the subjects herein submitted for your consideration, and every matter connected therewith, and also to call for, have access to, and examine all such books, documents, and papers as may afford the fullest information on the subject, and to inquire of and concerning the premises by all other lawful ways and means whatsoever.

And Our further will and pleasure is that you, or any five or more of you, do with all convenient speed report to Us, under your hands and seals, your opinion upon the matters herein submitted for your consideration.

And We do further will and command, and by these presents ordain, that this Our Commission shall continue in full force and virtue, and that you Our said Commissioners, or any three or more of you, may from time to time proceed in the execution thereof, and of every matter and thing therein contained, although the same be not continued from time to time by adjournment.

And for your assistance in the execution of these presents, We have made choice of Our trusty and well-beloved Nathaniel Baker, esquire, barrister-at-law, to be secretary to this Our Commission, whose services and assistance We require you to use as occasion may require.

Given at Our court at Saint James's, the twenty-second day of June 1875, in the thirty-ninth year of Our reign.

By Her Majesty's Command.

(Countersigned) RICH^D^ ASSHETON CROSS.

REPORT.

TO THE QUEEN'S MOST EXCELLENT MAJESTY.

In obedience to Your Majesty's commands, we have inquired into the extent to which the practice of making experiments upon living animals,—with a view to the advancement of science, or to the acquisition of knowledge available for the relief of human suffering or for the prolongation of human life,—is carried in this country,—and we have considered whether any and what measures ought to be adopted in respect of that practice. We have had the advantage of examining many eminent physicians and surgeons and physiologists of great reputation, as well as of hearing from the Secretary of the Royal Society for the Prevention of Cruelty to Animals the views of that Society. He has also laid before us several documents relating to the subject, which we have printed in the Appendix. App. IV.
We have thought it sufficient to give the references to that portion of them which consists of opinions extracted from published works or journals.

A new Society has been formed under the name of the Society for the Abolition of Vivisection. A letter from Sir George Duckett, Bart., whose name is at the head of the list of the Committee, declining to give evidence before us, will be found in the App. II.
Appendix. Mr. Holt, M.P. for the North-eastern Division of the County of 6175, &c.
Lancaster, one of the members of the Committee, has given evidence in compliance with our request. He stated to us very clearly and fully the conclusions 6170.
at which he has arrived and his reasons for them. Some observations which he 6230.
made to us on the constitution and course of proceeding of the Society will be 6287.
found in the Minutes. The honorary secretary gave evidence at considerable length. 4435.
It consisted in great part of extracts from published works, several of which we have not reprinted in extenso in the minutes,—but we have given the references so as to enable those who may desire it to refer to the originals for themselves.

Dr. Emanuel Klein, assistant professor at the laboratory of the Brown Institution, 3528.
lecturer on general histology at St. Bartholomew's Hospital, gave evidence before us. Dr. Klein has acted in the investigations which have been conducted under the medical officer of the Privy Council, and is author of the first part of the handbook to which we shall have to refer. The proof of his evidence was sent to him in the usual course for his corrections. This he returned with alterations which appeared to us to be so much at variance with the letter and spirit of the answers he had given us at his examination, that we felt ourselves unable to receive them as an authentic report of his evidence. In consequence of this refusal he has requested permission to withdraw the evidence. We have thought that this course would not be right, and we have included in the minutes the shorthand writer's note as it was originally taken, and have given in the Appendix the amended proof submitted to us, with the App. II.
correspondence on the subject.

We find that until a comparatively recent period physiology—the science 2211.
which treats of the phenomena presented by living animals—had been for some time past but little cultivated in this country, but that there has been of late years a great movement in advance. This movement is not special to physiology, but embraces physics, chemistry, pathology, and all the other branches of physical science. Laboratories for research and instruction in the sciences of light, heat, and 2678.
electricity, and of chemistry, no less than in physiology and comparative anatomy, have recently been established in Oxford and Cambridge. Physiological and pathological laboratories have been established in most of the large medical schools in the country. Much attention has been directed to the circumstance that, in the contract recently entered into for an addition to the University buildings at Edinburgh, provision is made for extensive appliances with a view to physiological experiment and instruction; while we are informed that until of recent years there was in this country no physical or physiological laboratory of any kind to which students had access, and in which they were regularly trained. There is, then, at the present time a general development of all the physical sciences,—what has been termed by one of the witnesses a great scientific revival. It is accompanied by 2215.

3090. the conviction, at which the men of science have generally arrived, that no teaching of physical science is complete unless it is illustrated by practical instruction. Physiology in particular is now for the first time assuming the position of a separate science. The professed physiologist has been until recently almost entirely unrepresented; that is to say, the physiological work has been done by persons engaged in other pursuits at the same time. The professor of physiology in Edinburgh was until a few years ago, a medical practitioner, a consulting physician; but the position is now filled by a professed physiologist, who occupies his whole time in that way. At present there are three positions in England, the tenure of which limits the holders to be exclusively professed physiologists, viz., Dr. Burdon-Sanderson's at University College (London), Dr. Foster's at Cambridge, and the Brackenbury pro-
1886. fessorship at Owens College, Manchester. It is indeed the expectation of those most
2242. conversant with the subject, that physiological investigations will more and more take
2301. place in connection with public institutions, that new chairs will from time to time
2868. be founded, and that an organised system of instruction in physiology will speedily
5420. become an important feature in scientific education.

It is evident, therefore, that the number of experiments at present performed upon living animals can by no means be regarded as the limit of the number which we are called upon to include in our consideration, but that on the contrary we must assume that the experimental method is being rapidly developed,—very
523. rapidly, Dr. Sharpey has assured us,—for, he says, that the application of physics to the phænomena of life, particularly in making exact quantitative determinations, is one of the great characteristics of modern physiology.

In laying before Your Majesty our opinion as to the extent to which the practice now prevails, we have not the means of referring to statistical returns except as regards the experiments performed in the physiological laboratories attached to medical schools and universities; and there can be little doubt that experiments have been and now are performed occasionally by private persons, of whose number we
2[illegible]7. are able to form no accurate computation. The number of persons systematically engaged in the performance of them does not appear to be more than from 15 to 20 at the utmost. Such statistical information as we have been able to obtain will be
App. III. found in the Appendix.

A very strong feeling has been excited in the country, within the last two or three years, on this subject.

On the occasion of the assemblage in London in 1874 of delegates from foreign countries connected with associations for the prevention of cruelty to animals, Your Majesty was graciously pleased to give public expression to a warm interest in the success of
App. I. their efforts,—to a horror in hearing and reading of the sufferings which the brute creation often undergo,—to a fear that this is sometimes the case from experiments in the pursuit of science,—and to a hope that the entire advantage of those anæsthetic discoveries, from which man has derived so much benefit himself in the alleviation of suffering, may be fully extended to the lower animals.

We believe that these are the sentiments of Your Majesty's subjects generally. The present feeling appears to have been excited by a variety of concurrent circumstances, such as the movement, of which we have already spoken, involving, it is generally believed, a great increase, present and prospective, in the practice of subjecting live animals to experiments;—the introduction at some of the principal medical schools of experiments, by way, not of original research only, but of demonstrations to students given in public;—and the circulation here of the reports of many very painful experiments, mainly taken from foreign publications;—but most of all by the appearance in 1873 of a work called a Handbook of the Physiological Laboratory, professing to be intended for beginners, and describing many very severe ex-
2284. periments. It is admitted in his evidence by the editor himself, that no adequate means
2333. had been taken either to explain the meaning which was intended to be conveyed by the word "beginners," viz., beginners in the special study of physiology,—or to make known what he told us is the general understanding in all English laboratories, that anæsthetics should be administered in the great majority of cases, and in other cases painful experiments should not be repeated merely to demonstrate truths already
2367. sufficiently established. This, it was presumed, would be taken for granted. He
2368. expressed his regret that this feeling should have been occasioned by the publication
2369. of the book, and gave us reason to expect that he would take measures, to which he referred, for correcting the impression it had produced. Much attention also has been drawn to a series of experiments which were recently performed by Dr. Ferrier in the laboratory of Dr. Crichton Browne, at the West Riding Lunatic Asylum at Wake-

field. These have been widely made known to the public, and it has been asserted that from their nature they could not have been performed under anæsthetics. If that had been the case they would doubtless have been extremely painful, but they were performed in the presence of many medical men, and we are positively assured by Dr. Ferrier and Dr. Crichton Browne that all the animals experimented upon were 3172. first rendered insensible to pain, and were kept so during the operations. Much 3193. allowance must be made for the misunderstanding and exaggeration to which reports 3228. on such a subject are necessarily liable when not critically examined and not based 762. upon the evidence of eye witnesses.

Medicine rests upon the triple basis of clinical observation, and patho- 1444. logical and physiological research,—and the practice of operating upon the living subject appears to have been coeval with the commencement of the pursuit of medical science, and to have been applied not only to animals, but at some times even to men, without regard to the suffering it involved. Speaking of the time of Celsus, Bacon, in the Advancement of Learning, says:—"As for the passages and pores, it is "true what was anciently noted, that the more subtle of them appear not in anatomia, "because they are shut and latent in dead bodies, though they be open and manifest "in live; which being supposed, though the inhumanity of anatomia vivorum "was by Celsus justly reproved, yet in regard of the great use of this observation, "the inquiry needed not by him so slightly to have been relinquished altogether, or "referred to the casual practices of surgery, but might have been well diverted upon "the dissection of beasts alive, which, notwithstanding the dissimilitude of their "parts, may sufficiently satisfy this inquiry."—Book II. Title X. 9.

Galen, in his writings, describes with painful particularity the great number of 499. the tables he had on which animals might be fastened, and the severe experiments which he used to perform upon them.

After the revival of letters, the practice was extensively pursued in the 16th and 17th centuries. Harvey appears to have been almost entirely indebted to it for the ever memorable discovery of the circulation of the blood. 394. "When," he says, "I first gave my mind to vivisections, as a means of "discovering the motions and uses of the heart, and sought to discover these from "actual inspection, and not from the writings of others, I found the task so truly "arduous, so full of difficulty, that I was almost tempted to think with Fracastorius, "that the motion of the heart was only to be comprehended by God. For I could "neither rightly perceive at first when the systole and when the diastole took place, "nor when and where dilatation and contraction occurred, by reason of the rapidity "of the motion, which in many animals is accomplished in the twinkling of an eye, "coming and going like a flash of lightning, so that the systole presented itself to "me, now from this point, now from that, the diastole the same; and then every-"thing was reversed, the motion occurring, as it seemed, variously and confusedly "together. My mind was therefore greatly unsettled, nor did I know what I should "myself conclude, nor what believe from others. I was not surprised that Andreas "Laurentius should have said that the motion of the heart was as perplexing as the "flux and reflux of Euripus had appeared to Aristotle. At length, and by using "greater and daily diligence, having frequent recourse to vivisections, employing a "variety of animals for the purpose, and collecting numerous observations, I thought 138. "that I had attained to the truth." The deer in the Royal Parks were placed at his disposal for this purpose; and when he had arrived at such a point that he felt he could demonstrate the thing, King Charles the First and his Queen, with the princesses, attended a lecture of Harvey's to witness the circulation of the blood proved upon the living body.

The great physiologist of the eighteenth century was Haller. Dr. Sharpey, after 592. speaking of Haller as one of the greatest of physiologists and the author of the greatest work upon physiology perhaps that ever appeared, quoted to us the following extract from his writings, published in the middle of the century. "But it "is not sufficient to make the dissections of the dead bodies of animals. It is "necessary to incise them in the living state. There is no action in the dead body; "all movement must be studied in the living animal, and the whole of physiology "turns on the motions, external and internal, of the living body. Hence no progress "can be made in investigating the circulation of the blood and its more recondite "movements, or the respiration, or the growth of the body and the bones, the course "of the chyle, or the motion of the intestines, without the sacrifice of living animals. "A single experiment will sometimes refute the laborious speculation of years. *Hæc* "*crudelitas ad veram physiologiam plus contulit, quam omnes fere aliæ artes quarum* "*conspirante opera nostra scientia convaluit.*" But it must not be supposed from this passage that Haller was an advocate of cruelty; for in his Life by Dr. Henry of Manchester it is recorded that he was convinced that an idle inquisitiveness or a passion for reputation could not justify our killing sensible beings in torments, and that whatever reason we may have to regard them as formed for our use, it is absurd and cruel to imagine that they are designed also to be the sport of our curiosity or vanity.

We have great satisfaction in assuring Your Majesty that at the present time a general sentiment of humanity on this subject appears to pervade all classes in this country. "It deserves detestation and abhorrence," is the emphatic reply of Mr. Darwin, when the question is put to him what would be his view of trying a painful experiment without anæsthetics, when the same
4672. experiment could be made with anæsthetics,—or in short of inflicting any pain that was not absolutely necessary upon any animal. This principle is accepted generally by the very highly educated men whose lives are devoted either to scientific investigation and education, or to the mitigation or the removal of the sufferings of their fellow creatures; though differences of degree in regard to its practical application will be easily discernible by those who study the evidence as it has been laid before
4667. us. Eminent as Mr. Darwin is in science, he has been but little conversant with
444. experiments on living animals. Of those who have practised such experiments from their earliest years of study, none is more distinguished than Dr. Sharpey. He assures the Commission that when he was a very young man studying in Paris, he went to the first of a series of lectures which Majendie gave upon experimental physiology, and was so utterly repelled by what he witnessed, that he never went back again. This was no small evidence of humane sentiment on the part of a young and ardent student at the outset of his professional career, for it involved the sacrifice of what was probably the principal object of his residence in Paris; and he does not hesitate now, in his riper years, to speak of one of Majendie's experiments in particular
474. as "his famous, it might rather have been called infamous" experiment.

2062. The feeling of the students on the subject is thus described by their teachers. Dr. Pavy says that at the commencement of his lectures he is almost obliged to give a little apology for saying that the course will be an experimental one. He sees upon the faces of the students sitting before him an expression which leads him to consider it necessary to make some explanation, and to tell them at once that no experiment will be introduced which will wound the feelings of the most sensitive amongst them. It is what he is constantly in the habit of doing, and he has found it necessary from what
2194. he has observed in his audience. He is speaking of his own class at Guy's Hospital, but this he says we may take as a fair average example of the students at all well
2898. conducted schools of medicine in the country. Dr. Rutherford told us, speaking of the students at Edinburgh, that if an animal has been suffered to come out from
4465. anæsthesia, the students at once resent it. Dr. McDonnell, speaking of the students in Dublin, says that unless he was able to give some good reason for doing away with the anæsthetics, the students would not tolerate the occurrence; the public opinion of
5468. the students would be strongly against it. Sir William Gull assures us that in a medical school anything like cruelty or indifference to suffering would be scouted by the public opinion of the students, and that he has never seen anything like indifference.

Dr. Haughton told us that he was present in Norwich on the occasion when the experiments upon dogs were performed which led to the celebrated prosecution, and the public was so much shocked that he thinks M. Magnan, who performed them,
1951. was in danger. Of this experiment we are told by Professor Humphry, who was
707. present during a portion of the time, that it was an experiment of some scientific
708. value, and the dog he saw did not appear to be suffering much. Sir William Fergusson
718. told us he thought it was grounded on incorrect views altogether, and Dr. Taylor that
1107. the experiments were of a most cruel kind and could answer no sufficient purpose.
1188.

The Secretary of the Royal Society for the Prevention of Cruelty to Animals, when
1545. asked whether the general tendency of the scientific world in this country is at variance with humanity, says he believes it to be very different indeed from that of
1549. foreign physiologists; and while giving it as the opinion of the society that experiments are performed which are in their nature beyond any legitimate province of science, and that the pain which they inflict is pain which it is not justifiable to inflict even
1546. for the scientific object in view, he readily acknowledges that he does not know a single case of wanton cruelty, and that in general the English physiologists have used
1543. anæsthetics where they think they can do so with safety to the experiment.

The recommendations which we shall humbly submit to Your Majesty will turn in a great measure upon the use of anæsthetics. The whole subject of experiments upon living animals has been, or at least ought to have been, relieved of the greater part of its difficulty by the discovery of anæsthetics and particularly of the anæsthetic properties of the vapours of sulphuric ether and of chloroform, in 1846 and 1847.

In considering what those recommendations ought to be, the first question that offers itself is, Can the practice of subjecting living animals to experiments be altogether prohibited?

To this question it may seem sufficient to reply that such an enactment must inevitably lead either to a general evasion of the law, or to an universal flight of medical and physiological investigators and students from the United Kingdom to foreign schools and laboratories, and that by this means the general treatment of animals in experiments would certainly not be altered for the better. We have not been entitled to expect the attendance of foreign witnesses; and it is obvious that our recommendations, if approved and adopted by Your Majesty, can have no force in foreign countries. We have, therefore, thought ourselves bound in fairness to avoid receiving adverse testimony in respect to foreign physiologists beyond what is to be derived from their own published writings; and it has given us sincere 2914.
pleasure to receive incidentally from competent witnesses an assurance of the 3856.
humane spirit which they have known to prevail in some foreign laboratories. 5418.
We are, nevertheless, fully justified in asserting that nothing will be gained to the 2217.
cause of humanity by compelling native students to seek their education abroad instead of at home; and it is evident that the voice of public opinion here will have an influence in the great public institutions of this country, which it cannot exercise if experiments on living animals shall be driven into private chambers to be carried 2897.
on, with inferior appliances, in conscious violation of the law.

But even if it were possible to accomplish the purpose by legislative prohibition, would it be reasonable to do so?

No one is better entitled to speak with authority on this subject than Sir Thomas Watson, not only on account of the high esteem in which he is held on professional and personal grounds, but also because the objection which prevails in many minds against all who have been accustomed to witness the infliction of pain is totally inapplicable to him. His practice, it is well known, has been that of a physician, and we learn that he has attained to his eminence in the profession without having ever himself seen an experiment on an animal at all,—making himself 33.
acquainted with those performed by others and their results, and making such use 64.
of them as he could. When the present controversy began, Sir Thomas Watson published, in a periodical of the day, his opinions upon it. This paper will be found in our Appendix; and his views will appear, in the minutes of our evi- App. III.
dence, to have been sustained by himself and supported by the general concurrence of the highest medical and scientific testimony. He begins with the general 8.
proposition that, at a small expense of suffering to one of the lower animals, we to
may obtain knowledge which enables us to prevent or mitigate pain much more 14.
severe and lasting, or even to ward off peril to life, or to prolong life in a human being. He states his opinion that for such purposes experiments are justifiable; but that they ought to be made under very great restraint, and with very careful forethought; that no such experiments are excusable if they are made at random; that to justify them at all there must be some definite object in view of a previously instructed mind; some plain question to settle, some important doubt or uncertainty to remove, some hypothesis containing the promise of service to humanity to be confirmed or confuted, at least some reasonable hope and prospect of resulting benefit; that the experimenter ought to be an absolutely good anatomist, that he ought also to be master of all that had hitherto been learnt respecting the question which he was endeavouring to elucidate, and that he should take especial care to have the proper implements and apparatus at hand for the performance of his experiments, and that he should have trained assistants who would not blunder in assisting him. Sir 22.
Thomas Watson says that, since the introduction of anæsthetics, the great majority 23.
of experiments can be rendered very nearly, if not entirely, painless. He further states that it is the decided wish of the most eminent men in the medical 45.
profession in both its branches to prevent abuse and to repress cruelty as much as possible, and he expresses his confident opinion that the Crown and Parliament may 46.
look for the support of those eminent men in any reasonable measures which they may adopt for that purpose.

In a kindred spirit, Sir James Paget, who is President of the Royal College of

Surgeons and of the Medical and Chirurgical Society, laid before us a series of resolutions which were passed at the meeting of the British Association in Edinburgh
267. in 1871. Those resolutions were:—"(I.) No experiment which can be performed "under the influence of an anæsthetic ought to be done without it. (II.) No "painful experiment is justifiable for the mere purpose of illustrating a law or fact "already demonstrated; in other words, experimentation without the employment "of anæsthetics is not a fitting exhibition for teaching purposes. (III.) Whenever, "for the investigation of new truth, it is necessary to make a painful experiment, "every effort should be made to ensure success, in order that the sufferings inflicted "may not be wasted. For this reason, no painful experiment ought to be performed "by an unskilled person, with insufficient instruments and assistants, or in places "not suitable to the purpose; that is to say, anywhere except in physiological and "pathological laboratories, under proper regulations. (IV.) In the scientific preparation for veterinary practice, operations ought not to be performed upon living "animals for the mere purpose of obtaining greater operative dexterity."

269. Sir James Paget proceeded to say that these resolutions had received his entire approval. The principle of them was adopted in a petition signed by Mr. Darwin, Professor Owen, Mr. Huxley, Sir William Gull, Sir William Jenner, the President of the College of Physicians, and several more leaders in science:—and the Bill which was introduced into the House of Commons by Dr. Playfair in the last session of Parliament must (without committing to all its provisions all who were favourable to it generally) be accepted as a proof of the readiness of men of the highest eminence in science to submit to the consideration of the legislature this difficult question.

The opinion that experiments upon animals susceptible of pain ought not to be altogether prohibited, even if it were possible to do so, has been supported before
272. us by reasoning which it is not easy to confute. All medical science, it is urged by Sir James Paget, being in a state of progress, is continually coming in sight of problems which can only be solved by experiments either upon man or upon some lower animal; so that if a patient suffering from injury or disease is brought to a physician or surgeon, the physician or surgeon must either adopt some course of cure which has become known to him from the former experience of the profession acquired by practice upon other patients or by experiment by himself or others on animals, or else he must adopt upon conjecture the remedy which he thinks most likely to be useful; that is to say, in other words, he must submit the present patient to an experiment, instead of proceeding to his treatment with the
1442. assurance which might have been given by past experience. "We save ourselves," Mr. Simon expresses it, "a great deal of needless experimentation on man by some "experimentations on the lower animals." This cogent argument is evidently susceptible of indefinite illustration from memorable cases which have from time to time occurred in history.

We propose to consider experiments upon animals under three heads,—operations involving the use of the knife,—the administration of poisonous or dangerous drugs, —and the production of disease; but before we do so it may be well to observe that in a large proportion of the experiments made for scientific purposes either no pain at all need be inflicted, as in the familiar instance of examining the circulation of the blood in the web of a frog's foot under the microscope;—or the animal is first permanently deprived of sensation, as in the study of reflex movements, of muscular irritability, of the action of the heart in cold-blooded animals, in the maintenance of the heart's action by artificial respiration, and other cases.

It may also be convenient to notice that two classes of experiment are usually spoken of in the medical profession under the common appellation of therapeutic experiments,—*i.e.*, experiments made with the view of ascertaining the properties of new remedies, or of acquiring greater precision in the knowledge of the action of those already in use. Therapeutic experiments are sometimes conducted by operations upon living animals, as when the organ, for example the liver, is exposed, in order that the effect of some agent upon that organ, whether a well-known agent or a newly discovered or suggested agent, may be open to view and be observed:—or they may consist in simply trying upon a living animal the effect of an agent which it is proposed to introduce, but which it is thought proper to try upon animals before the trial is hazarded upon man. In the tripartite division of experiments which follows, the former of these classes will be included under the head of operations, the latter under that of the administration of drugs.

Experiments upon living animals then may be considered under three principal heads, viz.:—

a. Operations:—These are performed for the purpose of examining, either for original research or for demonstration to students, the processes of life.

b. The administration of poisonous or dangerous drugs:—for the purpose of exhibiting the effect, or of discovering the cure,—or for the purpose of assisting legal investigations.

c. The production of disease:—for the purpose of observing its progress, and discovering the means of preventing, mitigating, or curing the effects of the same or similar diseases in men or animals.

a. The principal cases which have been selected for us as instances in which such operations have resulted in great discoveries have been the discovery of the circulation of the blood,—the discovery of the action of the lacteal and lymphatic system of vessels,—and Sir Charles Bell's discovery of the compound function of the spinal nerves; these three subjects have been selected for illustration because they lie at the very foundation of our present physiological knowledge. Professor Turner, of Edinburgh, to whom we have been indebted for this selection, has submitted to us notes of the history of various discoveries, and we desire to direct the particular attention of all who wish to examine the extent to which practical medicine has been improved by physiological experiment to his evidence and that of Dr. M'Kendrick on the subject. **3025.** **3025.** **3878.** **3916.**

It would require a voluminous treatise to exhibit in a consecutive statement the benefits that medicine and surgery have derived from these discoveries. Let us take for our example the discovery of the circulation of the blood, and the various improvements in the treatment of diseases, and in the safer method of performing surgical operations on the human subject, that have resulted from it. In medicine it is obvious that a knowledge of the nature of, and of the proper treatment to apply in the large and important class of diseases of the heart and blood-vessels, could not have been acquired without a knowledge of the mechanism of the circulation. In surgery, this discovery has exercised a still more direct influence; and the narrative of the improvements in practice directly referrible to it would lead us by gradual and successive stages from the time when after an amputation red-hot irons were applied to staunch the bleeding vessels, to the employment of the carbolized ligature of the present day. If Harvey's experiments, and those upon the lacteal system, were to be performed now, the animals would first be rendered insensible to pain; and even in the case of Sir Charles Bell's experiments, where sensation was the immediate subject of the investigation, by far the most severe part would also be performed while the animal was in a state of complete anæsthesia. **4538.** **3065.**

If, therefore, this inquiry had been instituted at some former time, we should have been compelled, in regard to this class of experiment, to weigh in the one scale the infliction of great and perhaps protracted suffering,—and in the other the sacrifice of knowledge, most important to mankind, but only to be looked for from experiments which involved such suffering. By the discovery of anæsthetics we have been relieved from that necessity, and our present task is to devise measures which may prove effectual to prevent abuse.

b. With reference to the administration of poisons to the lower animals, Sir James Paget brings forward the hope of discovering an antidote for snake poisons, in proof of the absolute necessity for the performance of these experiments. He mentions the many thousands of Your Majesty's Indian subjects who perish annually from snake bites. It is hopeless, he says, to make observations upon the persons bitten by the snakes, with the expectation of effecting a cure. A Government reward has been offered for the discovery of a sufficient antidote. Sir James Paget mentions the case of an enthusiast who was so sanguine of obtaining this reward that he was with difficulty restrained from subjecting himself to the bite of the cobra in proof of the efficacy of his antidote. The medical authorities of St. Bartholomew's Hospital persuaded him to transfer the experiment to some pigeons, which all instantly died. Sir James Paget says we cannot expect that short of 50 years hence a true antidote for snake bites should be found by any other means than by a series of experiments. **305.**

Of the advantages derived to man from the administration of drugs to animals by way of experiment, Sir James Paget gives an illustration in the case of that fatal and distressing malady angina pectoris, which appears quite recently to have been brought in some degree within the domain of medical control, in consequence of a discovery made from observing the effect of nitrite of amyl on living animals. It must not be forgotten that for the discovery of anæsthetics themselves both men and animals have been largely indebted to the practice now in question. **308.**

On the subject of administering poisons to animals in furtherance of medico-legal jurisprudence we examined Dr. Taylor, lecturer on medical jurisprudence and toxicology at Guy's Hospital, the author of well-known works upon the subject.
1155. Dr. Taylor has been a principal witness on all great cases of poisoning for the last 20 years. He referred us especially to his experience in the cases of Palmer, Smethurst, and Pritchard. He tells us that in the case of Palmer the conviction resulted from evidence given by himself and Dr. Rees. The effect of strychnine was
1167. at that time little known. Great difficulty was occasioned to the prosecution by the mode in which the viscera were handled after they had been taken from the body. The main point in controversy was whether it was possible that the poison could have been the occasion of the death, seeing that it was not found in the contents of the stomach. This possibility was established by experiments upon six rabbits made by
1162. Dr. Taylor and Dr. Rees. Again, Dr. Taylor referred us to a case which occurred in Suffolk 12 or 14 years ago, in which a woman was charged with having administered arsenic to her stepchild, who had clearly died from the effects of that poison. Dr. Taylor and Dr. Pavy established by experiments upon rabbits and dogs what satisfied the coroner's jury that the presence of arsenic in the stomach was not inconsistent with the statement that the death had been occasioned by the admitted application of arsenical ointment to the skin. These cases might be multiplied indefinitely.

It is not possible for us to recommend that the Indian Government should be prohibited from pursuing its endeavours to discover an antidote for snake bites :—or that without such an effort Your Majesty's Indian subjects should be left to perish in large numbers annually from the effects of these poisons; nor can we say that new medicinal agents ought in the first instance to be tried upon man, when the risk to human life might have been prevented by a previous trial upon animals. We cannot recommend that for want of such experiments criminals like Palmer should be permitted to escape, or persons suspected be deprived of the means of establishing their innocence. But we think it is most desirable that an effectual
1180. restraint should be placed upon what Dr. Taylor has described to us as purposeless
1168. cruelty,—on experiments made in excessive numbers,—on experiments made to
1169. establish what has been already proved,—on experiments attended with great pain,
1171. 1187. and defeating the very object in view,—on experiments made where a man has been desirous of bringing himself forward, or trying a new thing merely for the sake of a little notoriety.

c. The production of disease in animals has been already, and is likely to become still more, the source of knowledge prophylactic as well as therapeutic, tending in the most important degree to the prevention as well as to the mitigation and the cure of disease in the human family. There can be no simpler, or more striking
5530. example than that of vaccination for the small-pox, the discovery which was said to have saved more lives than all the wars of Napoleon had destroyed. It was by observation that Dr. Jenner discovered the immunity from small-pox of those who had contracted cow-pox. But it was by experiments upon cows that the origin of the cow-pox, a disease stated to be derived from grease in the horse, was ascertained. At the present time systematic experiments are being made under the direction of the Committee of Your Majesty's Privy Council for the purpose of establishing facts which may guide that department in taking efficient measures for the prevention or
1367. for the cure of disease. In an explanatory memorandum given to Mr. Forster when Vice-President by Mr. Simon, the medical officer, it is stated that the first aim is to obtain exact scientific knowledge of the causes and mode of attack of any disease which is in question; and in this study it frequently happens that more or less experiment has to be made as to the results which the administration of a particular influence will produce on an animal. Studies of this kind are sometimes made more immediately in the interests of man, as for instance in the case of the Asiatic cholera, and sometimes in that of the domestic animals, as in the case of sheep-pox and cattle plague, but perhaps oftenest in the common interests of both. Mr. Simon states in his memorandum that being obliged, as medical adviser of the Government, to make himself as proficient as he can in all that relates to the preventing and resisting of disease, he has felt it indispensable to have recourse inter alia to such studies. The subjects to which his investigations have been particularly directed have been cholera,
1359. tubercle, pyæmia, sheep-pox, and disinfectants. Much of the practical advice
1414. given by the Department to the public as to how they shall deal with the cases of cholera which may arise, and the precautions they shall take against the spreading of the disease, is founded on the basis of such experiments. The mode in which cholera is spread had indeed been suspected. It was a theory in the

teaching of Dr. Snow in 1849, but Mr. Simon states that since that time it has been
converted by experiment into a certainty which can be made a basis for the advice of a 1415.
Government department. Important knowledge has also been acquired in respect of
tuberculosis, that fatal malady which causes the loss of one tenth of the whole 615.
number of the human family who die in the United Kingdom. This knowledge may 1420.
be expected to receive fresh development, and to lead, if not to the suggestion of any
cure, at least to the avoidance of many of the causes which now occasion the production of the malady in the human subject.

For other instances of a like nature, we must refer to the evidence. The deduction
we draw from them is that, whether we look to the possibility of cure or to the
probability of prevention, we cannot recommend the total prohibition of experiments
of this class. It consists in subjecting a comparatively very small number of
animals to diseases not generally involving severe pain,—and from the observation
of these diseases results are likely to be derived tending to the mitigation, or possibly 615.
even the removal, of some of the severest scourges which afflict the human race.

But if it be impossible altogether to prevent experiments, and would not be reasonable to do so,—even if it were possible,—under what aspect ought they to be regarded by the law?

The law at present takes no special cognizance of the subject, but leaves it to the
operation of the statute 12 & 13 Vict. c. 92, commonly known as Martin's Act,
if indeed that Act should be held to be applicable; and the Act extends only to
domestic animals, leaving frogs, rabbits, guinea pigs, and other animals entirely
unprotected. It gives, that is, in respect of domestic animals, a remedy doubtful, 1890.
even if applicable, and not easily enforced, and in respect of others no remedy at all.

In considering the question of legislative interference, we have found in some
minds a decided prepossession against it. This appears to be connected, as in 4430.
the case of Mr. Lister, with a notion that such interference implies an imputation
of cruelty upon those who are engaged in these investigations; an imputation they
are conscious they have not deserved. From this prepossession, as we have already
seen, many of those whose position and character entitle them to the greatest weight
are wholly free; and it has almost always yielded to the consideration that if there 4431.
be a proved necessity for legislative interference to prevent abuse, such interference 2916.
will be right, provided that the teaching of physiology and the prosecution of research 5912.
by competent persons are not interfered with. Sir Thomas Watson, Sir George
Burrows, Sir James Paget, and many others have suggested the analogy of the 3220.
Anatomy Act, which has worked very satisfactorily for its purpose, and some scientific 2 & 3 Will. 4.
witnesses have expressed their opinion that the interference of the legislature is called c. 75.
for in the interest not only of humanity, but also of science. Sir James Paget considers that the present practice of the medical schools may be an error; no such experi- 320.
ments should be done, he says, except with the consent of a Committee. He thinks
it unreasonable to suppose that four members of the medical profession would join in 324.
approving an unnecessary or unreasonable experiment,—though one in his zeal might
do so, four certainly would not.

Sir William Fergusson thinks that experiments which involve suffering are carried to 1018.
a greater extent than they need be, and that there is continued and useless repetition.
His own opinion is much less favourable to these experiments than it was when he was 1031.
young, because he had much less grasp of the subject at that time. The more matured
judgment of his later years has led him to say to himself that he would not perform
some of the operations now that he performed in his earlier years. He thinks if the 1040.
public really knew what was actually going on in this country at this time, they would
expect an interference on the part of the Crown and Parliament just as much as with
reference to the disinterring of dead bodies years ago. He laid before us a protest,—on 6136.
the subject, we have been told, of cruelties alleged to have been practised at the 1030.
Veterinary College at Alfort,—signed in 1867 by the late Mr. Syme, and other eminent 1028.
surgeons, as well as by Mr. Wilkinson, the principal veterinary surgeon to the forces.

Of those who are most directly occupied in purely physiological instruction and research,—Dr. Burdon-Sanderson says that the state of things which he would like to see 2242.
established with reference to physiological research, is such as would unquestionably
discourage the making of experiments by any one, excepting by persons trained in a
school of physiology. He thinks there would be some inconveniences attaching to legis- 2348.
lation, but also that there would be even for physiology some advantages. The diffi-
culties would apply with reference to private individuals, but though he thinks it would 2349.
be an objection if private individuals should be interfered with, he does not lay great
stress upon that, because they are few and will probably become fewer year by year.

2680. As research is carried on into the more difficult parts of physiology, the investigator requires appliances of greater complexity, which are exceedingly expensive, and even if he could afford to buy them, he would have to build a place adapted for their use.
2220. Dr. Burdon Sanderson was an assenting party to Dr. Playfair's Bill, having taken an active part in regard to it, in the main approving of it, and thinking it a suitable measure.

3252. 3273. Dr. Ferrier, Professor of Forensic Medicine at King's College, London, thinks that any legislation that would retard physiological research would be a discredit to the country; but that owing to the great agitation which has taken place, it is very desirable to dissociate experiments for the purpose of original research from cruelty to animals, or the law which punishes that cruelty, and that some legislation for that purpose would be desirable.

Professor Rolleston of Oxford thinks we may bring the forces of society to bear
1298. upon the individuals. His impression of the English nature is that it is a law
1303. abiding nature, and that a pronunciamento on the side of carefulness would have its
1319. effect. He is himself amenable to the visits of the anatomy inspector, and does not feel in the least annoyed by them. He is of opinion that any experiments worth doing will be done in a public laboratory, and that no great inconvenience would arise from
1324. compelling all experiments to take place in a laboratory amenable to the visits of an inspector.

2319. Dr. Michael Foster, the Prælector of Physiology in Trinity College, Cambridge, is not prepared to go quite so far as Dr. Sanderson in the matter of the necessity for legislation, inasmuch as so far as his experience has gone in this country, he thinks
2321. there have been no abuses, and the humane sentiment which prevails might of itself be trusted entirely to prevent such abuses; but he would not object to measures which might prevent physiological experiments falling into the hands of other people of less humane sentiments if it should appear to be necessary. He does not object to legislative interference; but he does not so far agree with Dr. Sanderson as to think it desirable or necessary.

5381. Dr. Gamgee, Brackenbury Professor of Physiology in Owens College, Manchester, thinks that licenses might be given with great advantage if no vexatious spirit guided
5424. those who gave them. Having in view a considerable increase of great schools, he sees no objection to the establishment of some reasonable regulations for such institutions; and has no sentiment of an affront offered to himself in the proposal of regulations which, while they did not limit the progress of science, and the efforts of competent scientific people, should restrain the performance of operations upon living animals by incompetent persons, for no definite object, and without proper precautions.

2874. Dr. Rutherford, Professor of the Institutes of Medicine and Physiology in the University of Edinburgh, says, that it might be almost a dangerous thing for the power of granting a license to be vested in the Secretary of State: that it would be better to vest it in the councils of the Royal Society and other scientific societies, under whose jurisdiction the Inspector of Anatomy might act; and these councils might in turn be
2877. responsible to the Secretary of State. Dr. Playfair's bill, he thinks, was objectionable
2880. because it would have prohibited an experiment for the purpose of demonstration
2881. even under complete anæsthesia; and there are some minor ones which cannot well be performed under anæsthesia, such as demonstrating the action of strychnia. With these exceptions, neither Lord Henniker's bill nor Dr. Playfair's would interfere with the
2875. proceedings of a competent and well organized school like that of the University of
2887. Edinburgh; but would have the effect of interfering with persons of a totally different description, if there should be any such, who employed themselves in such
2897. pursuits. In organized schools there are securities against abuse which do not exist elsewhere.

5940. Dr. Handyside, Teacher of Anatomy in the Edinburgh School of Medicine, suggests that a system of licensing would afford the general public ample security that the practice is nowhere being carried on to any objectionable extent. He refers to the
5945. analogy of the Anatomy Act, as supporting a system of inspection. He thinks that the control should in some degree be entrusted to laymen, and proposes that justices of the peace should certify to the fitness of the persons applying for licenses, to prevent the profession being without a check.

1867. Dr. Samuel Haughton, the Medical Registrar of the School of Physic of Trinity College, Dublin, the largest school in Ireland, tells us that vivisections are strictly
1869. prohibited, and that the opinion of the educated public in Ireland is very sensitive on the subject of vivisection for the purpose of teaching classes. As regards
1874. original research, he thinks there is a good deal of second-rate physiological

practice going on, which needs control; that the conscientiousness and common sense
and savoir faire of young physiologists ought not to be trusted without it. His opinion 1917.
is that the practice should not be allowed at all for teaching purposes; that for purposes 1963.
of original research it ought to be tolerated, but regulated:—the supervision
having an analogy to, or being based upon, the provisions of the Anatomy Act.

For the opinions given by other witnesses we refer to the minutes of evidence.

But even if the weight of authority on the side of legislative interference had been
less considerable, we should have thought ourselves called upon to recommend it by
the reason of the thing. It is manifest that the practice is from its very nature liable
to great abuse; and that since it is impossible for society to entertain the idea of
putting an end to it, it ought to be subjected to due regulation and control. Those
who are least favourable to interference assume, as we have seen, that interference
would be directed against the skilful, the humane, and the experienced. But it is
not for them that law is made, but for persons of the opposite character. It is not to
be doubted that inhumanity may be found in persons of very high position as
physiologists. We have seen that it was so in Majendie. Dr. Carpenter told us that 5627.
he has seen in many instances a perfect callousness to animal suffering before the
introduction of anæsthetics, a callousness which strongly repelled him. That very
severe experiments are constantly performed cannot be doubted by those who read
the documentary evidence which has been laid before us, and the testimony of
Dr. Walker and of other witnesses who speak from personal knowledge of the
sufferings which they say have been often unnecessarily inflicted in the name of
science. Dr. Anthony, who resides in the neighbourhood of Birmingham, told us 2509.
that he is acquainted with instances of young men who carry on experiments in
private houses from mere curiosity. He considered that he had obtained his knowledge 2523.
confidentially, and declined to give us any further clue to the individuals than to say 2526.
that he has a consciousness that the thing has been done, is done, and probably will be
done. We have had some evidence that cases have arisen in which the unpractised 3466.
student has taken upon himself, without guidance, in his private lodgings, to expose 4917 to 4934.
animals to torture without anæsthetics for no purpose which could merit the name of
legitimate scientific research. Evidence of this nature is not easily obtained. So far 1687.
as our evidence goes, such cases appear to have been exceptional and abnormal, but 5076 to 5087.
the repetition of them is not on that account unworthy to be guarded against by a
legislature desirous of giving effect to the moral sense of the community at large.
Besides the cases in which inhumanity exists, we are satisfied that there are others in
which carelessness and indifference prevail to an extent sufficient to form a ground
for legislative interference. We have not thought it part of our duty, the majority of
us not having had professional training, to decide upon matters of differing professional
opinion, but we have been much struck by the consideration that severe experiments
have been engaged in for the purpose of establishing results which have been
considered inadequate to justify that severity by persons of very competent authority.
Cases may not improbably arise in future in which the physiologist may be disposed
to underrate the pain inflicted in the course of establishing results which may prove
to be trivial, or even worthless. Looking to the circumstance that a great increase
is to be expected in physiological inquiry, it appears to us most important that
some legislative control should be established to prevent abuse extending in this
direction. It is, moreover, much to be regretted that a feeling of suspicion,
and even of abhorrence, should have been permitted to grow up among a large
and very estimable portion of the public, against those who are devoted to the
improvement of medicine and to the advancement of science. Publicity is the
antidote of suspicion, and we look to the reasonable superintendence of constituted
authority as affording the means of reconciling in the public mind the sentiment of
humanity with the desire for scientific knowledge.

Our conclusion, therefore, is that it is impossible altogether to prevent the practice of making experiments upon living animals for the attainment of knowledge applicable to the mitigation of human suffering or the prolongation of human life:—that the attempt to do so could only be followed by the evasion of the law or the flight of medical and physiological students from the United Kingdom to foreign schools and laboratories, and would, therefore, certainly result in no change favourable to the animals:—that absolute prevention, even if it were possible, would not be reasonable:—that the greatest mitigations of human suffering have been in part derived from such experiments:—that by the use of anæsthetics in humane and skilful hands the pain which would otherwise be inflicted may, in the great majority of cases, be altogether prevented, and in the remaining cases greatly mitigated:—that the infliction of severe and protracted agony is in any case to be avoided:—that the abuse of the practice by inhuman or unskilful persons,—in short the infliction upon animals of any unnecessary pain,—is justly abhorrent to the moral sense of Your Majesty's subjects generally, not least so of the most distinguished physiologists and the most eminent surgeons and physicians:—and that the support of these eminent persons, as well as of the general public, may be confidently expected for any reasonable measures intended to prevent abuse.

With these views we have examined the measures which were introduced into the two Houses of Parliament in the last session, as well as the bill which has been submitted for our consideration by the Royal Society for the Prevention of Cruelty to Animals, and we now propose humbly to recommend to Your Majesty the outline of a measure which, in the interest both of humanity and of science, we think it will be expedient to adopt. But before doing so we will briefly state our views upon certain points of interest which have come before us in the course of our inquiry.

It has been proposed to enact that the object in view shall be some immediate application of an expected discovery to some prophylactic or therapeutic end, and that any experiment made for the mere advancement of science shall be rendered unlawful. But this proposal cannot be sustained by reflection upon the actual course of human affairs. Knowledge goes before the application of knowledge, and the application of a discovery is seldom foreseen when the discovery is made. The first origin of a great discovery is often, like the germ of the natural life in an animal or a vegetable, so small as to be scarcely perceptible, and yet it may contain in it the seeds of the grandest results. "Who," says Helmholtz, "when Galvani touched "the muscles of a frog with different metals, and noticed their contraction, could "have dreamt that . . . . all Europe would be traversed with wires, "flashing intelligence from Madrid to St. Petersburg with the speed of lightning? "In the hands of Galvani, and at first even in Volta's, electrical currents were "phænomena capable of exerting only the feeblest forces, and could not be detected "except by the most delicate apparatus. Had they been neglected, on the ground "that the investigation of them promised no immediate practical result, we should "now be ignorant of the most important and most interesting of the links between "the various forces of nature. . . . . Whoever, in the pursuit of science, "seeks after immediate practical utility, may generally rest assured that he will seek "in vain." As we have seen, Harvey's great discovery has been followed by remedial consequences of inestimable value, but those consequences were not foreseen by himself at the time that he made the discovery. It was, at the time that he made it, 247. "a mere scientific discovery."

Again, it has been proposed to sanction experiment for original research, but to 951. prohibit it as far as regards demonstrations to pupils. In the regulations issued by App. III. the Royal College of Surgeons, attendance upon physiological lectures is required from candidates for the medical profession; but it is stated that it is not expected that learners shall perform vivisections. We are told by Dr. Acland, Regius Professor of Medicine in the University of Oxford, the President of the Medical Council, that the question of the extent to which practical physiology is necessary for students would have been brought before the Medical Council this year, at the session which has lately closed, had it not been for the appointment of our Commission,—and that at his suggestion it was delayed until after we should have made our report to Your Majesty. It seems to us that living animals ought not to be subjected to experiment at all for any purpose of ordinary education. But in the case of professional education, as at one of the medical schools, it cannot, we think, be denied that there is much force in the argument that teaching without demonstration can scarcely be 2231. considered teaching. With respect to the medical schools we accept the resolution of the British Association in 1871, that experimentation without the use of anæsthetics is not a fitting exhibition for teaching purposes; and whatever may be the hazard of 267. evasion in private chambers, we think there can be none in the public lecture room of a great institution, when Parliament shall have established the obligation and the Crown have undertaken to enforce it.

A collateral argument has been brought forward in favour of this distinction, aiming not at the saving of pain to the animal, but at the saving of demoralization to the student. But the tendency to demoralization is connected, as the shadow with the substance, with the rightness or wrongness of the thing itself; and the evidence we have quoted above seems to show conclusively that at the medical schools where such demonstrations are exhibited under anæsthetics, the sense of humanity in the students is not in fact impaired.

It has been proposed to prohibit experiment except in public halls, to which a 3441. certain portion of the non-professional public shall at all times have access. It appears to us that this proposal would, if carried into effect, tend to frustrate the experiment as regards its usefulness, and, perhaps, as regards the effective administration of anæsthetics also, since the most essential requisite for the conduct of a delicate experiment is that the person who makes it should be free from any mental interruption or disturbance; while the presence of ignorant spectators could do nothing to secure the real humanity of the experiment. An animal may be suffering exquisite torture, and yet (so far as we yet know) the worari poison may, by its effect upon the motor nerves, prevent the exhibition of any feeling. Or, on the other hand, an animal may make every demonstration of suffering while the real sensation is destroyed. In the human subject, when chloroform is employed, or when by an 186. injury to the spine the connexion with the brain is interrupted, it sometimes happens that all the outward manifestation of pain is exhibited, when the patient afterwards 3375. disclaims having experienced any sensation of it. These effects are perfectly familiar to the instructed, but would be simply misleading to the uninitiated.

A good deal of evidence has been given as to the effect of the poison called worari or curari. This poison is very convenient to an operator, since it paralyses the motor nerves, and keeps the animal quiet. It has however been positively stated by perhaps the highest authority on such a subject, Claude Bernard, to have no effect in producing insensibility to pain. This opinion is now beginning to be disputed, but we think that until the question shall be much better settled than it is at present this poison ought not to be regarded as an anæsthetic by those who administer the law in respect of experiments on animals.

Some physiologists, while quite ready to assert broadly the principle that so far as is possible no painful operation ought to be performed except under anæsthetics, do not extend the application of the principle to cold-blooded animals. Of this kind of animal the typical instance is the frog, in which there has sprung up an import trade for the purpose of experiment, the large frog of Germany not being indigenous here. Dr. Schäfer, the assistant professor of physiology in University College, after saying 3772. that the treatment of animals there is, he is quite certain, dictated most strictly by a sentiment of humanity, nevertheless goes on to say that they consume a large number 3783. of frogs, and that the experiments upon living frogs are usually performed without anæsthetics, because the opinion is that the frog is not so sensitive as the higher 3791. animals, and he says that no special precaution is taken to diminish pain in their case. 3802. This doctrine is one which ought not to be too readily admitted, for the question is an important one; the proportion of frogs to all the other animals put together that are subjected to experiment is so great, that the frog, we are told, is called 3698. the physiologists' animal. Dr. Gamgee tells us on the contrary that when he 5414. decapitates a frog he always crushes the brain, and explains to the students why he 103. 514. thinks that the brain might otherwise retain a sensibility, which would not be retained in a warm-blooded animal.

It is said by some of the witnesses that it is very difficult to subject frogs to 4563. chloroform. But this is contradicted by the experience of others whose authority 727. is indisputable, as for example Professor Humphry and Dr. Brunton; and Dr. M'Donnell 5739. 4564. tells us that he constantly uses it for frogs and even for tadpoles, and that if a little chloroform is diffused through water, and the tadpole is put swimming about it for a few minutes, there is enough absorption through the skin to render it soon insensible, and it can be placed under the microscope for half an hour or so, and not stir. We think that in framing rules for the administration of a system, there ought to be much jealousy in too readily admitting convenient doctrines, and that proper care should be taken to insist upon the removal of the sensibility to pain 5992. even in the case of cold-blooded animals.

Mr. Pritchard, Professor of Anatomy in the Royal Veterinary College, performs most of the operations there. He says that they have no operations for the purpose 786. of experiments, and thinks it would be improper that they should. He is not aware of any being performed in this country by veterinary surgeons, does not think it is the practice, and, if it were, the principal of the college, Professor Simonds, and persons in positions like his own, would be ready to support the Government in any 790. reasonable measures of regulation. In the New Veterinary College at Edinburgh the operations are, as a rule, performed by the principal, Mr. Williams. He admits as respects one, but only one, important operation, that it is occasionally performed for teaching purposes, but he says that the animals are always rendered insensible by 6056. chloroform. The principles of our report apply to the practice of veterinary surgeons, and they, like the rest of the community, are included in the purview of the measure we are now about to recommend.

App. III. We have spoken of three bills which have been submitted to us. They will be found in the Appendix. All these bills proceed upon the basis of license by the Secretary of State, subject to withdrawal in case of abuse,—and of adequate inspection. This basis, as we have already seen, was laid down also in the evidence of Sir Thomas Watson and of other most competent witnesses, and upon it we think an effectual measure for the prevention of abuses may be founded.

1567. We were reminded by the secretary that the Royal Society for the Prevention of Cruelty to Animals is not the society established for the total abolition of experiments. The Royal Society for the Prevention of Cruelty to Animals has prepared its bill upon the supposition that experiments of a nature to cause pain are justifiable if they are performed when the animal has first been rendered wholly insensible to pain, and is destroyed before the effect of the anæsthetic ceases. The bill also provides that the place shall be registered, and that no such experiments shall be made by anyone while lecturing or giving instruction to students in classes or otherwise. The secretary has told us that the society is formed for the prevention of cruelty, and would not step out of its direction to legalize anything of a contrary nature; yet he says, in reference to a supposed case in which a great good was aimed at, and a small amount of suffering inflicted, that in such a case, even if their own bill had become law, the society would not prosecute. But we have already stated that experiments under complete anæsthesia may be used in the course of a lecture without objection on the score of cruelty; and there might be many experiments made for the purpose of original research in which the injury inflicted would be very small and the pain would be almost nil, and in which it might be more humane to permit the animal to enjoy life, than to destroy it. 1680. 1681. 4560.

The bill introduced last session into the House of Lords went upon the principle that no experiment should be lawful unless the animal were completely under the influence of an anæsthetic, and the experiment were performed in a place registered by the Secretary of State. Any person might apply to the Secretary of State for a special license to perform vivisections without the use of anæsthetics; such license was to continue for six months and no longer. On proof of any abuse, the registration of the place might be withdrawn.

The bill introduced into the House of Commons prohibited all experiments causing pain, or of a nature to cause pain, with the following exceptions, viz.:—1st. For the purpose of new scientific discovery, but for no other purpose, an experiment might be made, provided that the animal was placed and kept under complete anæsthesia. 2nd. Where for the purpose of new scientific discovery, and for no other purpose, an experiment was desired to be made in which insensibility could not be produced without necessarily frustrating the object of the experiment, it might be performed by a person holding a license granted by the Secretary of State under certain conditions.

As regards the last bill, if adopted precisely as it was proposed, it would have prevented the use of an experiment for the purpose of demonstration before a medical class in a public institution, however absolute the security for complete anæsthesia:—an object not intended to be arrived at, we presume, by the framers of the bill.

What we should humbly recommend to Your Majesty would be the enactment of a law by which experiments upon living animals, whether for original research or for demonstration, should be placed under the control of the Secretary of State, who should have power to grant licenses to persons, and, when satisfied of the propriety of doing so, to withdraw them. No other persons should be permitted to perform experiments. The holders of licenses should be bound by conditions, and breach of the conditions should entail the liability to forfeiture of the license; the object of the conditions should be to ensure that suffering should never be inflicted in any case in which it could be avoided, and should be reduced to a minimum where it could not be altogether avoided. This should be the general scope of the conditions; but their detailed application should be left to be modified from time to time by the minister responsible according to the dictates of experience. In the administration of the system generally, the responsible minister would of course be guided by the opinion of advisers of competent knowledge and experience. Dr. Playfair's bill provided a machinery for the purpose, and some arrangements of the kind proposed in that measure would be necessary. But we think it is inexpedient to divide the responsibility of the Secretary of State with that of any other persons by statutory enactment, and we recommend that his advisers should be from time to time selected and nominated by himself. Their names should be made known to the profession and the public. It may be found desirable that one of the conditions to be attached to a license should be that the experiments should be performed in some particular place; but this is a detail which may vary with circumstances, and we think it ought not to be stereotyped by statute.

The Secretary of State must have the most complete power of efficient inspection and of obtaining full returns and accurate records of all experiments made. Any place in which experiments are performed must be registered and open to efficient inspection. The appointment of an inspector or inspectors will be necessary, and we have seen that the analogy of the Anatomy Act has been appealed to by many high authorities. It is to be observed that the duties under that Act are of a nature much more mechanical than those which will be required in the present instance. The inspectors must be persons of such character and position as to command the confidence of the public no less than that of men of science.

Abuse of the power conferred by the license must, of course, render the holder liable to its withdrawal; but this will involve great disgrace; and the withdrawal of the license of an eminent man without real cause might be a serious public mischief. We have felt it necessary, therefore, to consider what steps should be taken when the question of such withdrawal may arise. We think that the holder of a license, when he shall receive notice that the Secretary of State intends to withdraw it during the period for which it has been granted, should be at liberty to demand a public inquiry; that this inquiry should be held before one of the Judges of the Supreme Court, with two competent assessors to be appointed by the Secretary of State, the Court having the full power of conducting it as a legal investigation by summoning and swearing witnesses, issuing commissions, and so forth:—that on the result of this inquiry, the Secretary of State should determine whether the license ought to be withdrawn, and when he decides in the negative, should have the power of giving the holder of the license the reasonable costs of his defence.

Magistrates ought to be empowered, on cause shown, to authorise the police to enter and search the premises of persons suspected of performing experiments without a license; and the performance of such experiments without a license should be penal.

It has been suggested that cases may occur in which an urgent necessity may have occasioned an experiment when there has been no licensed person within reach, and it has not been possible to apply for a license; such as a sudden case of suspected poisoning, arising, perhaps, in a remote place, when the experiment has been reasonably considered indispensable, for the purpose either of cure or of medico-legal investigation. Bonâ fide cases of this kind ought evidently to be free from the risk of vexatious prosecution, and this can be secured by vesting in the Secretary of State the power of putting a veto on a prosecution. 1913.

We believe that by such a measure as we have now proposed the progress of medical knowledge may be made compatible with the just requirements of humanity. In zeal for physiology, the country of Harvey, Hunter, Bell, and Darwin may well endure the test of comparison. We trust that Your Majesty's Government and the Parliament of this kingdom will recognize the claim of the lower animals to be treated with humane consideration,—and will establish the right of the community to be assured that this claim shall not be forgotten amid the triumphs of advancing science.

CARDWELL. (SEAL.)
WINMARLEIGH. (SEAL.)
W. E. FORSTER. (SEAL.)
JOHN B. KARSLAKE. (SEAL.)
T. H. HUXLEY. (SEAL.)
JOHN ERIC ERICHSEN. (SEAL.)
RICHARD HOLT HUTTON. (SEAL.)

NATHANIEL BAKER,
Secretary,
8th January 1876.

Should it please Your Majesty and Parliament to pass any measure such as we have recommended in this Report, I desire to suggest one additional restriction which might either be embodied in the statute or endorsed by the Secretary of State among the conditions of the license which we have proposed. That restriction is, that the household animals, dogs and cats, should be exempted altogether from liability to experiments of this kind. The evidence we have taken seems to me to supply two weighty reasons for such a restriction; while a third is contained in the very nature of the relation existing between these creatures and man. The first, and in some respects the most weighty and practical reason, is this,—that there is evidently a very strong presumption that the demand of physiologists for these creatures is supplied by persons who decoy them away from their proper owners, and that in this way a strong temptation is furnished for actual theft, and all the distress which thefts of this nature too often cause. In answer to a question as to the source of supply, an eminent physiologist said (answer 2822), "I know as regards rabbits and guinea pigs, and so "forth, that they are drawn from the usual sources, that they are bought in the "market;" but when asked specially as to dogs and cats, he replied (answer 2823), "I cannot tell you where they come from. There is no proper provision in this "country by which one can obtain dogs, even for the most legitimate purposes, and of "course I am not informed as to the way in which they are obtained. They are always "paid for at a proper price." And another equally eminent witness, who had told us that in some two or three series of experiments on the cholera poison he had used considerably more than 90 cats, that being the number which he used (answer 5747) in one of those series of experiments only—replied to a question touching the source of supply, "They are supplied to me by a man;" and when further asked whether the person referred to obtained them in a legitimate way, he answered, "I make no inquiries" (5734, 5735). It would appear, therefore, that however painless the investigations conducted on dogs and cats may sometimes be, there is a special abuse to which even such painless experiments are liable, of a kind not affecting the experiments on rabbits and other creatures of which there is a sufficient market supply at low prices, namely, that the former furnish a strong motive for an illicit trade, not only degrading in itself, but causing frequently great distress to the owners of the creatures decoyed away. The second reason for this restriction with which our evidence has furnished us, is contained in an answer of Dr. Anthony, the pupil and dissector of Sir Charles Bell, who when asked whether the domestic animals are not liable to that special sensibility or "hyperæsthesia" to which civilized men appear to be so much more subject than barbarous tribes, replied, (answer 2596), "I am inclined to think so, that you have brought both under the "influence of what you may term civilization." It is true that the same witness told us, with obvious justice, that even in the same species you would find some creatures of very low, and others of very high sensibility, and that, in his opinion, intelligence might be taken as almost a measure of sensation (answer 2598); but as it will be impossible to draw fine distinctions in such cases, even if the other reasons affecting the question admitted of any such distinctions, and as it is notorious that no class of animals otherwise convenient for experimentation contains so many creatures of high intelligence, and therefore probably of high sensibility, as dogs and cats, it seems to me desirable, in consideration of this special sensibility, to exempt these members of our households from all liability to such experimentation. A third reason for this exemption seems to suggest itself from the very nature of our relations to these creatures, which we have trained up in habits of obedience to man and of confidence in him, so that there is something of the nature of treachery as well as of insensibility to their sufferings, in allowing them to be subjected to severe pain even in the interests of science. No doubt it is the intention of the measure we have recommended to reduce animal suffering to a minimum in all cases; indeed I may be allowed to say that the measure proposed will not at all satisfy my own conception of the needs of the case, unless it results in putting an end to all experiments involving not merely torture but anything at all approaching it; for where the pursuit of scientific truth and common compassion come into collision, it seems to me that the ends of civilisation, no less than of morality, require us to be guided by the latter and higher principle. But as there is no mode by which the discretion of the licensee, so long as he continues to hold his license, can be limited, I think we must assume that the subjects of these experiments will continue to be liable to a greater or less degree of suffering; and that we should try to diminish the evil involved in the infliction of that suffering to its lowest point. And if suffering is to be inflicted at all, with whatever humane economy it is meted out, it is better both as regards the evil of enduring and the evil of inflicting it, that the humble friends of man, which have been taught to obey and trust him, should not be selected as the victims. I may add that I do not find any trace in our evidence that there is a single one of the important scientific discoveries which have been represented to us as due to, or as finally verified by, experiments of this kind, of which science would have been deprived had any such limitation as this been at the time in force.

I do not include all the domestic animals in this suggestion, for two reasons. In the first place it is not necessary. The animals useful for agriculture or for purposes of food—like horses, oxen, and sheep—are a great deal too valuable, a great deal too well guarded, and for the most part a great deal too large for the purposes of ordinary physiological experiment. And in the next place, in the interest of these classes of animals themselves, it would be undesirable. The only sort of experiments to which they are commonly subjected are pathological experiments, *i.e.*, artificial inoculations with disease, made with the view of discovering some cure or some mitigation for the epidemics which periodically decimate them, like cattle plague or sheep-pox. Such experiments stand on a somewhat different footing from experiments made purely or chiefly in the interest of man himself. They are not very numerous; wherever they endanger life they are necessarily costly; and they may result in discoveries of the highest possible benefit to the races of creatures in whose behalf they are made.

I know of but one serious objection to this proposal. It has been urged upon me that by drawing a distinction in favour of certain classes of animals, the legislature would be taking a step in the wrong direction, since all humane people are desirous to see the Cruelty to Animals Act (commonly called Martin's Act), which at present protects domestic animals only, extended to wild animals; whereas the adoption of such a restriction as I have suggested would furnish, it is said, a new excuse for leaving wild animals unprotected by law. I cannot see the force of this objection, which, if it were valid, would be a very serious one, since I strongly desire to see the scope of Martin's Act extended so as to include creatures of all kinds. We should not, I think, be the less anxious to guard the less sensitive creatures from torture, because we had put the more sensitive under special safeguards. It seems to me that creatures bound to us by special ties may well and safely be permitted special privileges; indeed, that the tendency of any measure which recognised more explicitly the claims of our family dependents to be especially guarded from anything like hostile treatment, would have a generally humanizing influence on social manners, and improve instead of deteriorating the treatment even of wild animals.

RICHARD HOLT HUTTON. (SEAL.)

NATHANIEL BAKER,
Secretary,
8th January 1876.

MINUTES OF EVIDENCE

TAKEN BEFORE

THE ROYAL COMMISSION ON VIVISECTION.

Monday, 5th July 1875.

PRESENT:

THE RIGHT HON. VISCOUNT CARDWELL, IN THE CHAIR.

The Right Hon. LORD WINMARLEIGH.
The Right Hon. W. E. FORSTER, M.P.
Sir J. B. KARSLAKE, M.P.
JOHN ERIC ERICHSEN, Esq.
RICHARD HOLT HUTTON, Esq.
N. BAKER, Esq., Secretary.

Sir THOMAS WATSON, Bart., M.D., called in and examined.

Sir T. Watson, Bart., M.D.

5 July 1875.

1. (*Chairman.*) It is quite unnecessary and supererogatory to ask you any such question, but I will just get it on the notes: you are one of the physicians in ordinary to the Queen, and you have been president of the Royal College of Physicians, and a member of the Medical Council?—Yes.

2. You have recently published your views upon what you have called Vivisection?—Yes.

3. The subject on which you are invited to give evidence to-day is a little wider and is not correctly limited by the term Vivisection; it is described in the Commission as "The practice of subjecting live animals to experiments for scientific purposes?"—That is the right definition, no doubt, of the whole subject.

4. Including the subjecting of animals, for instance, to poisons?—Yes.

5. Which would not correctly be described by the word Vivisection?—No.

6. You have given, no doubt, great attention to both branches of the subject, and not merely to the one which is included in the word Vivisection?—Yes; I have thought of it in its larger sense.

7. Have you any objection to furnish the Commission with a copy of the paper which you have written on the subject?—No; it is entirely at the disposal of the Commission.*

8. In that paper it is said that "at a small expense "of suffering in one of the lower animals we may "obtain knowledge which enables us to prevent or "mitigate pain much more severe and lasting,—or "even to ward off peril to life, or to prolong life—in a "human being." Is that so?—I believe that entirely.

9. Is it your opinion that for such purposes experiments are justifiable?—That is my opinion.

10. Is it your opinion that if justifiable they should be made under very great restraint, and with very careful forethought?—Yes.

11. And that without that restraint and without that forethought they would not be justifiable?—I hold that opinion.

12. Do you hold that any such experiments are excusable "which are made at random, simply to see what will happen"?—I believe that experiments so made are not excusable.

13. Do you think that "to justify them at all there "must be some definite object in view of a previously "instructed mind?"—I do.

14. "Some plain question to settle, some important "doubt or uncertainty to remove, some hypothesis "containing the promise of service to humanity to "be confirmed or confuted, at least some reasonable "hope and prospect of resulting benefit?"—That is my opinion. I might perhaps say with respect to the experimenter that he ought to be an absolutely good anatomist, that he ought also to be master of all that had hitherto been learnt respecting the question which he was endeavouring to elucidate, and that he should take especial care to have proper implements and apparatus at hand for the performance of his experiments, and that he should have trained assistants who would not blunder in assisting him.

15. Do you think that any man is justified "in "making any painful experiment upon a living "creature who does not possess the skill, judgment, "intelligence, and previous knowledge" to which you have pointed?—I believe that no man is justified in making such experiments who is not so qualified.

16. And that "all possible care should be taken by "the experimenter to prevent the frustration of his "object through want of foresight and needful pre-"paration," such as that which you have pointed to?—Yes.

17. Do you think that when he has already thoroughly satisfied himself of the solution of any physiological problem he is justified in repeating the experiments, however mercifully they may be conducted?—I believe he is not justified in repeating experiments after he has satisfied himself upon the question at issue.

18. Now even for those purposes, such limited purposes as you have pointed to, do you consider that experiments are necessary?—I do.

19. Are the experiments that you refer to always very painful?—No; I believe that most of the experiments may be conducted with very little expense of pain to the animal.

20. During the course of your experience anæsthetics have been introduced?—Yes.

21. Have they made a great difference in the degree of pain to which a humane experimenter, such as you speak of, would subject animals?—A very great difference indeed.

22. Are the great majority of experiments such as can be rendered altogether painless?—I think so. There are some experiments in which the manifestation of pain is the very test of the matter in question, and these cannot therefore be rendered painless.

23. But the great majority of experiments can, can they not, be rendered entirely painless?—Very nearly, if not entirely, painless.

24. There are some experiments, are there not, in which, though they cannot be rendered entirely painless, yet the pain can be so much mitigated as that only a small portion of the experiment shall be painful?—I believe that to be the case with most of the experiments.

25. With regard to those of which, as you say,

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* Appendix III., § 1.

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pain is the test, do you think it is justifiable to subject an animal to lingering pain ?—Not to lingering pain that can be avoided.

26. But is lingering pain ever justifiable ?—No, I think not.

27. Do you think that the eminent persons who are at the head of the two branches of the medical profession would be as much disposed to promote humanity as any other eminent persons in the country ?—I may venture to say that I think they would be as entirely desirous of mitigating pain as any other persons could be.

28. If then what you consider abuses prevail, and it should appear that by reasonable measures those abuses could be prevented, do you think that the Crown and Parliament might look for the support of those eminent persons in carrying those measures ?—I am quite prepared to believe that they might look confidently for such support.

29. Would you say that any experiment that can be performed under anæsthetics ought certainly to be so performed ?—Yes.

30. And that to neglect to entirely destroy, or greatly to diminish, the pain of even a necessary experiment was an abuse which ought to be repressed ?—Yes, I quite think that.

31. You have been for many years, I think 50 years, in the medical profession ?—Very nearly, within two or three months.

32. Having attained to the eminence which I will not ask you to describe, and which we know without its being described, may I ask you whether you yourself have ever been a practiser of these experiments ?—No, I have never been a practiser, nor can I say that I have ever seen one of the experiments.

33. Then we may take it that, although you have stated it to be necessary in some cases and for some purposes that these experiments should be performed, yet a man may attain to the degree of eminence to which you have attained without ever having seen any of them ?—Certainly I do not remember ever to have seen an experiment on animals at all.

34. Nevertheless, you do not think it would be possible that we should have arrived at our present state of medical and surgical knowledge without some such experiments having been made ?—I think we could not have reached our present surgical or medical knowledge without such experiments.

35. Will you have the kindness to give us some illustrations of that opinion ?—It is difficult sometimes to adduce the link which connects the experiments with the future results, but I may say that the discovery of the circulation of the blood by Harvey is an instance in point; if the circulation of the blood had never been discovered (and its discovery was aided and perfected by innumerable vivisections, in the strict meaning of that word, made by Harvey himself) we could never have ascertained or learnt either the surgical or the medical cure of aneurisms, for example, which would infallibly without such cure destroy human life, and produce much human suffering. Harvey himself dwells in anticipation upon the therapeutic fruits of his great discovery. He speaks of having got rid of a large and dangerous tumour by cutting off the supply of blood by tying the little artery which fed that tumour. That is one instance. And so again I should say with regard to the discoveries resulting from the experiments made by Sir Charles Bell and Doctor Marshall Hall upon the nervous system, they have enabled us to direct our therapeutic influences, the influences of drugs (which have been ascertained in far other ways), so as to soothe pain, and to diminish very much the danger to life of many very painful diseases. So again (as I have seen somewhere remarked, and I think very much to the purpose) there were experiments on living animals by the late Doctor Hope, which enabled him to ascertain the causes of certain morbid sounds of the heart; and thereby, knowing those causes, to address to persons who presented those sounds the proper method of appeasing their sufferings, and of promoting, if possible, their recovery. These are instances which suggest themselves at once to me; but there must be innumerable other cases in which the discoveries made by scientific inquirers, through experiments on animals, have been conducive to human comfort, and to the prolongation of human life.

35*a*. (*Lord Winmarleigh.*) Did Harvey ever state the animals on which he made these experiments ?—Yes; and they were very numerous.

35*b*. And the discoveries as they were made by each successive experiment ?—I do not know that that is specified minutely in his works; but he states that he examined the double hearts of vertebrate animals, and the single hearts of many of the lower animals, snakes, molluscs, and so on, snails and beetles, and other creatures.

35*c*. Would it have been possible to make experiments of that kind without the use of anæsthetics; I mean without pain ?—No, not without pain, for at that time anæsthetics were not discovered.

35*d*. (*Chairman.*) But now anæsthetics would enable you to perform some or perhaps all of those experiments while the animal was in a state of unconsciousness ?—Yes, I think many of them certainly.

35*e*. Are experiments upon animals resorted to for the purposes of medico-legal investigation ?—Yes, I believe they are; to investigate the operation of poisons, for instance.

35*f*. When new drugs and supposed remedies are discovered, is the effect of those drugs and those supposed remedies tried in the first instance upon those animals ?—I do not know whether they are much tried in that way; but I confess I should not have much confidence in any results from such trials, because it is well known that the effects of the same drug are often very different indeed upon the human subject and upon the animal.

35*g*. It would be therefore quite possible that a painful experiment or series of experiments might be tried, and the result, so far as the human subject is concerned, might be worthless or misleading after all ?—Yes, I think so.

35*h*. I think you are not personally much aware, are you, of the extent to which experiments upon animals are carried by persons who are not what you have described as competent persons ?—No, I have no personal knowledge on that subject.

35*i*. If a severe experiment is performed of a complicated character, may it not very often happen that nothing at all is proved by that complicated experiment ?—Certainly.

35*j*. Would you not consider that a great abuse ?—A very great abuse indeed.

36. And one justly to be repressed ?—And one justly to be repressed.

37. Now if experiments are performed by unskilled people, is it not probable that the result will be without any object ?—Most commonly I should think, without any utility.

38. And therefore in any point of view they are to be condemned and repressed ?—Yes.

39. Supposing an experiment to be painful, and the fact which results from the experiment to have been already proved to the satisfaction of the scientific world, does it appear to you to be justifiable to repeat such an experiment ?—I think it would be quite unjustifiable.

40. And that anything of that sort ought to be repressed ?—I think so.

41. Should I be right in summing up the general purport of what you have been so good as to say to us in something like these words, that in your opinion it is not possible altogether to denounce the trying of occasional experiments upon living animals ?—I think it would be very undesirable that all such experiments should be repressed.

42. But admitting that, it is your opinion that, now that anæsthetics have been discovered, the great majority of experiments may be made when the animal is in a state of unconsciousness ?—Yes, I think so.

43. That those experiments which are not entirely painless may, many of them, be rendered comparatively painless by the proper use of anæsthetics ?—Yes.

44. That if, in a very few cases comparatively, pain is the test of the question at issue, at any rate lingering pain is under no circumstances justifiable ?—That is my opinion.

45. And is it your opinion that it is the wish of the most eminent men of the medical profession in both its branches to prevent abuse, and to repress cruelty, as much as possible ?—Yes, decidedly.

46. And that the Crown and Parliament may look for the support of those eminent men in any reasonable measures which they may adopt for that purpose ?—I am confidently of that opinion.

47. (*Lord Winmarleigh.*) You say that there are certain experiments, the test of which is pain. Are there any experiments, except those which are connected with the nerves, in which that is the case ?—Pain presupposes that the nervous system is concerned in the experiment,—pain is felt through the nerves, and through the nervous system only.

48. Do you happen to know what has been the nature of the experiments in those cases ?—Some of the experiments of Sir Charles Bell were of this nature. He wished to ascertain of two branches of a nerve which, if either, ministered to sensation, which, if either, ministered to motion alone; and of course it was necessary that he should produce pain in order to test one part of that experiment. The manifestation of pain by the animal experimented upon would show that the nerve then interfered with was the minister of sensation.

49. We may presume that in those experiments the use of anæsthetics is quite useless, that it would be of no effect ?—They would defeat the object of the experiment.

50. Do you think that those are very material with reference to the application to man ?—They have shed much light upon the nature of many of our diseases, and therefore have thrown a corresponding light upon the method of treating them. Anæsthetics, as we are familiar with them, were not known when Sir Charles Bell's experiments were made.

51. Was it necessary in making those experiments to subject the animals to any lengthened pain, what Lord Cardwell has termed lingering pain ?—No, there was no necessity at all; if he was satisfied that he was dealing with a nerve which ministered to sensation, there was an end of the experiment.

52. You believe then that experiments of that kind may be conducted without lingering pain ?—Without lingering pain.

53. (*Sir John Karslake.*) Are there any cases besides those that you have mentioned in which the use of anæsthetics frustrates the object of the experiment ?—In all those cases where the manifestation of pain constitutes the test of the experiment anæsthetics would interfere with it.

54. I wished to ask you whether any other cases suggest themselves to you, than those which you have last described, in which the use of anæsthetics would frustrate the object of the experiment ?—Not that I know of.

55. Not in the case of administering poison intentionally ?—I should think that the use of anæsthetics might vitiate the results of any trials of poisons upon animals.

56. Probably except in those cases anæsthetics might always be administered, except where the object was to ascertain whether pain was given or not ?—The object, I presume, of administering poison to animals for scientific purposes might be interfered with by the operation of the other poison; for anæsthetics are to a certain extent poisons themselves.

57. (*Lord Winmarleigh.*) But does it follow that because any drug would be poisonous to a dog it would necessarily be poisonous to a man ?—No, not at all.

58. On that account I think it was that you said that you had no great faith in experiments made of the effects of drugs upon animals ?—No, I should not have any faith in such experiments.

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59. (*Sir John Karslake.*) Still the effect of a particular drug upon animals would be beneficial, would it not, with a view to ascertain what the effect of the drug would be upon animals whose lives you hoped to preserve, in consequence of the use of that experiment ?—I have been looking at the question in the light of the benefit which might be produced to the human race by such experiments.

60. But if you can save pain to a large number of animals by experimenting upon one, would you think it justifiable to do so ?—Certainly, I should.

61. (*Mr. Forster.*) A question that Lord Cardwell asked you was with regard to experiments upon living animals. In your answer did you include such experiments as giving animals a disease in order to watch the operation of that disease ?—I do not think that experiments would ever enable one to learn anything in that way. The experiment of giving diseases to living animals is, I think, justifiable upon certain other views.

62. But when you stated that you yourself had not only never practised these experiments, but had never witnessed them, did that include a statement that you have not witnessed experiments in which animals have had diseases given to them ?—Yes, I have never witnessed experiments of that kind.

63. What anæsthetics would you recommend to be used in the case of experiments upon animals ?—Chloroform is the one of which the efficacy is best understood.

64. (*Mr. Erichsen.*) Although you have never performed any experiments or witnessed them, you have used the results of the experiments of others, have you not, as the basis for the advance of your professional knowledge ?—I have made myself acquainted with the experiments and their results, and have turned them to such use as I could.

65. I mean such experiments as Majendie's on the nerves ?—Yes.

66. I suppose one may also gather from what you have stated that we may look upon a new era as having been introduced in experimentation on animals since the introduction of anæsthetics ?—Yes.

67. And that many of these experiments on the nervous system, which undoubtedly were of a necessarily painful character, such as those of Sir Charles Bell and of Majendie, it would now be useless and indeed cruel to repeat, inasmuch as the results are established ?—Exactly.

68. You look upon them as a decided gain to science, and these experiments need never be repeated ?—I should think it cruel and unjustifiable to repeat them, to determine any point which has already been satisfactorily ascertained.

69. (*Mr. Forster.*) I should imagine, from the answers which you have already given to Lord Cardwell's questions, that you would think it a reprehensible practice to use experiments upon living animals as an illustration in medical teaching, to use them for teaching a class ?—I do.

70. And therefore you would not think that in merely teaching either the principles or the practice of medicine or surgery, there ought to be an illustration by experiments ?—Not of any point which has been already ascertained.

71. But even where it has not been ascertained I should rather gather from your answers that you would say that these experiments ought to be conducted by experts, with a view to obtaining knowledge themselves, rather than as a mode of teaching classes ?—Yes, certainly.

72. (*Mr. Hutton.*) You said that anyone who performed experiments of this kind ought to be a good anatomist, and to have trained assistants. Now would you justify experiments of this kind for the purpose of training surgeons, I mean in order that young men who would not be able otherwise to have the proper command of tying an artery, for instance,

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should do it on an animal to get their hand in ?—I should doubt myself whether it would be of any service to them.

73. You would think that for that purpose, therefore, it is not justifiable to make these experiments ?—I should think it decidedly unjustifiable.

74. Then you say that the majority of experiments can be rendered entirely painless. Now, is it not true (as I have been told) that on the lower animals the effect of these anæsthetics is so powerful, say on frogs, for instance, that it is hardly possible to administer them ?—That I do not know at all; I have no knowledge on that subject.

75. Can you tell me anything about the particular action of an anæsthetic which is sometimes called curare and sometimes woorari ?—I know it has been said that the woorari poison, which renders the animal quite unable to move its limbs, has been used to veil, and so to conceal from the observers, the pain which the animal nevertheless suffered ; but that any such fraudulent trick has ever been practised by the physiologists of this country I decline to believe.

76. I do not imagine that that is what is usually stated, but that it has been used to keep the animal perfectly motionless. Are you acquainted with this "Handbook for the Physiological Laboratory" which I hold in my hand, edited by Dr. Burdon Sanderson ?—No, I have never seen it.

77. In this handbook it is constantly named as being used, and apparently for the purpose—for instance, in experiments on the arterial system—of preventing the failure of the experiment by the motion of the animal ; but I find the strongest possible evidence in Claude Bernard's books that it has no effect whatever on the nervous system ; that it simply paralyses the motor system ?—If that is all it does, then it does not enhance the animal's sufferings.

78. It simply renders it motionless, and leaves the suffering alone ?—And would facilitate probably the experiment itself.

79. (*Mr. Forster.*) Then you would not consider that it ought ever to be used as an anæsthetic ?—No, if it is not an anæsthetic.

80. (*Mr. Hutton.*) You do not know from your own experience whether it is an anæsthetic or not ?—No, I do not know anything about it, more than that it has the effect that you have described of depriving the animal of the power of motion.

81. (*Chairman.*) In your judgment it is not an anæsthetic, is it ?—I imagine it is not an anæsthetic at all.

82. And to treat it as an anæsthetic is a fraudulent trick in your view ?—It would be.

83. Then, in short, you have described that as a fraudulent trick ?—If it were used merely to deceive bystanders into the belief that there was no pain suffered, I should call it a fraudulent trick ; but it might be used for a much better purpose, namely, to facilitate the aim of the operation.

84. (*Mr. Hutton.*) You were saying that the circulation of the blood had been discovered by Harvey by vivisection. Is it not true that he was led to it by a great many other observations before he verified it in that mode ; that it was a verification rather than a discovery, in fact ?—Yes ; but I think the discovery was rendered perfect by his vivisections.

85–6. Was not the microscopic verification of it in the frog's leg even more perfect ?—He looked at the hearts of very small animals indeed ; slugs and snails, which have a single heart only.

87. Still that was not the only mode in which it could have been verified ; it might have been verified by microscopic research ?—Pardon me for saying that that microscopic research implied in the first instance the cutting open of the animal.

88. Not in the case of a frog's leg, a part of which is transparent, is it not ?—Yes ; but his inquiries related principally to the condition of the heart, and of the great vessels which lie close to the heart.

89. I find that a great many of these experiments are given for histological purposes, for the purpose of studying the cellular tissues, and they take place on the living animal for fear that the process should be changed by death. Now, would you justify experiments of that kind for a remote advantage in the study of the science of histology, that it may lead to some future beneficial discovery ?—I hold the opinion that the experimenter upon animals ought to have some definite object in his mind, the attainment of which would be to the benefit of the human race.

90. That there should be some immediate prospect of alleviating suffering by the experiments, you mean ?—Yes.

91. (*Chairman.*) In short, that the inflicting of pain upon animals is a great evil ?—A great evil.

92. That it cannot be justified unless it be by the direct prospect of some greater good ?—Yes.

93. And that that greater good is not a general roving inquiry, but that it must be, to be justifiable, the discovery of some object directly and greatly beneficial to the human family ?—Yes, or the solving of some doubt.

94. (*Mr. Hutton.*) When you said that you could not justify the infliction of lingering pain, what do you mean by "lingering" ?—Pain which might follow the operation, and might remain after the influence of the anæsthetic had departed.

95. For instance, for two or three weeks ?—Or even for two or three hours ; for any time, I should say, lingering pain which could be avoided should be avoided.

96. But supposing the object is to study an induced disease, it is almost necessarily lingering pain, is it not ?—I do not know that experiments are ever made with that object of studying an induced disease.

97. I understood that with regard to the grouse disease a good many experiments of that kind had been made ?—I am ignorant of them, and cannot pronounce any opinion upon them. Mr. Forster asked a question just now which had relation to the induction of disease.

98. (*Mr. Forster.*) I will give you what I have heard to be an object in experiments—to find out the effect of very minute choleraic poison, to discover whether it was in water, for what you may call medical sanitary purposes ?—As far as I understand the object of inducing disease in experiments upon the lower animals, it seems to be (and if I take the right view of it, it is, in my opinion, a very justifiable object) with a view of discovering the producing causes of certain diseases ; and probably with the discovery of these producing causes, the discovery also of the means of destroying those causes, and therefore of preventing such diseases in future.

99. But that is a process of investigation of which you have no personal knowledge ?—I have no personal knowledge of it.

100. Might I ask you what distinction you would draw between vertebrate and other animals as regards their sensitiveness to pain ?—I do not know at all that there is any difference ; but the reason for having recourse to the lower animals in Harvey's investigations was that he might see what happened with respect to a single heart.

101. But do you yourself imagine that a snail would suffer as much pain as a dog ?—I do not know.

102. You have no opinion about it ?—No settled opinion. There are persons who think that we have some grounds for believing that the inferior animals do not feel so sensitively as the more highly organized.

103. There is a sort of popular notion that a warm-blooded animal feels more than one that is not. Is there any scientific foundation for that opinion ?—I do not know how that can be ascertained. We know what Shakspeare said, but I imagine he was wrong.

104. (*Mr. Hutton.*) Has there been much progress in this kind of investigation during your lifetime ?—If you mean by experiments made on living animals, I think that is true of all those experiments, particularly those which were made with

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respect to the functions of the nervous system by Sir Charles Bell and by Dr. Marshall Hall.

105. I do not mean good results, but do you think that the practice has largely increased in the profession of studying by this mode?—I do not know.

106. (*Lord Winmarleigh.*) You said that you had no knowledge whatever of the extent to which what you have termed vivisection is carried on in England? —No, I have not; but I am confident that there cannot be many persons in this country who are qualified, or who ought to be allowed, to perform such experiments as are under discussion now.

107. Have you ever considered in your own mind how, supposing some legislative enactment was to be passed, the object of limiting it to competent persons could be best carried out?—I cannot say that I have given much consideration to that subject, but it strikes me that any persons who should prosecute physiological knowledge by experiments on animals, ought to be such persons only as might be licensed by some high authority to do it.

108. You would have no other restriction placed upon them, as to the necessity of having a witness by on behalf of the public?—I have not considered what the precautions should be, but only how the experiments might be licensed or allowed.

109. Would you allow anybody who possessed a license of that kind to perform these experiments at his own discretion?—Yes, I think so, if he were licensed by some high authority, which authority was well advised by some responsible person.

110. What person?—I am thinking of the Inspector of Schools of Anatomy. He is a person who exists now, and who is the adviser, I imagine, of the authority; I do not know of what high authority, whether it is the Home Secretary or not.

111. Would you restrict the issuing of licenses to that one authority, or would you place it in the College of Physicians or Surgeons; or in what central authority would you place the power of issuing them?—I think it should be in some high authority, such as one of the Secretaries of State, properly advised by a responsible officer as to the eligibility of the person who seeks the license.

112. (*Chairman.*) Subject to the usual practice of this country as regards responsibility to Parliament? —I may perhaps be quite wrong and presumptuous in saying so, but it seems to me that this vivisection question is scarcely a fit one for discussion in a popular assembly like the Houses of Parliament. I think it could hardly be debated without giving rise to much misapprehension and prejudice, and angry passionate feelings which had better be avoided altogether.

113. (*Lord Winmarleigh.*) What should be the nature of the authority which the Secretary of State should rely on?—He should rely on the responsible officer to whom I have alluded, such as exists now as Inspector of the Schools of Anatomy, who regulates the supply of subjects for dissection.

114. Is that a very high authority in the medical world?—Yes.

115. Is that a higher authority, for instance, than the President of the College of Surgeons, or the President of the College of Physicians?—I think that it would be a very invidious task for either of those persons to undertake.

116. (*Mr. Erichsen.*) You would have an inspector of physiological laboratories as you now have an inspector of dissecting rooms and schools of anatomy? —Yes; I think that would be a great safeguard; and I think that with such safeguard it might be pretty safely left to the honour and conscience and humanity of the experimenters themselves. I would rather that it should be left in that way than that they should be restrained by the fetters of a statute which might be, perhaps, more easily evaded.

117. (*Mr. Forster.*) You are aware that a great deal of the feeling about this matter in this country arises from the belief, whether well founded or not, that these experiments are tried very frequently on a great number of animals in some places on the continent, do you think that it is more practised by physicians or surgeons or by men of science on the continent than it is in England?—I think so from what I read and hear, but I have no exact knowledge on that subject.

118. (*Chairman.*) When you speak of a license being given, for instance, by the Home Secretary, on proper advice, to anybody to perform an experiment, do you mean that he should become a licensed person for the general performance of experiments, or that he should apply *toties quoties* for a license for each experiment or series of experiments?—I do not think that it should be *toties quoties*, but a general permission.

119. Subject to revocation if it came to be known that there was an instance of abuse?—No doubt.

120. (*Mr. Forster.*) Have you ever considered this alternative: supposing that there was to be legislation instead of licensing particular persons, giving them power to make experiments, that there should be a strengthening of the present Cruelty to Animals Act, so that the *onus probandi* should be thrown on a person who was discovered to be making experiments to prove that he was a fit person, and was doing it with a proper object?—That is a point which I have never considered.

121. You have never considered that alternative to actually licensing persons for the experiments?—No; indeed what I have said with respect to licensing persons has been only in my mind this very morning since a conversation I have had with Sir George Burrows.

The witness withdrew.

Sir George Burrows, Bart., M.D., called in and examined.

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122. (*Chairman.*) You are the President of the Royal College of Physicians?—I have the honour to be so.

123. And late President of the Royal Medical and Chirurgical Society?—I was.

124. And late President of the General Medical Council of the United Kingdom?—I was for five years.

125. Have you had any personal experience of this subject, the trying of experiments upon living animals for scientific purposes?—I have had limited experience. That experience consisted partly of witnessing experiments made by others, and partly in this, that I performed some experiments some years ago myself.

126. Will you state in a tlitle more detail the nature of the experiments?—My experiments were to elucidate the condition of the brain in different forms of death. There having been a considerable discussion in the medical profession for many years as to the condition of the bloodvessels of the brain under different pathological circumstances, these experiments were made by destroying animals, either by strangulation, that is by hanging, or by hæmorrhage, or by poison, to ascertain what was the condition of the bloodvessels of the brain in those different modes of dying, with a view to elucidate the pathology of the different modes of death in human beings; but the animals all died in the experiment. Death was accomplished perhaps in a minute or less.

127. (*Mr. Forster.*) In no case longer than a minute, should you think?—If a small animal dies by hæmorrhage it sometimes takes longer.

128. Would you say that it was in no case five minutes?—No; certainly it was not.

129. What animals were they?—Rabbits principally; sheep sometimes.

130. But the suffering of the animals was not in

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any case longer than five minutes, and generally not so long?—Certainly never five minutes; indeed, I should think the suffering was not so long as that, because the mode of death caused cessation of feeling before the animal was absolutely dead.

131. (*Mr. Erichsen.*) Was there more pain than in sticking a pig, or any butcher-like occupation?—Not so much, because there was no division of the integuments.

132. (*Chairman.*) Will you give the Commissioners an idea of the other experiments which you have witnessed?—The other principal experiments that I have witnessed were experiments that were performed some 30 or 40 years ago by Doctor Charles B. Williams and the late Doctor Hope, to determine the cause of the sounds of the heart and its motions. The motions of the heart were pretty well determined by Harvey in his experiments on living animals, but the causes of the sounds resulting from those motions, which since the use of the stethoscope we have been able to hear by applying it to the chests of living animals, were not known till those experiments were performed. It is very well known that on listening on the walls of the chest in a healthy person there are certain sounds; but the causes of those sounds were not understood before these experiments were made; and also it is known that on listening on the walls of the chest there are very strange modifications of these sounds when people are diseased; and the cause of the sounds was not understood. These experiments were performed on living animals, which were rendered insensible before the experiments were commenced, and destroyed after the experiments were completed.

133. How were they rendered insensible?—I think it was worari poison: it was before the time that chloroform was known. I am not sure of all the various substances used, but I know that worari was used, and I believe at the suggestion of the late Sir Benjamin Brodie. The animal was rendered quite insensible, and then the heart was exposed and its movements were seen; and by placing the stethoscope on different parts of the heart we were able to hear the sounds generated in particular parts of the heart; then further experiments were made, while the animal was insensible, by introducing fine curved needles through the walls of the vessels and drawing back the valves which immediately modified the sounds, and gave us sounds perfectly analogous to, if not the same as, the sounds often heard in the human frame when the heart and its valves are diseased; and from that time to this the diagnosis or distinguishing from the sounds of the heart the changes of structure which have gone forward in the valves has become a certainty; whereas before that it was confusion; and in that way great benefit arose to human beings in regard to the treatment of diseases of the heart, and the advice which can be given to them. It is hardly necessary, I think, for me to go more at length into that subject.

134. Then I understand your opinion to be that some experiments are necessary for progress in medical science?—I think they are absolutely necessary for the progress of medical science, and I have myself both seen and performed experiments which, I think, have tended very much indeed to advance medical science, and to the better treatment of certain diseases.

135. Since the period that you have last spoken of great discoveries have been made in anæsthetics?—Very great indeed.

136. Was the poison which you spoke of the use of really an anæsthetic?—It rendered the animal insensible; it made no sign of suffering.

137. It is not considered a genuine anæsthetic, is it?—No, because it is actually a poison; it destroys life.

138. But now that anæsthetics have been discovered these experiments might have been performed without exposing the animals to any suffering at all?—I think entirely. Perhaps I may remind you that with respect to the circulation of the blood, the importance of which discovery will not, I suppose, be underrated, that physiological knowledge could never have been arrived at except by experiments on animals both cold blooded and warm blooded; and very recently an oration was delivered by Doctor Guy, as Harveian orator, in which he recited some of the experiments by which Harvey arrived at the full conviction of the circulation of the blood, and to perform which experiments Charles the First put the animals in the royal parks at the disposal of Doctor Harvey, in order that he might carry out these investigations; and when he had arrived at such a point, that he felt he could demonstrate the thing, Charles the First and his Queen, with the princesses, attended a lecture of Harvey's to witness the circulation of the blood proved upon the living body.

139. Would you recommend that example as one to be followed now?—No, I would not; but I mean that it is nothing new in this country that experiments should be made on living animals for the purpose of the advancement of science.

140. Now anæsthetics having been discovered, and having got to the perfection to which they have now been brought, may most of the experiments which are necessary or useful be employed while the animal is entirely unconscious?—I should think nearly all; some few cannot be, particularly those which relate to the nervous system. If you want to ascertain the function of a particular nerve, if you put the animal into a complete state of anæsthesia you would defeat the purpose of the experiment; but those experiments are most of them not of a serious nature.

141. Take for instance such an experiment as those performed by Sir Charles Bell on the nerves of the face, would that be of a painful nature?—So slightly so that you would hardly think it necessary to put an animal or anyone else in a state of anæsthesia to perform it.

142. If performed on a human being for any object, would you use chloroform?—No. I think the utmost a surgeon would do in such a case would be to direct a little ether spray on the surface to deaden the sensibility; but these experiments of Sir Charles Bell, which have been of such use to us in the treatment of disease, arose in this way. It was known that on the side of the face of animals and of man there is a very ample distribution of nerves; but before Sir Charles Bell's experiment there was confusion as to the functions of particular nerves; and he proved that some were nerves directing the movement, and others were nerves communicating sensation; and others, nerves for the nutrition of certain parts; and being a good anatomist, by making a section half an inch long, he was able to determine whether the nerve was one of motion or of sensation. Very important results have followed from that experiment, inasmuch as we often have to treat people who have paralysis on one side of the face; and we may judge of the severity of the case or the danger to be apprehended, according as it is one nerve, or two nerves, or all the nerves simultaneously affected. A person may have a paralysis on one side of the face, and it is simply a paralysis of the facial nerve which comes out behind the ear, and we can tell the person, "Well, this is a trifling affection; there is some little "pressure on your nerve, which can be easily re- "medied." We go to another person and find the same expression of countenance, but at the same time there is a loss of sensation; and we say, "This is "serious; there is some mischief going on in the "central portions of one hemisphere of the brain; "this is a thing which is attended with danger, and "must be treated on totally different principles." It is experiments of that kind which since Sir Charles Bell's time have directed physicians to the proper treatment of particular forms of paralysis.

143. In this particular form of experiment, had the same thing been done on a human being the pain would be so slight, I understand, that you would not think it necessary to use chloroform?—I am quite sure the pain would not be greater than the prick of a spur on a horse's side, because it is momentary, and done with a very sharp instrument, and there is an end of it.

144. But I understand you to say that the great majority of experiments can be performed under chloroform in such a way as that the animal shall be entirely unconscious ?—Yes ; and in the great majority of those which cannot, the more painful part of the operation can be performed under anæsthetics, and then the remainder carried out.

145. Supposing an experiment to be of a very lingering character, would you think that it was justifiable at all ?—It would depend upon the importance of the object to be attained very much.

146. May I understand that the sentiment of the leading men in the medical profession in both its branches is as likely to be favourable to humanity as the sentiment of eminent men in any other department of science ?—Of course it is difficult to form an opinion upon that matter ; but my opinion decidedly is that the members of my own profession, in both branches of it, are naturally as humane and likely to be as humane as any other men.

147. And that they are likely to consider that any inhumanity or any abuse of a practice of this nature is discreditable to the profession, and ought to be put down if possible ?—I think there would be a general condemnation of any abuse of it.

148. And a general disposition to support any reasonable provisions to check abuse ?—Yes ; I think there would be a readiness to assent to any reasonable proposition to check abuse.

149. And that incompetent persons should not be permitted to set themselves to the performance of experiments ? — I think that incompetent persons, persons who have not a thoroughly good knowledge of anatomy and physiology beforehand, ought not to be allowed to undertake those experiments.

150. That that is simple cruelty without any useful purpose whatever ?—By accident a useful purpose might result ; but it is not a justifiable proceeding. Unless a man has prepared himself beforehand by acquiring a very sound knowledge of anatomy and as sound a knowledge of physiology as he can, I do not think he is justified in making experiments on any animal.

151. And that it would be a reasonable thing that the Crown and Parliament should interfere to prevent any practice of that sort on the part of uneducated and incompetent people ?—I think there should be some sort of interference with incompetent and uneducated people attempting experiments on living animals certainly.

152. Have you considered at all any particular mode by which it would be most wise to endeavour to effect that object ?—I think that is by far the most difficult question which you have put to me. It would be extremely hard to say that if scientific men wanted to perform an experiment on a frog or a rabbit, they must immediately go to some authority established by the legislature and ask permission to perform it. I think that that would be an interference with the progress of science, and would be more than would be justifiable in my judgment. On the other hand, I think that in the case of persons who wish to undertake a series of experiments on living animals, or who intended to perform a series of experiments on living animals, it would be quite proper that there should be some controlling and restraining authority ; but I think that an incessant interference, and the necessity for every individual experiment of applying for authority to the Secretary of State or any other authority, would be almost too great an interference with individuals.

153. I think I collect from you first that some experiments on living animals are in your opinion necessary for the progress of science ?—I think there is no doubt of that.

154. And probably for medico-legal investigation ?—I think there is no doubt of that.

155. And you think that, in consequence of certain statements which have come before the public, there is an exaggerated idea prevalent as to the painful nature of the majority of experiments ?—I do think from the conversations I have had, not with scientific people, but with humane people whose feelings are very acute and who are horrified at the idea of cruelty being practised on dumb animals, that they have an idea that the practice of what is called vivisection is carried out to a much greater extent than it really is ; and I believe that many of those persons, who are persons of strong feelings and very humane in their nature, are under an impression that vivisection means the dissection of living animals ; they think that these operations upon the living body are somewhat analogous to that of dissection on the dead body. I am sure that there is a confusion in their minds on the subject, and it ought to be made well known to the public that really what is called vivisection very often is something so trifling that, as your Lordship put it to me just now, you would never think of employing an anæsthetic to perform it ; and also experiments that are performed on animals very often do not imply any cutting at all ; no instrument is used ; you have to administer a medicine or a poison to see what the effects are. Now we know pretty well what the effects of strychnine on an animal are ; but an experiment might be performed to show the effects of strychnine on the nervous system or the muscular system, and there is no cutting. So also an experiment might be performed to see the effect of inoculation ; you might inoculate an animal ; if you inoculated a human being to prevent some particular disease you would hardly call that vivisection.

156. But you are probably aware that in some cases at least, either here or abroad, very severe, and what would by most people be called extremely cruel, operations have been performed on animals ?—I am quite aware of that.

157. And those are what you have hitherto referred to, when you have spoken of the abuses ?—I think there have been great abuses in the performance of operations and experiments on living animals.

158. And that those abuses ought to be restrained ?—I do think so.

159. And that the knowledge of those abuses is what has led to the strong feeling which is entertained by a portion at least of the public on the subject ?—Partly that, and partly, I think, ignorance of the subject.

160. And that the majority of experiments could be performed while the animal was entirely unconscious, and that they ought to be so performed ?—I think that they could be so performed, and ought to be so performed.

161. And that the highest scientific men would be quite as much disposed to arrive at that conclusion as any other portion of the public ?—I do not know whether they would as readily as any other portion of the public, because they see the difficulties in the way. Other people are governed simply by sentiment, and wish to avoid pain to dumb animals, but they do not know the difficulties in the way ; whereas scientific men see that there are great difficulties in the way.

162. But if the great majority of experiments can be performed while the animal is entirely unconscious, what is a great difficulty in securing that that shall be done in such cases ?—That is to say, what is the difficulty of securing that the operation is performed under anæsthetics. I think I have said already that there are a certain class of experiments which cannot be performed under anæsthetics, but that the great majority of them most undoubtedly can be performed under anæsthetics.

163. If that be so, is it not reasonable that any experiments which must inflict great pain should be performed under some special sanction ?—Yes ; it is quite reasonable and proper.

164. Although you think it may be difficult to arrive at a conclusion as to what that special sanction should be ?—There is the difficulty, I think.

165. Have you thought about the subject at all, so as to be able to assist us on that point ?—I think that, whatever controlling authority there may be, it should

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be one that would, to a certain extent at any rate, command the respect and confidence of scientific men. I do not think, for example, that the Attorney-General prosecuting, or the action of the Home Secretary, would alone be sufficient, unless the scientific world could be assured that that legal functionary or that minister of state had been properly advised upon the matter. I think that the scientific world would think it a great hardship if they were to be restrained, unless they were restrained by some one who really had a competent knowledge to enable him to know when he ought to exercise that authority.

166. But supposing that that legal functionary, or minister of state, acted under responsibility and with the highest professional advice, you would consider that that was a proper sanction ?—Yes; but I do not know whether there would be any security that he would have competent scientific advice; that is the danger. You see if you take a gentleman occupying the high position of a Secretary of State for the Home Department, there is nobody, so far as I know, in that office to advise him on such subjects at all; and he would naturally, when a question of this nature came before him, go home and send for his general practitioner, Mr. A, B, or C, and ask him what he thought about the matter. I should not say that that was competent advice.

167. You would think it ought to be responsible advice ?—Yes; I think there should be some person to advise him, having functions somewhat analogous to the Inspector of Anatomy. Anatomy in this country was at one time conducted in the most scandalous and disgusting manner in every way; and now the anatomical schools are conducted in a totally different way. There are certain places licensed for dissection, and the Inspector of Anatomy has the power of going in at any time to see that everything is conducted with propriety, and to examine the book in which a record of all the bodies is entered, and the circumstances where they come from, and so on, and also that the remains of the bodies are properly interred; and it seems to me that there might be either the Inspector of Anatomy, or an officer having analogous functions, with reference to these physiological experiments; somebody who would be called upon to investigate the matter, to send in a written report; and upon which written report the Secretary of State, or other authority, would act. Then if the action be called in question, there would be a document from a person competent to give information on the subject, to satisfy the scientific world that there was nothing arbitrary or captious in the proceeding. I think that if the legislation proposed were that the Secretary of State for the Home Department should not have the power of interdicting or allowing these things without the scientific world and the world at large knowing that he had taken pains to obtain advice, and that advice could be brought forward if called for, that would be much more satisfactory than leaving it to the *ipse dixit* of the Secretary of State.

168. There were difficulties formerly in regard to anatomy, I understand you ?—Very great difficulties.

169. And you say that the practice was in some cases scandalous ?—Yes.

170. Which has been remedied ?—Yes, by the Anatomy Act.

171. The same general course of proceeding which has overcome the difficulties in regard to anatomy is what you would suggest with regard to the difficulties of the present case ?—I think they might be overcome in that way. What I think is that there should be some kind of competent authority, such an one as would command respect, so that one could say, "This is not "a captious system, but it is founded on proper "information, and which could be produced to the "scientific world," and so that the Secretary of State could say, "This is the information which I have "before me, and I exercise my judgment upon this "information."

172. So long as it was a responsible opinion founded upon proper professional warrant and information, you would see no objection to the interference of the Secretary of State ?—I think not. But another thing strikes me. Of course, in a country like our own, where the Government changes from time to time, and the men who occupy high offices pass through them, one first and then another, one man may have prejudices one way, and another another; and if the advance of physiology is to be dependent upon the feelings or opinions of an individual, I think that would be very unfortunate. I think that there should be some officer or person appointed, who is a fit person, a good anatomist and a physiologist himself, and who is competent to form an opinion, and that his opinion should be given to the Secretary of State; and that this individual should be a permanent officer as long as he conducted himself properly; *quamdiu se bene gesserit;* but I think it would never do to leave it to a gentleman occupying the position of Secretary of State unless he had good information to guide him.

173. (*Sir John Karslake.*) I understand that you would rather suggest that a person who wished to carry out these experiments should make application to the Secretary of State, who should refer his application at once to such an officer as you have mentioned. Would that be the process ?—There might be certain places licensed for the performance of physiological experiments, as you have certain places licensed for dissection; and then of course the place would be sanctioned by authority, and experiments would be performed there because it was licensed to have experiments performed there. I do not mean to say that that is the best way. I do not presume to say that I have thought the matter out enough.

174. I rather understood you to say that the cases in which vivisection ought to be practised are comparatively rare ?—Yes.

175. That wherever the thing has been clearly established already, you would never have vivisection performed merely for the purpose of confirming what has been satisfactorily established ?—That is my opinion; that where experiments have been performed already, or a point of physiology has been ascertained, the experiments should not be permitted. I do not think that an experiment should be repeated over and over again in our medical schools for illustrating what is already established.

176. But I also understood you to say that the person who does make these experiments should be a practised anatomist, and should not be allowed to do it till he has a considerable degree of scientific skill ? —I think he ought to be a man who has considerable scientific skill and knowledge of those branches of science, anatomy and physiology in particular.

177. Would not that rather point to the person, who was licensed as you suggest, applying on behalf of himself personally rather than on behalf of any particular building in which these experiments should be sanctioned ?—I do not mean to say that it might not be right and proper that an individual of that competent nature should be allowed to apply for leave to perform experiments irrespective of their being performed at a particular place.

178. Then you think that leave might be given to allow experiments of this description to be performed at a particular place subject to the penalty of the withdrawal of the license on improper experiments being performed, or on their being improperly performed there ?—Yes, it is a difficult question; but I can conceive a case to arise where a surgeon or a physician either might wish to do something in which he would like to have his knowledge fortified by an experiment on an animal; and if he were to wait for application, say to the Secretary of State, and the Secretary of State has to refer to his inspector and to get his answer, the patient might die meantime. Such a case might arise; I do not mean to say that it is likely to arise frequently, but if an experiment were deferred till all this correspondence had been gone

through, the circumstances would have passed by which necessitated the experiments, and the person might not have got the benefit which he otherwise would.

179. Whereas in such a case as that a comparatively painless experiment upon an animal might enable the practitioner to determine what course he should adopt with the human patient?—Yes; it might occur with surgery in particular, I think.

180. (*Lord Winmarleigh.*) Have you any knowledge of the extent to which this practice is being carried on in England at the present time?—I have not any accurate knowledge on the subject. I can tell your Lordship that I was for 30 years the physician and the principal medical teacher of one of the largest medical schools of this metropolis, that attached to St. Bartholomew's Hospital, and the number of experiments made in those 30 years were very few indeed.

181. You have no knowledge of what is going on in the different large towns of England at the present time?—No, nothing except what I know from occasional reading of the newspapers and scientific journals.

182. Could you state to the Commission briefly what are the chief benefits that have been derived to the human race from the experiments which have been made within your own time by these means?—I do not think that I could give you a sufficient account of them by merely answering the question in that way.

183. For instance, you mentioned just now the benefits derived as regards the affections of the heart?—Yes, those are some of the principal ones that have come within my range as a physician.

184. You could not mention any others of equal importance?—Yes; the experiments that were performed by Doctor Marshall Hall some years ago upon the nervous system, by which he established very important principles as to the functions of different parts of the spinal chord independent of the brain itself, showing that there are certain functions residing in the spinal chord which were not known to exist before, commonly called the reflex functions of the spinal chord.

185. And are the spinal chords of animals affected by the same things as those of human beings?—Yes, of mammalia; I am speaking of the reflex action of the spinal chord as pointed out by the experiments of Doctor Marshall Hall and others.

186. The experiment made with reference to the nerves I believe cannot be made without causing pain to the animal?—Those experiments would not occasion very great pain, because in some of those experiments there would be the division of the spinal chord itself, by which all parts of the body below the division would be without sensation of any kind, and the experiment would show you that although all communication between the brain and the distant parts of the body was severed, yet there were peculiar functions performed in these distant parts which you would have fancied beforehand were dependent on the brain; showing that they are altogether independent of the brain, and are dependent on a principle resident in the spinal chord itself, quite independent of the brain. For example, a man may be lying in his bed and apparently motionless, supposed to be paralysed, say a soldier or sailor; and the nurse in charge would have told the surgeon in days gone by, and might do so now, "That fellow is deceiving us; I have seen him move his legs in the night." "Is that the case?" "Yes, I can swear to it, and you can go to him yourself and ask him to move his legs." He says that he cannot do so. "You must try, sir;" and you uncover the bedclothes and just touch the fellow's foot with a feather, and he will draw his legs up, and not know that he is doing it. That is from an independent function in the spinal chord. He himself would be quite unconscious of the movement and have no power or control over it at all. Those experiments of Dr. Marshall Hall and others point out to us an explanation of forms of disease which we did not understand before.

187. And of which the medical profession have taken advantage for the benefit of the human race?—Very much so indeed. No doubt we should have believed before those experiments that there must be some disease in the brain in that case, whereas we know now in many of these cases that the disease is not in the brain at all, but in the spinal chord.

188. Have any great benefits been obtained with regard to the liver through discoveries made by means of experiments?—There have been very important discoveries indeed. There are the experiments of the French physiologist, Claude Bernard, by which he established that the functions of the liver were not, as commonly supposed, simply for the secretion of bile, but that another very important function was performed by the liver, the elimination or elaboration of a principle which becomes the saccharine principle of the blood.

189. That was discovered by means of animals ,and could not have been discovered by any experiments on the human frame; is that so?—It never entered into the thoughts of men before that the liver had such a function as that; and I do not know how they would have arrived at such a conclusion without such experiments.

190. Do you say that without destroying animal life you could not have got that knowledge?—I will not go so far as that, but I cannot conceive how you would have got it in any other way.

191. Do you believe that in the long run the chief experiments that have been made upon these animals have been such as could not have been performed upon the human frame without danger to life; I mean those experiments now so much complained of as having been made on animals; do you believe, speaking of them generally, that they are experiments which could not have been made safely on the human race?—They could not; they would involve the death of the human individual. These experiments of Claude Bernard on the liver involved the death of the animal.

192. Do you think that there are any of these experiments which cannot be made without inflicting pain on the animal?—Under our present knowledge of anæsthetics they can generally be performed without creating pain.

193. You think, without exception?—I think so, excepting those upon the nervous system, and a large portion of those, for example, when you want to get down to the spinal chord of an animal to divide it, the animal is partially under an anæsthetic whilst it is being done, and you see the result of the experiment afterwards. The most painful part of the experiment would be cutting down to the part, and all that could be avoided by putting that animal in a state of anæsthesia while that part of the experiment is made.

194. But the test of some of the experiments is the pain they cause, is it not?—Yes, some on the nervous system, not all.

195. It would not be a successful experiment in some cases, unless it showed what pain was caused?—That experiment of the division of the spinal chord itself puts an end to all pain in the part; then, as to the subsequent experiments on the parts which are below the section of the chord, they are absolutely without feeling.

196. Do you believe that there are any means of conducting the experiments on the nervous system without causing pain to the animal; I mean the general nervous system?—That is a very difficult question to answer. There are some parts of the nervous system which might be operated on without causing pain.

197. But you could not apply that without an exception?—No; but the fact is I am not what may be called a scientific physiologist, and therefore it must be understood that I am not well informed on all these points.

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198. (*Mr. Forster.*) You, I think, would limit these experiments, as I gather from your answer, to cases in which the experimenter was a man of real scientific knowledge, and likely to be able to arrive at some knowledge which would be of use to the profession?—I think ignorant persons, persons who have not obtained a high knowledge of anatomy and physiology, certainly ought not to be allowed to perform these experiments.

199. Would you or would you not think it desirable to illustrate medical teaching in classes by experiments on living animals?—I think it would be very improper to do so.

200. And in the same way you would think it improper that young men teaching themselves, as it were, should go into a course of these experiments?—I think, unless their knowledge has been already ascertained, and they are clearly men competent to do it, they ought not to be allowed to do it. But I can conceive a young man, not a teacher of physiology, having a great love and taste for the science, and having a thorough acquaintance with all that has been done hitherto, believing that he could advance knowledge by experiments; and I can fancy his performing experiments which might lead to important results.

201. Would you encourage young students to do that?—No, certainly not; I should deter them.

202. You would draw a line between this kind of experiment and other kinds of experiments, because you would say, generally speaking, I suppose, to a young man who was studying any branch of science, physics or chemistry, that the more experiments he tried the better?—Yes; certainly.

203. But in this case you would think it very desirable that he should be limited?—I would.

204. Do you think that the medical students in the kingdom are much in the habit of trying these experiments?—No, I do not think so.

205. The impression exists on the minds of some persons, who take great interest in the matter, that they are in the habit of doing so, and there are two or three facts that induce them to think that. One is the fact that this book, a book entitled "Handbook for the Physiological Laboratory," edited by J. Burdon Sanderson, seems to encourage these experiments just as much as any other experiments. Are you aware of the book?—I know of its existence, but I cannot say that I have read it.

206. Is it a book which is considered a sort of handbook to the profession?—It is very new; I think the very first edition of it cannot be above a year old. It certainly is a book which has come out within the last 12 months.

207. I find at the beginning of the preface these words, "This book is intended for beginners in "physiological work. It is a book of methods, not "a compendium of the science of physiology, and "consequently claims a place rather in the laboratory "than in the study. But although designed for "workers, the authors believe that it will be found "not the less useful to those who desire to inform "themselves by reading as to the extent to which the "science is based on experiment, and as to the nature "of the experiments which chiefly deserve to be "regarded as fundamental." I think one would gather from that that the experiments here mentioned are rather mentioned with a view to their being made?—I do not know whether that is a correct inference or not; but I should certainly think it means to imply that these experiments were carried on more extensively than I am aware of; but I should think it did not mean that it was for students, but rather for those who were teaching physiology.

208. I have not read the book, but here is an experiment upon rabbits (*handing the book to the witness*); it is headed "Excitation and section of the spinal chord "in the rabbit;" is that what you have been describing in your answer to Lord Winmarleigh?—No, not at all.

209. Now is not that an experiment that without anæsthetics would be both painful and lingering?—I should think both painful and lingering.

210. Would it be a successful experiment with the use of anæsthetics?—I do not feel myself competent to say; it is a long detailed experiment.

211. You would not in your position in the profession recommend to young men to practise these experiments on living animals by themselves, or recommend these experiments to be part of medical teaching?—I should recommend them not, and should take steps to prevent their doing it.

212. I observe in this book that has been lately published that an experiment is represented as being tried under the influence of curare; is that really an anæsthetic or not?—It is, I suppose, the same as worari, is it not? It is only a different way of spelling it. I have seen animals under the influence of it, and they seem to be insensible; I cannot say more.

213. Supposing that it be not pretty well proved that curare is an anæsthetic, you would, I imagine, be strongly opposed to an animal being put under its influence?—I should certainly.

214. (*Mr. Erichsen.*) You were speaking of the advance that had been made in medicine in respect of diseases of the heart and the nervous system during your practical experience; could that advance have been arrived at by any mere clinical or pathological observation, or in any way except through the means of experimentation upon living animals?—No advance had ever been made up to this time; and I do not, in looking back now, see how it could have been done. I would not absolutely say that if they had taken a very large number of instances and very carefully tabulated them, they might not at the end of a number of years been able to arrive at approximate results; but I think it doubtful whether any one person could have done so.

215. Up to the time that Sir Charles Bell made his experiments on the nerves of the face it was the common practice of surgeons to divide the facial nerve for the cure of neuralgia, tic doloreux; whereas it exercises, and was proved by Sir Charles Bell to exercise, no influence over sensation, and its division consequently for the relief of pain was a useless operation?—Yes.

216. We have spoken hitherto chiefly of painful experiments; but are there not a great many experiments that have also been practised that do not occasion any very material suffering? For instance, with regard to the induction of disease, do you think that such experiments as endeavouring to ascertain how tubercle is developed in animals, by placing them in certain unfavourable hygienic conditions, would be a proper experiment to perform in order to elucidate the causes of tubercular disease?—I think quite justifiable.

217. And it would not be painful?—If the disease were induced, of course there would be all the inconvenience of that to the animal.

218. But domestic animals die very largely of that disease?—Yes.

219. Then there are also a number of experiments that have been performed for medico-legal purposes, with reference to testing the effect of drug remedies and poisons. Do you think that such experimentation is proper in order to test the quality of a new remedy before it is applied to man. Supposing a new alkaloid, for instance, is discovered, if it is desirable to test the effect of that alkaloid, would it be right to test it at once upon a man or upon one of the lower animals?—I think such an experiment on an animal would be quite justifiable.

220. Those experiments have been of use?—Very great use indeed.

221. (*Mr. Hutton.*) Have you not rather underestimated Sir Charles Bell's own view of the painful nature of his experiments, because I notice in a letter of his to his brother in 1822 he says, "I shall be "writing a third paper on the nerves, but I cannot "proceed without making some experiments which "are so unpleasant to make that I defer them. You

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" may think me silly, but I cannot perfectly convince " myself that I am authorised in nature or religion to " do these cruelties; . . . . and yet what are my " experiments in comparison with those that are " daily done, and are done daily for nothing?" That is a passage in regard to those particular experiments that you spoke of as almost painless, I think?—Well, there were some other experiments of Sir Charles Bell's that were much more painful than those I have alluded to; but with respect to that passage, all I would say is that I think it redounds very much to the honour of the man, exceedingly so; and although he was prepared to make physiological experiments for the advancement of science, his good feeling made him shrink from that which he thought it afterwards his duty to do.

222. Do not you think that people are beginning to underrate much more than they used to do the painfulness of these experiments, while the experiments themselves are multiplying in number? That passage which I have read is dated in 1822, and he speaks there of the experiments as large in number, whereas they are much more common now than at that time.—I think he refers there to experiments going on at that time in Paris by Majendie, who was rather regardless, perhaps, of the sufferings of his victims; I think Sir Charles Bell adverted to that probably.

223. Do not you think in our English schools this particular method has grown of late years; that this method is considered a more powerful scientific method than it was thought to be in Sir Charles Bell's time?—Yes.

224. And therefore its use has been multiplied?—It has been resorted to more frequently.

225. In fact the practice now is very much more extended in medical schools than it was 40 or 30 years ago, and in physiological laboratories?—I think it is.

226. You were speaking of your own experience in St. Bartholomew's, that during your 30 years there there were very few experiments of that kind, if any?—Very few. I will not say none, because it was while I was there that I tried the experiments which I adverted to, and I remember the effects of the experiments which Mr. Erichsen alluded to.

227. Are you aware of the experiments of Doctor Legg, for instance, on the biliary fistula of cats?—I have been 10 years away from Bartholomew's, and therefore I think you had better ascertain that from some other source.

228. When you say that by far the majority of these experiments might be conducted by anæsthetics, is it not true that in the lower animals they are so powerful that you cannot give them,—to frogs for instance,—unless it be this doubtful anæsthetic, this curare?—I am not able to give an opinion.

229. A large number of experiments are made upon frogs, and if they cannot be conducted with anæsthetics, then the majority of experiments cannot be, I suppose?—I am not conversant with physiological questions sufficiently to answer that.

230. You would say that the experimental method is growing, as compared with other methods, in the medical schools?—In physiological laboratories, not in the medical schools.

231. (*Mr. Forster.*) When you speak of medical schools do you mean such as that at St. Bartholomew's?—Yes.

232. (*Mr. Hutton.*) I thought that a medical school was thought of little value unless a physiological laboratory was annexed to it?—No doubt physiology is now practically taught as it was not before taught.

233. Would you say that the legitimate object of these new experiments is new knowledge or new remedies; would you put the pure scientific object as the proper object of these experiments, or would you say that you must have in view some clear alleviation of human or animal pain as the object of the experiments?—It is knowledge in the first instance that is the object of the experiments; and that knowledge having been obtained, it leads most materially to the alleviation of human suffering.

234. You would make it an experimental science in the same sense in which chemistry or mineralogy is an experimental science, and would say that experiments, wherever they seemed to be leading to fresh knowledge, might be pursued simply on the ground that the experimenter sees a glimpse of some fresh light and knowledge which he can get by experiments; would you go as far as that?—No; I think it is going almost too far to say that I do not think experiments are to be performed merely where persons see a glimmering of knowledge to be obtained.

235. But a good many that I have seen described seem to have been of that character, that they opened more questions than they solved, so that I wanted to know whether you took the scientific view of these things, or the view that to sanction such experiments you must have a real remedial agency in prospect?—I think myself that the advance of knowledge would be a sufficient object, if there is good reason to believe that knowledge would be greatly advanced by the experiment, provided it is done with proper precautions.

236. It is hardly ever possible to know whether knowledge will be advanced; in all experimental sciences it is the mistakes that bring as much knowledge as the things that are done with a real clear anticipation of what is going to happen. Those which give a result that was not expected are quite as instructive as those which give a result that was expected?—It may often happen that there are discordant opinions on a particular point of physiology or pathology, and that discordance very likely might be removed by a well-arranged and well-performed experiment; the two parties would then get much sooner to the truth, and that discordance would disappear then.

237. But you would limit it by saying that there must be some immediate prospect of a beneficial result in view; that it is not justifiable to look upon painful experiments as the agency of mere experimental science?—With respect to physiology alone or to medicine at large do you mean?

238. I want to know with regard to either?—I think myself that where there is a sufficient reason to hope and to expect that knowledge would be greatly advanced, without seeing what ulterior benefits are to result from that knowledge, the experiment should be performed.

239. You put it on the same ground as the other experimental sciences with the limitation that you would never give pain where you could help it?—Not exactly on the same ground, because it is immaterial when a man takes chemical ingredients whether he mixes them in a certain way or not; but that is not the case with living animals; in their case the man must have a good object and a reasonable prospect of advancing knowledge; whereas a chemist might try all sorts of combinations without any definite object at all.

240. You mean that for pure curiosity it would not be allowable to make these experiments?—Just so.

241. But that whenever you had a real scientific result in view it would be allowable?—I think so.

242. Whether it led to a remedial agency or not?—If the man had a scientific result in prospect I think it would be justifiable, provided it were performed under proper conditions.

243. Here is an experiment which a medical man describes, and which he saw. He says, "Several " frogs were put under the influence of 'woorara' or " 'droorara' poison, which, while it deprives the poor " creatures of the least power of movement, leaves " sensibility to pain unimpaired; some think the latter " is even increased. They were then slit open in " such a way as not to kill them, and a delicate trans- " parent tissue containing bloodvessels partially " dragged out from the inside of their bodies, and " placed under a microscope. They were left thus for " two hours." That was to study the tissues; and a great many of the experiments in this handbook are to study the changes in the tissues which take place, for which it is necessary that the animal should still be alive. That is a purely scientific purpose, but it

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does not immediately lead to any remedial agency. Would you justify that kind of experiment?—I do not know. Unless I had time to consider the matter, and saw the experiments and knew all the conditions, I should not like to express an opinion on it.

244. (*Chairman.*) I understand you to say that Harvey's discovery has been followed by great remedial consequences?—No doubt.

245. But those remedial consequences were not foreseen by Harvey at the time when he made the experiments which resulted in his great discovery?—No, they could not be foreseen. At the time when Harvey made his experiments there was great confusion in respect to the course of the blood, and his experiments were made and settled that question.

246. His experiments were made for the purpose of making the discovery, which he did make, of the circulation of the blood?—Yes.

247. It was at the moment a mere scientific discovery?—A mere scientific discovery.

248. But it has been followed since by great remedial consequences, which were not foreseen by Harvey at the time that he made the experiment?—Exactly so.

249. The opinion which you have been so good as to give us I have understood to be to this effect, that experiments in general can be made under proper anæsthetics while the animal is entirely unconscious and incapable of suffering?—As a general rule that is so.

250. And you have reprehended very much any person who makes an experiment of that sort otherwise than under complete anæsthesia?—I reprehend its being done in any other way, if it can be done under complete anæsthesia.

251. Then you have said that you think experiments which cannot be made wholly painless can yet be made principally painless by the proper application of anæsthetics?—Yes, I have said so, and I hold to that opinion.

252. Then you have said that there may be a limited and a very limited number of experiments left, which will cause a degree of pain to the animal, which it is painful to the operator and to everybody else to contemplate?—No doubt that is the unfortunate result.

253. And you have said, with reference to those experiments, that you think there should be in every case a special sanction; and with regard to that special sanction you have rather pointed to the experience in regard to anatomy as a guide to the remedies which you think it might be suitable for us to recommend to the Crown and Parliament?—Yes, I do think so.

254. That is a correct description of your general view upon the subject?—Yes, it is. With respect to the last part of the summary of my views which you gave, I should say, that whoever is the authority should be guided by somebody who has full and sufficient knowledge of anatomy and physiology, so as to give really sound advice, and who is responsible for that advice.

255. (*Lord Winmarleigh.*) Should he not also have a general knowledge of the character of the person? Supposing, for instance, that a Secretary of State (if he was the person to give the authority) should be desired by some person fit to give him that advice, should not that man also have a knowledge of the general profession?—Yes; I think he ought to have a knowledge of the individual, what his character is, and his position, and what his attainments are.

256. It would not be necessary that he should have a knowledge of physiology?—No, but that he should know his character and standing and position.

257. (*Chairman.*) Did not Majendie come once to this country?—Yes, I believe he did.

258. And was not he almost driven from it by the feeling which his experiments created?—I cannot be sure. I think that you will get that information much better from other sources than from me.

259. (*Sir John Karslake.*) Do you think that the administration of new drugs as a poison to animals is useful with a view to ascertain their effect on the human subject?—It has been useful on different occasions, no doubt.

260. Sir Thomas Watson seemed rather to think that it was not, as I understood him; but in your experience it has been useful in certain cases?—I think it has, certainly.

261. So that by bringing science to bear you think it would be justifiable to use poisons on animals to discover the symptoms produced when they are taken by human beings?—The information obtained would be imperfect, but it would be useful as enabling us to avoid doing injury. If a new alkaloid was discovered, and it was reported to have certain properties, it would be desirable to have it administered to an animal to see its effects before it was administered to a man.

The witness withdrew.

Sir James Paget, Bart., called in and examined.

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262. (*Chairman.*) Besides your position in the Royal College of Surgeons, you are, I think, President of the Medical and Chirurgical Society?—Yes.

263. And your attention has been very much directed to the subject which is now being inquired into by this Commission?—Yes.

264. You have prepared, I think, or have taken part with Mr. Huxley and Mr. Darwin and others in preparing, a petition which was intended to be presented to Parliament on the proposal of any Bill upon this subject?—Yes.

265. And that petition adopted certain resolutions?—Yes.

266. Which resolutions were passed at a meeting of the British Association at Edinburgh?—Yes; in 1871.

267. Those resolutions were these:—"(I.) No experiment which can be performed under the influence of an anæsthetic ought to be done without it. (II.) No painful experiment is justifiable for the mere purpose of illustrating a law or fact already demonstrated; in other words, experimentation without the employment of anæsthetics is not a fitting exhibition for teaching purposes. (III.) Whenever, for the investigation of new truth, it is necessary to make a painful experiment, every effort should be made to ensure success, in order that the sufferings inflicted may not be wasted. For this reason, no painful experiment ought to be performed by an unskilled person, with insufficient instruments and assistants, or in places not suitable to the purpose; that is to say, anywhere except in physiological and pathological laboratories under proper regulations. (IV.) In the scientific preparation for veterinary practice, operations ought not to be performed upon living animals for the mere purpose of obtaining greater operative dexterity."—Yes.

268. Those resolutions, having been agreed to by the British Association, have received your entire approval?—Yes.

269. And that of eminent scientific persons like Mr. Darwin and Mr. Huxley?—Yes; the petition was signed by many more: Professor Owen, Sir William Gull, Sir William Jenner, the President of the College of Physicians, the President of the College of Surgeons, and several more leaders in science.

270. So that in any reasonable plan for giving effect to those resolutions, the Crown and Parliament might rely upon the cordial support of the leading scientific men in this country?—Yes.

271. Now, is it your opinion that the question

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whether there shall be an experiment on a living animal is an optional question altogether?—No.

272. What is your view upon that subject?—I think it may be said generally that medical science, being in a state of progress, is continually coming in sight of things which can only be decided by experiments, either upon man or upon some lower animal.

273. Supposing that a patient is brought to you, having received some injury, for instance, which requires your care, you either do know what to do with him or you do not. If you do, you proceed upon knowledge already obtained; but if you do not, there is the necessity of an experiment in his case, is there not?—Yes.

274. And the experiment in his case will be excluded if you have previously obtained complete knowledge of a case of that kind?—Yes.

275. And it is for the purpose of such knowledge that you consider some experiments to be necessary?—Yes.

276. Since you have been in practice anæsthetics have been discovered?—Yes.

277. Do they materially affect the question which we are now considering?—Yes, in so far as they make a large number of experiments possible without the infliction of any pain at all; and those may be experiments of the most important kind.

278. Are a large proportion of the experiments that you would recommend capable of being performed under chloroform? — A very large portion; but there is a class of experiments which are becoming more frequent, and which I think in many respects more necessary, namely, testing the effects of medicines and of poisons, and the production of diseases in animals, for the purpose of studying them more accurately than is possible in men; and in those of course anæsthetics cannot be used during the progress of the disease.

279. Having got the resolutions, which you have approved, before us, may I assume that the leading men in physiology, and the leading men in the two branches of the medical profession, are quite as desirous to promote humanity as eminent men in the other branches of science?—Yes.

280. And that they would be glad that any reproach which any imputation of abuse would cast upon them, should be removed by proper and reasonable measures?—Yes.

281. And that they would be inclined to come forward to support the Crown and Parliament in such measures?—I think that there might be a doubt in their minds whether the changes required would be better accomplished by public opinion or by legislation; in any case, they would be very glad to have any abuse of experiments upon animals, that can be proved, reduced or abolished.

282. Now do you think (indeed you have already said so), that the repetition of experiments is not justifiable?—Certainly it is not.

283. That is to say, that when a scientific fact has been fully established, to repeat a painful experiment for the purpose of illustrating that fact to a new class of students is not justifiable?—Certainly not.

284. And ought not to be permitted?—Certainly it ought not.

285. Ought students under any circumstances to perform these painful operations themselves?—No; and I should be glad to state that there are regulations of the College of Surgeons, and, I think, of other examining bodies, to the effect that the students are not expected to do them. It is appended to the regulations requiring students to attend courses of practical physiology; and it states that the students themselves are not expected to perform experiments on living animals.

286. (*Lord Winmarleigh.*) Are they forbidden to perform them?—I do not think that they could be forbidden by any regulation issuing from a central educational authority; but they are in no sort required to do them, and, so far as I know, they never do them.

287. (*Chairman.*) Would you have any objection to give the Commission those regulations?—Certainly not.*

288. Will you kindly send them to us?—Yes. I do not remember the precise terms of the regulation, but it is to the effect that students are required to attend a course in practical physiology; but then it is added as a note, that students are not required to perform experiments, or painful experiments, on living animals. But I will send you the very words.

289. The object of the regulation being to discourage the practice?—To discourage it in every way.

290. Do you think that it is justifiable to perform painful experiments before a class?—No.

291. Do you think that the number of experiments, even without pain, that is to say, under anæsthetics, should be limited as much as possible?—Yes, they should be limited, I should say, to the illustration of those parts of knowledge which you cannot reasonably expect to communicate by words or drawings or by any other means.

292. Now what sort of experiments do you consider to be really necessary?—As an illustration, I think that every student should see a heart acting; and that can be shown without the infliction of any other pain than the pain, for instance, of decapitation in a large amphibious animal, such as a turtle; or it can be shown, under the influence of anæsthetics and with the help of artificial respiration, upon any warm-blooded animal, so as to produce no pain at all; and at the end of it the animal will be killed and have literally suffered no other distress than that of taking the anæsthetic. I think it is scarcely possible that a student can understand the way in which a heart acts unless he has seen one act.

293. Are there any experiments which do necessarily inflict pain, which you consider to be necessary experiments? Speaking now not of an exhibition to a class, but of experiments for the promotion of science, are there any which do necessarily inflict pain and which you think cannot be dispensed with?—If they are for the promotion of science, and not for the ascertainment of facts already ascertained, then I should answer that there are some.

294. Would you be so good as to illustrate that?—I can of course only illustrate it by cases in which the experiments have been already performed, and with a good result; and I would take, for example, the case of the ligature of arteries, for the cure of aneurisms or for the prevention of bleeding. Now although one might, by observations upon men alone in cases of accident, possibly, without reference to animals, have thought of tying an artery for the cure of aneurism, yet the manner in which it should be done, and all the precautions which have to be taken during the operation, and which have to be observed afterwards, could not have been ascertained by observations upon men alone, but were facilitated and made complete by operations upon animals, in which as far as possible the ligature of the artery was carried out in the same manner as it might be in men.

295. Then in such a case the suffering caused by the operation like that in the human being would be taken away by the use of anæsthetics?—It would be now if further observations are required. I am speaking of experiments upon animals that were made before the discovery of anæsthetics, by Hunter and others after his time; but the whole process of the recovery of an artery after ligature, and the means essential to its recovery, and the exact knowledge of all the process by which the artery is closed, could not have been ascertained without experiments upon animals; because it is as essential to know the whole process of the recovery as it is to know the particular manner in which the operation is to be performed. You might refer to a number of operations that were done for the ligature of arteries (some of which were done by Hunter himself, and some by those who followed him,) which failed, and the patients lives were lost, simply because at that time surgeons had not

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* Appendix III., § 2.

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ascertained the whole nature of the process for the repair of the injury done by the operation.

296. If such an experiment were performed now the operation would be performed under chloroform, and the pain of the recovery would be all to which the animal would be subjected?—Yes, which pain must be comparatively trivial, I imagine. It is so in man; it is usually an unimportant pain which he suffers in recovering.

297. Then I may conclude that very painful experiments are, in your opinion, very rarely necessary?—Very rarely.

298. That all the experiments which can be performed without pain it is the bounden duty of the surgeon to perform without pain?—Yes.

299. And that wherever there is some pain that must be suffered, the most merciful measures possible ought to be taken?—Yes.

300. With great care and great forethought?—Yes.

301. And that it should be limited to persons of great competence, and not thrown loose for anybody who chooses to perform?—Yes.

302. Are you of opinion that of late years more reliance has been placed than formerly upon experiments?—It is very difficult to say, because all medical inquiry has become of late years very much more active than it was 30 or 50 years ago; and it may be that the present course of inquiry leads to subjects which can scarcely be investigated except by experiments; but on the whole I am disposed to think that experiments have, sometimes, been entered on before all other methods of inquiry have been exhausted; that they have been, so to speak, rather premature than really necessary; for I should hold that an experiment on animals should for the most part not be made till one is quite satisfied that all other means of inquiry have been fairly exhausted, and exhausted in vain.

303. Now as you know our inquiry is not limited to the common term vivisection, but it embraces all experiments upon living animals?—Yes.

304. Some experiments upon living animals illustrate the operation of poisons, do not they?—Yes.

305. Will you have the kindness to explain your view on that subject?—It seems to me that the case of snake-poisons is a very fair one by which to illustrate the absolute necessity, if there be a necessity in anything, for the performance of experiments upon animals. I believe that there are not less than 20,000 deaths from snake bites annually in India. Of course I have no personal knowledge of that, but it is often so stated. Now it seems almost impossible that an antidote for an Indian snake bite should be found except by having animals bitten with snakes and immediately experimented on to see what is likely to cure them. It is perfectly hopeless to make these observations upon the persons bitten by the snakes, because, in the case of nearly the whole of them, they are not found till either they are dead or dying and past all hope of cure: so that the only objects upon which the experiments can be employed are the animals who may at the time be bitten and be immediately observed. It may be objected that the progress of science will ascertain by other means some antidote to snake bites. I should think it would be a very low estimate to suppose that the progress of science would do it in 25 years. In the meantime half a million of persons would be killed by snake bites, and those half a million might possibly, I do not say even probably, but they might possibly, be rescued by a sufficient course of experiments performed on living animals immediately observed, and having a variety of treatments employed upon them directly they are under the influence of the snake poison. And I may add that while these experiments, as I suppose, ought to be made with substances likely to produce the desired effect, the few persons found under the influence of snake bites are treated by means altogether inadequate, and of which the inadequacy would be discovered by proper experiments. I have asked about some experiments which have been performed lately in reference to snake bites. There has been a Government-reward offered for the discovery of a sufficient antidote for snake bites in India; and some few months ago a person applied for this reward, and was referred to Saint Bartholomew's Hospital, to see whether he had really found an antidote. He was so convinced, I am informed, of the efficacy of his antidote that he professed himself ready to be poisoned with some of the cobra poison. They persuaded him to wait, and they had some pigeons innoculated with the cobra poison, which all quickly died; and as the chance was, those that had used the antidote given to them died a little sooner than those which had not, and the inventor of the antidote was persuaded not to subject himself to his own treatment. I have thought carefully over the whole matter, and it does not seem to me probable that, under any circumstances, we can suppose that short of 50 years hence a true antidote for Indian snake bites should be found by any other means than by a series of experiments which must necessarily involve the painful dying of possibly a large number of creatures, pigeons, rabbits, and others.

306. With regard to medico-legal investigations, have you anything to say on that subject?—No, I have no familiarity with them.

307. Does it at all come within your knowledge to what extent those experiments are practised in distant places, in country towns and so forth?—I have no knowledge of that, but my belief is that experiments on living animals are not performed at all, except in some schools of medicine, and by some physiologists who are not connected with schools of medicine.

308. Nor can you guide us perhaps as to whether there is really a great extent of abuse in this country on the subject?—I think there is not. I think the worst I could say of it, as I said just now, is that some observers seem to put more reliance on experiments on living animals for therapeutic purposes than I should. But they can plead that very remarkable results have come from their observations, of which results I will give you one or two examples, if you will allow me. The treatment, for example, of *angina pectoris* I believe to be now very much more nearly successful than it ever was before, and to have been discovered mainly by experiments on living animals. The use of the substance called nitrite of amyl, which has the effect of checking the agony, and probably also sometimes the mortal effects of *angina*, was a result mainly of a series of experiments on living animals. I could give you the history of the experiments if you wish, but perhaps it is enough for me to make the statement.

309. (*Lord Winmarleigh.*) Was that such an experiment as could be performed without creating pain, or was it one which necessarily created a great deal of pain to the animal?—No, I apprehend not great pain; for nearly the whole of it, if not the whole, might be done under the influence of anæsthetics.

310. Not the whole of it?—I should think the whole might be done under their influence, but I would not venture to say that with certainty.

311. (*Sir John Karslake.*) What was the nature of the experiment? Was it an external application?—Dr. Brunton found that in the paroxysms of *angina*, in a person he had to examine, the blood vessels became very tightly filled; and it occurred to him that the reason of the intense agony of *angina* might be from this excessive tension of the blood vessels. One method of relieving the tension was to remove a quantity of blood, which, however, might be injurious. Another was to find some means by which the blood vessels themselves might have their tension diminished; and among the substances which he tried on animals was this nitrite of amyl. He found that it would relieve the tension of the blood vessels, and by relieving them would put an end to or mitigate the paroxysms of agony which occur in angine.

312. (*Lord Winmarleigh.*) Will you state the process?—It is all published, and if you would allow me

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I will send the Commission an abstract of the paper. It would require the exposure of blood vessels in the living animals, and the testing, with apparatus adjusted for the purpose, of the amount of pressure exercised by the blood on the interior of the vessel.

313. (*Chairman.*) All of which might be done under the influence of chloroform ?—Yes.

314. (*Lord Winmarleigh.*) And it could not have been done by any experiments on the human body ?—Certainly not.

315. (*Mr. Hutton.*) But does not chloroform very much alter the action of the heart, so as to render experiments of that kind uncertain ?—Not so much as to affect an experiment in the hands of a careful observer, who would first test the pressure of the blood while the animal was under chloroform, and then see whether it was diminished by the administration of nitrite of amyl or anything else.

316. (*Lord Winmarleigh.*) In all the different hospitals and colleges is there a school of physiology attached ?—There is not a separate school of physiology. Physiology is taught as one of the branches of study; and now there is adjacent to the hospital what is called a physiological laboratory.

317. I observe in the contract for buildings at Edinburgh that the contractor states to the promoters of the hospital that he has arranged a chamber for a school of physiology expressly for that purpose, in which he has made arrangements for animals, and for the experiments which are made upon animals; is that a common thing ?—Yes; but I should be sorry for it to be supposed that a physiological laboratory is for nothing but experiments on living animals. It includes a large quantity of work with which living animals have nothing to do; for example, all the microscopic teaching.

318. I am aware of that; but this contractor states that he has made arrangements for living animals; that he has made a provision for dogs and other animals. Now is that an exceptional case ?—That, I should think, is very much beyond what would be found in any school in London; certainly I can speak of Saint Bartholomew's, and by repute of some of the other larger hospitals, and no arrangement is made in them for living dogs.

319. How is the study carried on, for instance, in one of our principal London hospitals ?—It is carried on mainly through teaching by the use of the microscope upon dead textures, and some few living parts, such as the web of frogs, by showing such processes as those of artificial digestion; by testing blood and other fluids chemically; and sometimes by exhibiting experiments on animals kept insensible.

320. But now supposing that a school of physiology for experiments on animals is established at a hospital, who would be the parties in one of our great colleges who would be allowed to make all these experiments at the present time ?—At present it would be left entirely to the discretion of the lecturer on physiology. Therein I think may be an error. I think that in any case where courses of experiments on living animals are to be performed for the purpose of teaching, whether in the school of physiology or materia medica, or any other, no such experiments should be done except with the consent of a committee of medical officers and lecturers, or even, if it were thought proper, some of the governors of the hospital.

321. And do you think that it would be possible to limit these experiments by regulations in the hospital such as you have described ?—Yes.

322. And to forbid them in all other parts ?—Yes.

323. You would not, for instance, allow a body of surgeons in a town like Birmingham to conduct operations of that kind without some control such as you have described ?—There is a medical school at Birmingham.

324. Then I will take some town where there is not a medical school, that is what I wanted to get at. You would not allow these experiments to be made by any body of the medical profession, unless under some control such as you have described as what should be exercised in these principal hospitals ?—I think it might be safely left to members of my profession. I think it is unreasonable to suppose that any three or four persons of my profession would agree to an unnecessary or unreasonable course of experiments. One person in his zeal might do an unwise thing, but four certainly would not.

325. Can you state as to all the hospitals that no person of less authority than the person you have described would be allowed to make experiments ?—I cannot speak from knowledge of it, but I should have the strongest conviction that if it is done it is done secretly.

326. At any rate you think it ought not to be done ?—It certainly ought not to be done, except by persons in authority; and I think it would be satisfactory that for experiments to be done on living animals there should be a committee of the medical officers and lecturers of the hospitals or schools, before whom an outline of the course of experiments should be laid for their decision as to whether they were reasonable or not.

327. Do you think it would be unreasonable to say that no experiment should be made on a living animal, unless it were stated to some authority beforehand that some experiments were about to be made ?—I think it would be an unreasonable rule in the case of persons thoroughly conversant with the manner of making experiments, for they would be better judges of the propriety of them than those to whom the question would be submitted.

328. An opinion has been expressed here that it would be advisable to have an officer for these experiments corresponding to the inspector of anatomy, without whose authority they should not be performed; that is to say, that there should be a license given by this inspector to persons to perform these experiments ?—If you mean in the same manner as every anatomical teacher now, in order to have the bodies of the dead for dissection must have a licence, I think there is no objection to that; if there is any legislation at all I should think it must take that form.

329. And you think that no experiments should be made except by a person so licensed by some authority ?—If there is to be legislation at all. That of course it is not for me to decide: my impression is that public opinion will accomplish all that is necessary.

330. Without legislation ?—Without legislation; but if there is to be legislation, I think it must take that form, that no person shall be allowed to perform experiments on living animals, unless licensed as the teachers of anatomy are now licensed to have the bodies of the dead for dissection.

331. What are the regulations with regard to the bodies of the dead ? It is not all done under licence, is it ?—Yes; every teacher of anatomy in a medical school requiring bodies for dissection, not for ordinary examination after death, but for dissection, must be licensed for that purpose by the Secretary of State for the Home Department.

332. Do you think that law is rigidly observed ?—Yes, rigidly. It involves the whole process of the burial of the body; bodies may not be brought to the schools except under that licence, nor removed from them except with the authority of the person holding the licence.

333. (*Sir John Karslake.*) What is the case with reference to private practitioners ?—No private practitioner can have a body for dissection.

334. Cannot he under any circumstances ?—No.

335. (*Mr. Erichsen.*) And never does ?—No.

336. (*Sir John Karslake.*) Now the question has been put to you whether, in the event of legislation being suggested at all, it would be expedient to have licenses given by the Home Secretary, who would have some persons in authority to confer with, as to whether the person applying for the license was competent to hold the license or not. Should you think that advisable, having regard to the necessity that may occur for making an experiment upon an animal, with a view to a difficult operation on a man ?—Yes.

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I think in the case of teachers of anatomy it is requisite to have the signatures of two magistrates; I think something of that kind would be required.

337. But supposing a private practitioner about to perform a delicate operation on a man living in the country thought it absolutely necessary, with a view to perform the operation satisfactorily, to make an experiment on a living animal, should you think that he was justified in doing that?—I quite think that he would be justified; and that is one of the hindrances which I think legislation would put in the way of necessary knowledge.

338. Do you think that the case I have put is a case which might occur frequently in private practice?—No, not frequently, but occasionally.

339. And in those occasional cases it would be absolutely necessary to resort to that experiment on the animal with a view to carry out an experiment satisfactorily on the human being?—Yes.

340. That is the reason why you think that legislation on the subject would be inexpedient?—I think it would be a great hindrance to some useful measures; and the case you have cited is a case in point.

341. Public opinion would prevent the practice of vivisection from being carried on to an extravagant or improper extent?—Yes.

342. I suppose in the schools of surgery dead animals are very little used for the purpose of anatomy?—Very little.

343. It is almost entirely on the human being?—Yes.

344. And that therefore the cases in which it would be required to have a live animal under the knife at all, or under poisons, for the purpose of carrying out investigations as to the use and character of poisons, are comparatively very rare?—Very rare.

345. In your judgment, so far as you can have any experience in the matter, is the practice increasing to any great extent now?—I do not believe it is. In one direction it may be increasing, namely, in the inquiry into the influence of poisons and of supposed or real medicinal substances. In the other direction, that of mere physiology and of experiments for what may be called surgical purposes, such as tying of arteries, transfusion, division of tendons, fractures, and the like, I should think it is very much diminishing.

346. And that notwithstanding the absence of pain caused by anæsthetics?—Yes. I think it may be generally held that most of the facts on those subjects have been already ascertained, and that there is no advantage in repeating experiments.

347. And that the medical profession generally do not think it necessary to repeat experiments, because of what has been ascertained already?—Yes.

348. (*Mr. Hutton.*) Do you mean to say that this process of physiological investigation is not very much increasing as compared with what it was in Sir Charles Bell's time, for instance?—It has very much increased since Sir Charles Bell's time.

349. And he speaks of that as being already to a monstrous extent prevalent?—I think he must at that time have been speaking almost entirely of the French School of Physiology. Majendie was at that time the principal teacher. I should not like to mention Majendie's name with entire reprobation (as I have heard it mentioned), because he really accomplished a very large work and an excellent one, which has guided us to a great deal of knowledge, which, except for his experiments, could not have been ascertained. I quite think that his experiments were very needlessly repeated, needlessly shown to classes of students who could have profited nothing by them; but the knowledge which we have acquired by them has been very useful, even for saving human lives.

350. Do you not think that the mere fact that a laboratory has been proposed at Edinburgh with an engine at work for constantly keeping up artificial respiration in animals indicates quite a new era as it were in British medical schools?—I do not know really what the experiments for which the laboratory in Edinburgh is to be fitted up might be; and I quite think it is possible that they are developing the school of physiology unnecessarily in the direction of experiments on living animals; but it may be said in a general way, that the better the apparatus you use, although it may be large and cumbrous, the less is the pain that you inflict. Physiological experiments may be done under anæsthesia, but if the anæsthetic is not well given the animal gets out of its influence; therefore I should not blame anyone who had the design for performing experiments under anæsthesia for having all his apparatus very costly and complete.

351. Are not the schools at Leipsig, Vienna, Paris, and Florence, and others, all of them schools visited by our own medical men? They come back with these new methods in their heads, which they are anxious to apply for the purposes of teaching here?—Nothing imported from any of the schools you mention is commonly employed in the schools here.

352. Now Doctor Pye-Smith uses, even for the purposes of demonstration at Guy's Hospital, I think, living animals, and defends that practice?—I do not know whether he does; I could only speak certainly of St. Bartholomew's, where I think it is never done; unless on living animals that are completely anæsthetised, or a turtle, for instance, with his head removed.

353. But he is hardly a living animal?—Yes, except for the purpose of sensation. He has none with his head off, but many living actions go on and can be observed in him.

354. Do you know this handbook of Doctor Burdon Saunderson's?—Not well.

355. A large number of the experiments mentioned in it are not experiments under genuine anæsthetics at all?—I know enough of that book to make me believe that it must have been written for persons well instructed in physiology already, but anxious to make further inquiries in it. I should think such a book as that would never be used by students.

356. It is said to be specially for the use of students?—Not what I should mean by students; but for persons diplomatised, for instance, and still studying physiology.

357. You say that by far the greater number of these experiments can be made under anæsthetics. I find that exceeding few of these are even suggested to be performed under anæsthetics; and I have heard the opinion of physiologists that for the lower animals anæsthetics are not available; the frog is killed by chloroform?—As to the lower animals, there comes in a separate question as to the degree of their sensibility. My impression is that they are extremely little sensitive; and certainly with regard to a large number of them, although they may not use anæsthetics on them, the greater part of the experiments may be very well done after their brains are removed. A frog, for instance, will serve for nearly all the experiments you want to try on him after you have beheaded him.

358. In studying, for instance, inflamed tissues, they are obliged to keep the animal alive in order that the tissue may inflame; are they not?—But the inflamed tissue of a frog may be examined without the infliction of more pain than a casual scratch on one's hand.

359. As a matter of fact, these experiments are described as lasting for a couple of hours; and it is stated to be necessary to keep the animal alive for a couple of hours?—The pain is really nothing in the examination of the tissue of a frog's web.

360. In the case of the inflamed tissue of a frog's eye, would that be so?—That is more difficult of use; but you may examine the whole process of inflammation of the web without inflicting more pain than would come from an ordinary pustule on one's skin.

361. In a great many of these cases of the tissues of mammalia they are inflamed during life, are they not?—I have never performed experiments of that sort, and I would not venture to say how far they are necessary.

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The only experiments of the kind that I have performed on warm-blooded animals were on the transparent wings of bats; and in those you may see the process of inflammation completely with not more pain than is caused by dropping hot sealing-wax on your own finger.

362. That is guess work, is it not?—I know what a drop of hot sealing-wax on my finger is; and I should do the same with a bat's wing.

363. I find that there are experiments, lasting a long time, on the arterial system, for the purpose of noting the curve or the curves of the arterial waves. It appears that the ordinary anæsthetics are inapplicable for that purpose; that they affect the action of the heart so much that curare is the only one applicable; and Claude Barnard states that that only paralises the motor system, and leaves the suffering the same. That class of experiments you would regard as a painful class, would you not?—I should regard those as amongst the experiments that ought never to be repeated. Experiments for the purpose of repeating anything already ascertained ought never to be shown to classes, and ought never to be made, except on a reasonable expectation of discovering something new in physiological knowledge. I think those are experiments that should be strictly limited.

364. Where would you put your limit about the object of an experiment; should it be simply scientific?—I think you must put your limit in the reason of the person experimenting, and that he must be sole judge.

365. Then as all investigations suggest new ones practically the higher the scientific mind the more the number of experiments that would suggest themselves to it that might be performed with useful scientific results, I suppose?—I could not say that the number would necessarily increase; because the higher scientific mind would probably deduce from a few facts more accurate conclusions than the lower one would from many; but, knowing the general humanity of scientific men, I think they may be left to be fair judges of what amount of pain it is reasonable to inflict for the sake of attaining some useful knowledge.

366. (*Chairman.*) That assumes, does it not, that these experiments are only to be performed by persons of a high scientific character?—Certainly; I think that no others should perform them.

367. If veterinary surgeons and others in country towns operate themselves in experiments of this kind we should be justified in preventing that?—Yes.

368. Now coming up to high scientific names,—Majendie's is a high scientific name; but still the important results which you have attributed to him might have been ascertained with a less degree of suffering to animals if the experiments had been in the hands of some other persons?—Yes.

369. (*Sir John Karslake.*) Would that have been so with regard to the principal experiments performed by Majendie, (I am not speaking of the repetition); was any unreasonable pain inflicted by him considering that at that time there were no anæsthetics?—I think he had a rough way of experimenting; he seemed really quite indifferent to pain.

370. And, in fact, he never made any secret that he was indifferent to pain?—No, I think not.

371. And disgusted people very much by showing contempt for the pain of animals?—Yes.

372. But still many of the experiments made by Majendie were made so successfully that it was unnecessary to repeat them?—Yes.

373. So that science has gained something by those experiments, however cruel he may have been in the execution of them?—Yes.

374. (*Chairman.*) And the same amount of gain to science might have been obtained without that scandal to humanity which unfortunately attaches to his name?—Yes.

375. (*Lord Winmarleigh.*) Do you believe when you come to teach a class it is possible to teach that class as well without experiment as with?—Certainly, generally it is. There are some things which it is very hard to teach unless you can show the very thing. Taking the case I mentioned, of the action of the heart for instance, I do not think that anyone could understand how a heart moves and propels blood if he did not see it, and that is one of the cases in which I should say that living animals may justly be used, because they can be used without the infliction of pain, and immediately the experiment is over they can be killed. They can therefore be killed without the infliction of any more pain than would be inflicted if they were killed for food, or as vermin, or for any other reason.

376. How long would that experiment take which you describe with reference to the heart?—With artificial respiration you could show that, I should think, for 20 minutes or half an hour, without any infliction of pain.

377. And the anæsthetics would last the whole of that time, would they?—Yes, quite well.

378. You can renew the application I suppose?—Yes, you can keep up the effect of the anæsthetic for that or a longer time.

379. (*Mr. Hutton.*) You know, I suppose, nothing about the experiments at Florence, or Leipsig, or Vienna, or Paris?—I know nothing of them.

380. (*Chairman.*) Is there anything else that you would like to add?—I have been thinking over the matter since your Lordship was so good as to talk with me about it, and it has seemed to me that it is very advisable that it should be known how much experiments on living animals contribute to knowledge, without being of themselves the sole means by which it can be ascertained; that they really stand in the place of what would otherwise be very serious experiments upon the human subject; because all our knowledge of surgery and of medicine is progressive. We find, for instance, what we expect to be a sufficient means of cure; but we find, as soon as we begin to use it on the human subject alone, that something is omitted, that some condition was overlooked or never could be discerned; and then comes in the great advantage of experiments on animals to correct our observations. Thinking what instances I might state, the one of the transfusion of the blood has seemed a fit one. I believe the use of the transfusion of blood was discovered entirely from experiments on animals. I think it never occurred to any man that if a person was bleeding to death he should bleed some one else, and restore the lost blood. The first experiments were tried by transfusion from one animal into another and then into man, and a long course of experiments were performed which have shown us this, that transfusion of the blood may certainly save the lives of a considerable number of persons who would otherwise die. It is therefore now resorted to in every necessary case: but of course the experiments on animals led to a great many precautions being found necessary, the overlooking of which would be fatal in the case of man. For example, if in transfusing blood you inject any quantity of air the patient then and there dies; and that could never have been ascertained except by experiments on animals, where the injected air was seen with the blood, and all its effects carefully traced out. Again, if during the transfusion of blood the blood clots, and clots are injected into the vessel instead of the liquid blood, death may ensue; and the manner of preventing the clotting could never have been ascertained except by experiments on living animals. So that I think, looking at those difficulties, you may be quite certain that the practice would have been given up long ago because of its killing people, if it had not been that the means of preventing death were ascertained by experiments on living animals. And there is one other case which I should like to mention, because it so chances that it concerns the last case of experimenting on animals which I had to do with. Some years ago there was a committee of the Medico-Chirurgical Society for investigating the effects of

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anæsthetics; and one object was to determine the means by which the occasional fatal effects of chloroform might be averted. Of course in that case the observations were made on animals put under the influence of chloroform and allowed to die. The end of that course of observations was to find out that we have no certain means by which the fatal effects of chloroform can be prevented. I believe that from 20 to 30 persons die of the effects of chloroform every year in the British Islands alone; I have seen four die of it, and because of that for the last three years I have never had chloroform administered. I have gone back to the old invention of ether, which is very much less dangerous than chloroform. But there are troubles connected with it; ether is so bulky and difficult of administration and carriage that it cannot be used in military service; sailors and soldiers have to be provided with chloroform for operations on them. We therefore are still very much in want of an anæsthetic which should have the convenience of chloroform and the safety of ether. Now we cannot get that unless we try the effects of anæsthetics upon animals and let them die, and see how they die, and then see how their deaths may be prevented.

381. Those animals that die under anæsthetics do not suffer pain, do they?—No other pain than the distress of taking the anæsthetic and the premature death.

382. (*Mr. Hutton.*) It seems to me that you are using the term vivisection in a sense very much short of that in which the new school of physiologists are taking it, who make it a great means of studying the tissues in various morbid states in living animals, and almost the only means?—There must be reason and moderation in the use of these experiments. I can quite believe that ardent physiologists put more trust in the experiments on living animals than I should; and certainly those studying therapeutics and diseases think more of them than I should. I think more of the advantage of clinical inquiry. But I am very anxious that there should not be a general condemnation of experiments on animals, since it seems to me that there are a number of things absolutely essential for the life of man that cannot be ascertained by other means.

383. (*Chairman.*) And do you think that all those things might be secured with comparatively very little suffering to animals?—There is in many of these experiments the suffering of sickness which is inflicted on animals; but the inducement to perform them is very great. Members of my profession, who are constantly in the presence of persons suffering and dying from diseases that we cannot yet cure, cannot resist the temptation of endeavouring to ascertain the means of cure by any possible means, even though those means may inflict upon animals the same sicknesses as we try to cure, and try in vain to cure, in man.

384. But still anything like that amount of abuse which you have so justly reprobated is entirely beside the question?—Yes.

385. And any measures which it might be proper to take to render such abuses impossible would have no effect in limiting the amount of knowledge which you desire to see obtained?—I hope not; but they would need to be carefully adjusted to prevent that result.

The witness withdrew.

Adjourned to to-morrow at 12 o'clock.

Tuesday, 6th July 1875.

PRESENT:

THE RIGHT HON. VISCOUNT CARDWELL, IN THE CHAIR.

The Right Hon. Lord WINMARLEIGH.
Sir J. B. KARSLAKE, M.P.
JOHN ERIC ERICHSEN, Esq.
RICHARD HOLT HUTTON, Esq.

N. BAKER, Esq., Secretary.

Mr. W. Sharpey, M.D., LL.D., F.R.S.

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MR. WILLIAM SHARPEY, M.D., LL.D., F.R.S., called in and examined.

386. (*Chairman.*) I think that for more than 30 years, beginning with the year 1836, you were professor of physiology in University College?—Yes, I was.

387 And you have been long secretary to the Royal Society?—Very nearly 20 years.

388. And you are conversant with the subject for which this Commission has been appointed?—Yes.

389. You have sometimes practised experiments upon living animals?—Certainly.

390. Have you ever exhibited them?—Rarely; but I have exhibited experiments on living animals.

391. But the practice of exhibiting them has been rare, I understand you?—It has been rare; it has been rather exceptional.

392. Do you consider that anæsthetics have made a great change in the question which we are appointed to consider?—A very great change indeed, inasmuch as a great many, indeed most, physiological experiments on living animals may in that way be carried on without inflicting any pain whatever to the animal.

393. Is it your opinion that such experiments are necessary?—I think they are absolutely necessary for the progress of the science of physiology.

394. Will you be so good as to illustrate that opinion?—To take the case of the motion of the blood; Galen proved by vivisection that the arteries naturally contain blood, and not air or vital spirits, as was imagined before his time. Then Harvey, as everyone knows, discovered the true motion of the blood. That was also by vivisection. He exposed the heart in various living animals of different kinds, and watched the changes that the organ undergoes in its action, and studied very carefully the motions of the heart (this must not be forgotten), and the manner in which it impelled the blood, and he then traced the course of the blood along the arteries and its return by the veins, and established in fact the doctrine of the circulation of the blood as now received. That was done largely by vivisection. It is quite true that Harvey told Robert Boyle (in the only conversation which Boyle says he ever had with Harvey), that he was led to distrust the existing belief of the course of the blood by considering the arrangement of the valves of the veins. It was plain, he said, that the common doctrine then received that the blood moved to and fro in the veins outwards from the heart and back again was incompatible with the fact of the direction of the valves, which were so placed that the blood could only move in one direction, or back towards the heart; and Harvey insists on this in his remarkable work,—small in bulk, but very great in importance —on the circulation of the blood. But while he refers especially to the arrangement of the valves as a proof of the motion of the blood in the veins, that by no means constitutes the whole doctrine of the circula-

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tion. A main object was to show that the blood which returned by the veins was really the same blood, changed no doubt in its progress, but the same blood substantially that passed out by the arteries, and that he showed by vivisection. And I have thought it well to make an extract from his work, showing the fact that he had established his doctrine by vivisection on a variety of animals. Harvey knew that the blood passed from the arteries into the veins, but he could not tell by what channels it got from the one to the other; that was uncertain; and he very properly and philosophically did not attempt to determine a question which he had no means of thoroughly investigating. That was reserved for Malpighi, who made use of the microscope, and showed in the transparent parts of cold blooded animals, frogs for instance, the existence of capillary vessels between the arteries and veins through which the blood passes. Now in that way the true course of the blood was established, and also the operation of the heart; because although previously to Harvey's time the heart was recognized as a muscle, its mode of action was not understood and it was not known as a propulsive organ, whereas he proved that it really acts as a propelling organ, and drives the blood out of its cavities. The next step in reference to the physiology of the motion of the blood was made by Stephen Hales. He was a doctor of divinity, and a clergyman, not a medical man; he was the incumbent of a parish known very well to us all, Teddington on the Thames; he was a great experimenter in different ways, physical, chemical, and physiological, and, by experiments on horses and other quadrupeds, he ascertained and measured the pressure of the blood in the vessels. And that was carried out much further in our own time by improved apparatus, chiefly by Poiseuille, and by Ludwig who introduced the method of recording graphically the pressure of the blood. Then arose the question of the time taken for a portion of the blood to make the round of the circulation, and that was established by experiments on animals,—horses, dogs, sheep, and various others. The time was found to be different in different animals, and an inference was drawn by comparison as to the rapidity of the circulation in man; it turned out to be a great deal more rapid than was ever apprehended before. The rate of motion of the blood in particular vessels was more recently determined, as, for instance, in the carotid artery and other arteries of the body, although our knowledge of that is by no means so complete. That, I think, might suffice as to the motion of the blood. I should like to read this extract from Harvey's work. It is from a translation. I would have brought the original with me, but I did not suppose that it would be required: "Since, therefore, from the foregoing considerations, and many others to the same effect, it is plain that what has heretofore been said concerning the motion and function of the heart and arteries must appear obscure or inconsistent, or even impossible to him who carefully considers the entire subject; it will be proper to look more narrowly into the matter, to contemplate the motion of the heart and arteries, not only in man, but in all animals that have hearts, and further by frequent appeal to vivisection and constant ocular inspection, to investigate and endeavour to find the truth. When I first gave my mind to vivisections, as a means of discovering the motions and uses of the heart, and sought to discover these from actual inspection, and not from the writings of others, I found the task so truly arduous, so full of difficulty, that I was almost tempted to think with Fracastorius, that the motion of the heart was only to be comprehended by God. For I could neither rightly perceive at first when the systole and when the diastole took place, nor when and where dilatation and contraction occurred, by reason of the rapidity of the motion, which in many animals is accomplished in the twinkling of an eye, coming and going like a flash of lightning, so that the systole presented itself to me, now from this point, now from that, the diastole the same; and then everything was reversed, the motion occurring, as it seemed, variously and confusedly together. My mind was therefore greatly unsettled, nor did I know what I should myself conclude, nor what believe from others. I was not surprised that Andreas Laurentius should have said that the motion of the heart was as perplexing as the flux and reflux of Euripus had appeared to Aristotle. At length, and by using greater and daily diligence, having frequent recourse to vivisections, employing a variety of animals for the purpose, and collecting numerous observations, I thought that I had attained to the truth," &c. There is another large branch of physiology, that relating to the nervous system, which has been immensely advanced by vivisection. In the first place, there is the determination of the sensory and motory functions respectively of the posterior and anterior roots of the spinal nerves by Sir Charles Bell. He, besides reasoning on the subject, performed experiments on animals, one of which was when the animal was still sentient and conscious, but others were on animals that were stunned, and therefore the experiment was indecisive. He showed, it is true, that the anterior roots were motory, but it was not at all proved what were the functions of the posterior roots, except on the principle of exclusion, that as the whole nerve was sensory as well as motory, and as the anterior root was the motory root it might be presumed that the posterior root was the sensory root; but this was afterwards proved by Magendie, in a perfectly decisive experiment that was performed by him, in which, by cutting the posterior roots of a certain number of those nerves, he destroyed sensibility in a limb, whereas by cutting the anterior roots and leaving entire the posterior roots, he destroyed the power of motion and left the sensibility. After that was settled there was no occasion for further experiment. Then the functions of certain nervous centres, as for example, the spinal cord, the medulla oblongata, have been made out, so far as known, by vivisection; and so also with the functions of the different parts of the brain, though there is a great deal yet to be desired in respect to that, for we are very uncertain about the functions of particular parts of the brain itself; and I must say that much sound doctrine upon that question may be derived from pathological observations in the human subject. But there is a very broadly influential doctrine in the physiology of the nervous system, that of the reflex function, which has been largely made out by experiments on animals, and especially as regards the parts of the nervous system concerned in the production of reflex acts. That has been a most influential doctrine in physiology, and also in relation to the investigation of disease, particularly convulsive diseases,—but no doubt some practical physician can speak more fully upon that point than I can. Moreover, the functions of particular nerves have been ascertained by vivisection. Sir Charles Bell was thus enabled to point out the respective functions of the two principal nerves of the face; and I have no doubt that a member of the Commission here, Mr. Erichsen, could tell us that in former times there was a surgical operation for cutting a nerve of the face for tic doloreux, which nerve turned out not to be a sensory nerve at all. That practice has been exploded in consequence of the results of the experiments of Sir Charles Bell and Herbert Mayo. Then there is a very important nerve, the pneumo-gastric nerve, which, with the part of the brain connected with it, the medulla oblongata, plays a most important part in regulating the movements of respiration. That could only have been made out by vivisectional experiments. It also controls the movement of the heart; by the excitation of that nerve the heart is stopped and recovers again. That is the nerve too that supplies the nerves of the voice; as was known to Galen, and learnt by him through vivisectional experiments on animals.

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Again, while certain nerves depress the heart's action on being stimulated, others increase the heart's action; and as to the mode of their excitation, whether direct and immediate through stimulaus originating in the nervous centres, such as a mental stimulus,—or whether indirect through reflex stimulus,—those questions have been determined by vivisection; and it cannot be doubted that all that must have a very important influence indeed on the progress of medical science. There is still another very important discovery which I should mention, and that is with regard to the operation of certain nerves going to the arteries, which now go by the name of the vaso-motorial system of nerves. Through means of these the blood vessels may be narrowed and their capacity diminished when the nerves are stimulated into action; and on the other hand, when they are paralyzed the vessels are distended by the force of the blood driven from the heart; and this has an important effect in modifying the distribution of the blood; because, although the heart is the great central moving power, the alterations in the circulation in particular parts, and modifications of the rapidity in different parts, are all more or less effected through these vaso-motorial nerves. Not only has the influence of these nerves been discovered by vivisection, but also the part of the great nervous centre on which they are dependent, the medulla oblongata; the fact of that dependance has been discovered by vivisection. And again, these nerves may be influenced mentally, as in blushing, which by the way was wrongly interpreted formerly altogether. I remember that Sir Charles Bell gave the increased size of the vessels in blushing, and their fulness of blood as an example of the increased action of the arteries in driving on the blood. It turns out to be just the reverse, inasmuch as it is owing to a paralysis of the nerves governing the muscular coat of the arteries, by which their muscular power is weakened, not augmented. It is simply like the opening of a sluice, which can, of course, have no effect upon the force moving the fluid, but simply allows a greater quantity to go through. There are other examples of the value of vivisection in physiology, in what relates to the respiration. The movements of respiration; the effects of the suppression of respiration, producing what is called asphyxia; the mode in which the functions of the brain and the motion of the heart are arrested in such case; and the methods of resuscitating animals or individuals that have been suffocated; all that has been investigated through experiments on living animals. Again, I may mention that the importance of the red part of the blood, in combining with certain gases and transporting them from the lungs to the tissues of the body, and thus operating on the tissues, has been made out by experiments on animals; and the effects of noxious gases upon the blood, and the manner in which they occasion death, as for example carbonic oxide, have been ascertained by investigations on living animals, taking recent blood from animals. These are the chief illustrations that I think it desirable to adduce. Some of them do not bear directly upon the practice of medicine and surgery, but I think that is not what I should be inclined to view as the most important aspect of vivisection. I think that vivisection is of value as promoting the science of physiology; and that again is one of the great foundations of all rational medicine. I do not think I need urge upon the Commission what must occur very naturally to them, that, as the practice of medicine consists in investigating and watching the disordered conditions of the actions that occur in living bodies, and the source of these alterations and perversions, their consequences and the methods of obviating them, it is plain that, in order that such investigations may be successfully pursued, the knowledge afforded by physiology of the mode in which these actions are carried on, naturally in a healthy body, is, if not essential, at any rate highly desirable. It puts a lamp, so to speak, into the hand of the physician when he is studying disease. But there are a few cases (although I will not insist particularly upon them) in which experiments on living animals have been directly applicable to the healing art, to improvements in medicine and surgery. For example, it is well known that the different steps in the movements of the heart are indicated externally by certain sounds. Now the physician gains information as to the condition of the heart in disease by watching the changes that have taken place in these external signs by means of the stethoscope and in other ways. It is quite essential that the true relation of such external signs to the actual internal phenomena that occur should be established; and that has been the subject of various inquiries through vivisection, promoted for instance by the British Association for the Advancement of Science, and reported to the association— that is one case. To take another— the surgery of the arteries—the feasibility of tying some of the larger arteries in man was first ascertained by preliminary experiments on animals. And again, a very important inquiry took place early in this century by Dr. Jones as to the process followed by nature in checking bleeding. All this was carefully studied in a series of experiments, and was of much importance in surgery with reference to the treatment of wounded arteries and the effects of ligatures upon arteries in securing them, and Jones (Mr. Erischen will correct me if I am wrong; I have not recently looked at the point), if I remember rightiy, showed the disadvantage of using what at first sight might appear the safest kind of ligatures, large ligatures which were preferred for fear of cutting the vessels. Jones showed that a ligature should be so applied as to divide the internal and middle coats of the artery, and that the desired result of permanently closing the vessel was greatly secured in that way. With the coarse ligature there is a great risk of ulceration of the vessel, and of bleeding from it. That was another case of direct application to the healing art of the results of vivisection. And there is another kind of process to which I may refer, the process of the reparation of bone after injury and disease. In the last century there was an Italian experimenter, Michael Troja, who showed the influence of the periosteum in reproducing bone after portions had been removed or had died; and since then experiments were made by the late Mr. Syme, of Edinburgh, which are related in the Transactions of the Royal Society of Edinburgh, in which he showed the power of the periosteum in reproducing bone. But since then the investigation has been carried much further by Dr. Ollier, of Lyons; and he has shown in a very remarkable way the property of the periosteum, even when removed from the bone, and transplanted to another part of the body, of producing bone; and the results of these inquiries have been applied to the treatment of injuries and diseases of the bones and joints, with great success. There may be other cases that do not occur to me at this moment; but, as I said before, I should lay less stress upon the direct application of results of vivisection to the improvement in the art of healing than upon the value of these experiments in the promotion of physiology as a science.

395. The discoveries of Harvey for instance, and the discoveries of Sir Charles Bell, have been followed by important medical consequences?—Certainly.

396. Regarded by those investigators as scientific experiments at the time, the particular medical or surgical consequences that would result were not foreseen by them?—They were not.

397. But those consequences have nevertheless followed?—Yes.

398. And it is your opinion that the importance of experiments of this kind for scientific investigation is very much greater than for any direct medical or surgical results?—Decidedly. I believe the influence of physiological knowledge upon medicine is one that may not be very conspicuous, but it is not the less true. It operates perhaps impalpably sometimes upon the mind of the practical physician. In short, I should say

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that physiology is not to be compared to the reaping machine, but rather to the plough.

399. The general proposition that scientific investigation is important for the development of medical and surgical improvement is the proposition on which you insist ?—Clearly.

400. Such investigation, I presume, will be useful in proportion as it is carried on by persons of eminent scientific knowledge and attainment ?—Most decidedly.

401. And, therefore, its principal use is limited to those competent persons like Harvey and Sir Charles Bell, and other persons of that kind, who already possess a very high amount of scientific knowledge ?—Yes; it is clear that it must be carried on by persons who are instructed.

402. And unless carried on by highly instructed people it will be comparatively valueless ?—All experiments that are made without a clear perception of what it is desired to learn, and without some experience in the methods of experimenting, are of little or no value; mere random experiments are of no use.

403. You would consider them to be mere purposeless cruelty ?—Mere purposeless infliction of suffering.

404. When Monsieur Magendie had proved the distinction between the motor and sensory nerves more completely than Sir Charles Bell had proved them there was no need of any further proof ?—No.

405. Therefore, any further experiments for that purpose would be a purposeless infliction of pain ?—Quite so. While a doctrine is being established it may require verification, and in that way to a certain moderate extent a repetition of experiments before skilled persons; but once such facts are fully established, I do not think it justifiable to repeat experiments causing pain to animals; such experiments as those of Magendie on the nerves for example ought not to be repeated when the fact has been once fairly established.

406. You said just now that some of the things which have been proved by experiments upon animals might also be deduced from pathological observations on the human patient in the hospital ?—No doubt; but then the knowledge derived from experiments on animals is of great aid in interpreting the results of observations on disease in hospitals.

407. And that result will only be really attained by very scientific observers ?—Distinctly so.

408. We were told yesterday that some of the operations of Sir Charles Bell, by which he showed the distinction between the motor and sensory nerves of the face, were so painless that if the same thing had been done upon a human subject it would have been scarcely thought worth while to introduce chloroform; do you agree to that ?—Well, I should think so. In the first place the cutting of one nerve, the motor nerve of the face, would occasion no pain whatever; and with regard to the cutting of other nerves of the face, the sentient nerves, the pain would cease the moment they were cut across.

409. So that you would agree with the proposition which I quoted, that with regard to some of these experiments, even upon the nerves, the pain was scarcely worth mentioning ?—Yes. Moreover, they could not have been well performed under chloroform, because you would not then have had such clear evidence as to sensation.

410. Professor Syme, whom you have just quoted, was very careful, was not he, always in the infliction of pain ?—Yes; he was not much of a vivisector, indeed he was very little of a vivisector; he did not make many experiments upon animals, but he made some, and very judiciously.

411. And always did it with every regard to the sufferings of the animals that were the subjects of his experiments ?—Yes.

412. I think you have said that the experiments upon living animals may in the great majority of cases be rendered entirely painless ?—Decidedly.

413. Is there not another class of experiments where, though not entirely painless, a very great part of the pain may be spared to the animal by the proper use of anæsthetics ?—Certainly; the actual operation may be rendered painless to the animal, although a certain amount of pain may remain afterwards.

414. You instanced just now the experiment upon the arteries, and the subsequent recovery. We were told yesterday by a very eminent person that the pain of cutting and of tying an artery might be entirely taken away by anæsthetics, and that the subsequent pain of recovery would be comparatively very slight ?—Distinctly so. The arteries themselves are not very sensitive to pain; when an animal starts, or when a person starts, when an artery is tied, in amputation for example, it is generally from the nipping in of a little nerve with it in the ligature; but of course the preliminary cutting of the integuments, and cutting down upon the vessel is accompanied with more or less pain; it is not a very formidable operation as to pain; but that may be all obviated by the use of anæsthetics; and the subsequent recovery, as your Lordship says, is then attended with comparatively little suffering.

415. The number of experiments then where any very severe pain is necessarily inflicted upon an animal is comparatively small ?—It is so.

416. What is your opinion where the pain is necessarily not only great, but also protracted; are there any such experiments ?—Certainly there are some such experiments.

417. Ought there to be ?—There are cases in which they may be justifiable.

418. But those cases would be extremely rare ?—Not common, certainly.

419. And ought not, I presume, to be performed by any but some scientific and experienced person ?—Clearly; I think that all experiments ought to be performed by such persons.

420. (*Mr. Erichsen.*) Would you mention one such experiment as an illustration ?—I might mention the experiment of connecting the gall bladder with an opening in the side of an animal, and cutting off the passage of the bile into the intestines, in order to ascertain the effect of carrying the bile entirely out of the body; that is a protracted experiment and must be severe. Another very severe class of experiments, of which I think the repetition is very questionable, is experiments on starving animals to death. M. Chossat, of Paris, a great many years ago, nearly 40 years I suppose, made a number of experiments on all classes of animals, in which he starved them to death, and in some cases it took many days before the end came, and during that time of course they must have been subjected to great suffering.

421. (*Lord Winmarleigh.*) How long would those experiments of which you speak on the gall bladder last in the average ?—They would be carried on for weeks perhaps.

422. On the same animal ?—On the same animal.

423. (*Chairman.*) All the pain I understand of the mere operation would be removed by chloroform ?—Yes.

424. The pain that you speak of as so severe is the pain of the process by which the natural course of the bile is interrupted, and it is carried out of the body ?—I must correct myself there; I cannot speak very positively (because I never saw the experiment), as to the amount of actual pain that the animal endures; but it is a severe experiment, and injures the condition of the animal. In several cases they have died from emaciation after such an experiment.

425. I think you hesitate to say that experiments of this protracted kind are justifiable at all ?—I think they are justifiable, with the reserve that they must be performed with a very clear purpose, and by a very competent person.

426. (*Lord Winmarleigh.*) What is the benefit derived to the human race from that operation on the gall bladder ?—It is with a view of ascertaining what office the bile performs in the animal economy.

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427. Then it is the same in the human body is it, as in the animal?—Yes.

428. (*Chairman.*) Then the performance of any such experiments as you have last spoken of, though you consider them in some exceptional cases justifiable, is to be condemned unless with those extreme reservations to which you have last referred?—Yes; with the reservation that it should be performed by a thoroughly competent experimenter.

429. If any ordinary person, say a veterinary surgeon in a country town, were to occupy himself with painful experiments, you would consider that very reprehensible?—Yes; unless he was such an experimenter as Professor Hering, of Tübingen, who made that experiment upon the rapidity of the circulation of the blood. There are men in the veterinary profession I dare say, who might be competent to conduct experiments, and in some cases painful experiments. There is a most eminent veterinarian who has conducted experiments both painful and not painful, of an important kind, on horses, M. Chauveau, of Lyons.

430. Nothing was farther from our intention than to imply the smallest reflection upon any portion of the profession, but I presume that the training, in this country at least, which qualifies people to answer the description of the exceptional persons to whom you have referred, as fit to be trusted with these painful experiments, does not apply ordinarily to veterinary practitioners in the country?—Not ordinarily, certainly.

431. Then what I want to deduce from the inquiry is this, that although in your opinion such experiments may sometimes be necessary and may be justifiable, they must be in very limited hands indeed, and that any general power of performing such experiments is a power which is very worthy of being controlled?—That, of course, leads to the question of control. I think if the Commission inquire, they will find that such experiments as we are now speaking of are very rarely performed except by professors of physiology, and others that are competent; that there is no such thing in this country as the prevalence of the indiscriminate practice of vivisection.

432. Is it your opinion that in this country the greatest possible precautions are always taken to prevent the infliction of unnecessary pain?—I do not say so; but I think it is a matter of fact that few engage in such experiments. Few are disposed or inclined to do so. I speak of it as a matter of fact.

433. Do you believe that as a matter of fact, where painful experiments are performed on animals, the utmost use of anæsthetics is always made by the persons who perform the experiments, whoever they are?—I cannot tell. In the physiological schools they employ anæsthetics; but as for the indiscriminate performance of severe operations in various parts of the country I have no evidence of it whatever, and I never understood that there was any reason to believe that such a practice prevailed in this country, and I do not think it does.

434. If such a practice should be shown to prevail it would be an evil which ought to be dealt with?—If it did prevail, certainly. If the evil existed it would be very well to provide a remedy; but I do not believe that the evil does exist.

435. We were told the other day that the worari poison is used as an anæsthetic; do you believe it is an anæsthetic?—It is a very doubtful question to physiologists. I do not think that the urari poison abolishes consciousness. It certainly interferes with the operation of the nerves of the muscles, operating on their extremities, and cutting off the power of the nerves over the muscles, although the will might remain. With regard to the sensory nerves, we are not so well informed; but I think it is not unlikely that the extremities of the sentient nerves may also be affected so as to interrupt the communication of impressions, painful or otherwise, to these nerves. But I think that is a matter which requires further investigation, and it is a question by the way for experiment.

436. But what sort of experiments can you apply which will test the difference between stopping the motor nerves and stopping the sensory nerves; the animal cannot speak to tell you whether it feels pain or not?—It is not easy. The paralysis of movement is no proof of the abolition of will; because if you cut a nerve going to a limb you still may have the will to move it; but in consequence of the continuity of the nerve being interrupted you have no power over the muscles. Now it is the same thing with sensation; but we cannot tell so clearly that the urari poison interrupts the transmission of sensorial impressions. There seems some reason to believe that it is so. A part of the body of an animal, say of a frog, may be protected from the influence of the poison which operates generally, thus a particular limb, by tying the vessels of it, is saved from the effects of the poison, and the rest of the body is affected by it; now it might be shown, and it seems to be the case (but I have not made that experiment myself), that while a certain amount of irritation applied to the part that is protected from the influence of the urari poison is followed by the evidence of pain, the same infliction upon a part of the skin, we will say, which has been affected by the urari poison is not followed by the same indication of pain, so that it is not at all clear that the urari poison does not actually prevent the sensation of pain.

437. Then if experiments *primâ facie* painful are performed upon animals with no other security against pain than that of this particular poison we have no scientific security at all that the end is really attained?—No, we have no security that the sensation of pain is abolished.

438. It is a mere chance?—It is a mere question of probability.

439. Is it even a probability?—Well, I will not undertake to say, because it is a physiological question that really has yet to be satisfactorily determined.

440. (*Mr. Hutton.*) You are aware that Claude Bernard gives a very strong opinion the other way; he says that the patient suffers just as much as ever?—Unquestionably Bernard's authority must be taken as of much weight for that.

441. (*Chairman.*) That being so, is it legitimate to hold out to the public the notion that experiments of this kind are performed under the worari poison are performed under an anæsthetic?—I should explain to your Lordship that I never held the opinion that urari took the place of an anæsthetic.

442. I never supposed that you did. I had an object in putting the question, which I can explain to you if you like?—I wished simply that what I said might not appear as said in support of the notion that urari was a sufficient anæsthetic. I was merely dealing with a nice question in physiology.

443. If the public have any rights at all in this matter to have their feelings respected, those rights are not regarded when the experiments performed under the worari poison are held out as experiments performed under an anæsthetic?—No, I think not. I think that that is not a sufficient answer to the public.

444. You have mentioned the name of Mr. Magendie; were you ever yourself a witness of any of Mr. Magendie's experiments?—I have been. I may mention to the Commission that when I was a very young man studying in Paris, I went to the first of a series of lectures which Magendie gave upon experimental physiology, and I was so utterly repelled by what I witnessed that I never went back again. My objection to those experiments was twofold. In the first place they were painful (in those days there were no anæsthetics), and sometimes they were very severe, and then they were without any sufficient object. As an example, I may tell the Commission that Magendie made incisions into the skin of rabbits and on other creatures to show that the skin is sensitive. Now surely all the world knows that

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the skin is sensitive; no experiment, painful or without pain, is wanted to prove that. Then several of the rest of the experiments which he made were of a similar character, and he put the animals to death finally in a very painful way. The consequence was that I never went back to that course of demonstrations.

445. Now may I not infer that, in this country at least, and I hope in other countries also, the leading medical men and the leading scientific men may be relied upon as friends of humanity as much as eminent men in other branches of knowledge?—Clearly so. If you even take the case of those scientific men who have engaged in vivisectional experiments you will find that they do so with great reserve, and with a desire to set an example to others in that respect, and to avoid experiments that are severe unless when they are absolutely necessary for a scientific object, and in fact to discourage as far as lies in them the practice of painful vivisection.

446. This principle being, that no experiment should be resorted to except where its necessity for some useful purpose can be proved?—The question is, what is a useful purpose.

447. But assuming that the useful purpose cannot be shown, then they would desire that the experiment should not be performed?—I think decidedly so.

448. If performed where anæsthetics can be made use of entirely to annihilate pain, they would agree that anæsthetics ought to be made use of?—Most certainly.

449. And not anæsthetics such as that we have just spoken of, but anæsthetics the effect of which is perfectly well known?—Yes.

450. If experiments are partly painful, and the chief pain can be destroyed by anæsthetics, they would think it right that anæsthetics should be so used?—Certainly.

451. And if there are any painful experiments that can be mainly prevented by anæsthetics they would at least be of opinion that those should be minimised and that every merciful provision should be made?—Unquestionably. If you would allow me to say so, I think further that where anæsthetics are used it is simply a question of the sacrifice of an animal, the sacrifice of life. If the experiment involves subsequent pain after the termination of it I think the animal should be destroyed, and I think that under these conditions there need be no restriction imposed on experiments performed under anæsthetics, or in fact on experiments in which the animal is rendered insensible in any appropriate way. For instance, you render a frog insensible by destroying the brain rather than by using anæsthetics, you can render it thoroughly insensible in that way; and I think that so long as the animals are in a state of insensibility there ought to be no interference with the free use of animals for a scientific purpose.

452. But you admit clearly and without hesitation that the infliction of pain is an evil which ought to be minimised?—Certainly.

453. And you think that the Crown and Parliament might rely upon the support of the most eminent men in the kingdom if they limited these experiments with hat object?—I do not dissent from that opinion, but I have this qualification to put to it: I do not think that there are more than perhaps a dozen experimenters in this country who practice vivisection, and that not to any great extent. It is not practised by students. I have heard it asserted that it is practised by students. I can only speak of the school which I have been so long connected with at University College, and I know that there it is not practised by students. The exhibition of experiments might take place to the whole of a class of students in the lecturing theatre, or to a limited number of students in the laboratory; but in these cases the experiments are almost all performed under anæsthetics, and those which are performed before an audience of students in the lecturing theatre are very rare. I think a student ought to be admitted to witness experiments. I think the purpose of teaching a science is not simply to teach it as it is, but to enable those who engage in it to take advantage of opportunities that may occur of advancing the science, and if a student has never seen an experiment performed, and does not know the mode of doing it, he cannot take advantage of these opportunities; and he is more likely to inflict pain and do harm in his attempts to make physiological experiments. I should say, therefore, that so long as the experiments are made under anæsthetics there is no need for any restriction. And as to the question of the purpose of them the Legislature will probably find it difficult to define what are experiments "for the purpose of discovery." I see in a draft of one of the Bills which have been brought before Parliament the legitimate purpose is defined as being for the sake of a new discovery, and for no other. Now I doubt if any enactment in such terms as those proposed would be operative, because it is perfectly well known that in the history of science there has been no more fruitful cause of dispute than the question of what is a new discovery, and what is an original observation; and if the case ever came into a court, I have no doubt that you would get a number of eminent persons on one side, and an equal number on the other, who would maintain different opinions upon that, and give different evidence, although both sides would be perfectly honest in their expression of opinion; and therefore I believe that it would be impossible to get a conviction for the infringement of such an enactment. I think it is far better, if there is to be anything of the kind, that the restriction should simply be that it is "for a scientific purpose," and that you should trust in fact to the influence of public sentiment, and the example of the leading men in physiological science to correct any excesses that might be committed. I believe that in this country such evils generally become corrected in that way in course of time.

454. But in the meantime a great deal of extremely objectionable cruelty might take place?—It might take place; but I do not know that it does. I have never yet heard any evidence of it.

455 (*Lord Winmarleigh.*) How would you account for the great excitement that there is at the present moment in the public mind with regard to vivisection; do you think it is altogether unfounded?—I suppose it may perhaps be from what people hear of what goes on on the continent. It is one of those excitements in the public mind, and among a certain class of people particularly, that from time to time occur.

456. May I ask you what means you have of judging whether there is or is not a large practice of vivisection going on throughout the country?—My evidence is simply negative, that I have never seen any evidence or heard any evidence of it; and I believe if the Commission would make inquiry they would find that there is no ground for the belief. In fact I think there are a certain number of people in the country who are hunting a mere will-o'-the-wisp, and drawing sober people after them.

457. Could you give the Commission any idea of the chief places where vivisection is pursued at the present time?—I should say that in some of the schools there is vivisection going on; for instance in University College, but not by the students, and, as a rule, when of a nature to cause serious pain, the experiments are performed on animals rendered insensible—in all but exceptional cases, and these must be very few. Then I should say at Guy's Hospital experiments are carried on; and at Barthlomew's Hospital, chiefly experiments on the action of remedies; which, by the way, is a very justifiable kind of experiment to make, and very useful in medicine, to ascertain the power of medicines over the animal economy; also at King's College, and by Dr. Richardson at his private residence in London, and Professor A. Gamgee at Owen's College, Manchester. And then in Edinburgh, Professor Rutherford and Dr. McKendrick carry on experiments; and again

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Professor Foster, of Cambridge, is very much employed in experiments on living animals, in fact many of his are vivisection experiments; but I know that he is equally careful in reference to the use of anæsthetics.

458. There is an impression, at least I have reason to know that there is an impression, in certain quarters that these experiments are carried on independently by men of eminence in different parts of the country for scientfic purposes, but without any control whatever, and merely to satisfy themselves?—I should explain that independent experimental inquiries are not numerous, and their nature and results are made known through scientific publications. I believe that it is an opinion without sufficient foundation.

459. If such were the case you think the medical profession generally would be able to trace it?—I think so.

460. They would be acquainted with you think? it—Yes; of course it would be a strong thing for me to say that it does not occur; all I say is that I do not know of any evidence of its prevalence.

461. Have you any knowledge of the mode in which it is practised at these other schools besides the London University?—No, I am not aware of that; I presume at any rate that they use anæsthetics wherever they are applicable.

462. (*Chairman.*) Would you be surprised if it should be proved that the worari poison is used and is called an anæsthetic, and that animals are subjected to operations which must be very painful, unless the opinion, which appears to be the less sound opinion, is proved true, namely, that the worari poison is an anæsthetic?—I have no evidence about that; I cannot tell. I dare say that the urari poison has sometimes been considered a sufficient anæsthetic, but I do not think that it is used as a substitute for such things as ether or chloroform, or chloral, or opium injected into a vein. The chief use of employing urari is to render the animal quite still; that is the great purpose of it. What Mr. Hutton says is quite true, that it is not generally recognized as an anæsthetic, and, therefore, not used as an anæsthetic.

463. Then it is a contrivance to save to the operator the trouble which the manifestation of pain by the animal might occasion him?—It facilitates the operation at any rate.

464. (*Lord Winmarleigh.*) There is one point upon which I did not quite apprehend the difference of your answers. You stated early in your evidence that you thought a repetition of Sir Charles Bell's experiments would not be justifiable; that where the facts have been successfully ascertained the experiments could not justifiably be used in the teaching of classes?—No; because in the first place they could not be used under anæsthetics. You see anæsthetics would destroy the sensibility, and you could not use such an experiment under anæsthetics. And it is a painful experiment in two ways: in the first place, exposing the spinal cord and laying bare the roots of the nerve is a very painful operation; that part of it might be done under anæsthetics, and then the animal may recover its sensibility; but the subsequent prolongation of the experiment would be painful.

465. You have restricted your answers to experiments which cannot be made without inflicting pain, when you made the observation that you would not allow a repetition of Sir Charles Bell's proved experiments if they can only be carried out, accompanied by pain?—No; because I think the matter has been so thoroughly made out already.

466. In your subsequent answer I rather understood you to say that in the teaching of classes you did think it necessary in order to teach young medical men their profession that they should see experiments, but I now apprehend that you would limit those experiments to those which can be exercised without pain?—Clearly. I used often enough to use the dead body of an animal, for instance of a dog, for certain purposes; for instance, to show the manner of determining the pressure of the blood, by connecting it with an apparatus and sending water through the blood vessels through the arteries, and using an elastic gum bag to imitate the movement of the heart; and a great deal may be done in that way; but at other times I have had an assistant to show the experiment after the lecture to those students who desired to see it in the living animal, an experiment under anæsthetics in which the animal suffered nothing.

467. (*Sir John Karslake.*) Do you agree with a gentleman who was examined yesterday, that it is almost necessary, in order to instruct students in the action of the heart, that they should see the thing itself?—Yes; I think it is that they should see it themselves, and that may be done quite well when the animal is in an insensible state.

468. Will you let me ask you about this worari; as far as you understand, it is not used generally as an anæsthetic?—It is not with that intention.

469. May I ask you this: assuming it not to be an anæsthetic, is there any reason at all why that which is clearly an anæsthetic, like chloroform, should not be used in lieu of worari?—I could not exactly venture to express an opinion, not having in my mind all the different experiments.

470. Might I ask you this: is it your view that where worari is used it is merely to facilitate the operation, to keep the animal still while it is under the operation, and not in the least with the intention of deadening pain?—Well, I believe it is with the intention really of rendering the animal still.

471. And it is assumed that in rendering it still it still suffers the pain?—That is just the point in question.

472. At all events it is not used as an anæsthetic according to your view?—It ought not to be trusted to as an anæsthetic; that is my clear opinion.

473. Then it ought to be known according to your view that if it is used as an anæsthetic it is not proved to be sufficient for that purpose?—No.

474. Now you have spoken as to what you witnessed in days long ago in Paris, when Magendie was performing his operations. Notwithstanding what he did, would you say that they were utterly unjustifiable as far as you saw; he did throw light on a good many matters, did he not, by his operations?—Yes, but some of his experiments excited a very strong feeling of abhorrence, not in the public merely, but amongst physiologists. There was that, I was going to say famous experiment, it might rather have been called infamous experiment of his upon vomiting. I do not wish to occupy the time of the Commission in stating it at length; but substantially it was this: that he substituted a pig's bladder for the stomach of a dog which he had cut out and then filled the bladder with water, and induced vomiting by injecting an emetic into the veins; and the object of that was to show that the stomach, although it has muscular coats, was passive in vomiting, and that it was emptied merely by the pressure of the muscular walls of the abdomen and diaphragm; and the experiment, besides its atrocity in point of severity, was really purposeless, because it merely proved that if a bladder filled with water was compressed when the orifice of it was left free the water would come out. Surely we did not need that experiment to show such a simple result. Yet it was shown to the French Institute, and reported upon.

475. That you would rank among perfectly unjustifiable experiments?—Yes, and it has been so characterised in the writings of other physiologists.

476. Did he not perform operations which consisted in removing a portion of the brain and keeping the animal still alive?—Those were done chiefly by Flourens and others.

477. Where those experiments which led to advance in scientific knowledge?—No doubt some of these operations on the brain have; though we are yet very much in the dark as to the brain, still a certain progress was made by certain experiments of Flourens.

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478. In your judgment, could that progress have been made without those experiments ?—It is not easy to say on the spur of the moment; but I think there were points made out by Flourens, and some of those that followed him, which could not have been so well shown otherwise.

479. Now you expressed an opinion that these experiments upon live animals are not largely practised in England ?—I have a very strong opinion on the subject, that they are very little practised in England.

480. Now might I ask you whether, according to your reading or information, experiments such as you have described, namely, merely to ascertain that which you say is proved, by cutting the skin of an animal, are practised in France to a large extent; I mean experiments of that kind ?—I should not think it. There have been courses of experimental physiology besides those of Magendie, but I do not think they generally turn upon matters of that sort.

481. Then you would not say that according to your information the practice prevails extensively in France, or abroad generally ?—It is not that I am avoiding answering the question; but of late years I have not been so conversant with all that goes on abroad.

482. (*Mr. Erichsen.*) The experiments that are performed in this country are chiefly performed in medical schools according to your knowledge ?—I believe so.

483. And in the medical schools in the physiological laboratories of those schools ?—Yes.

484. These physiological laboratories have been established recently in a great measure, have they not, under the direction of the examining authorities and bodies in this country, such as the College of Surgeons ?—Yes.

485. Requiring a certain knowledge of practical physiology to have been acquired by the student ?—Yes, but not requiring that he shall perform the experiments.

486. And there is a great deal in what is called practical physiology which has nothing to do with vivisection ?—Yes.

487. And a great part of the business of these physiological laboratories is occupied with matters unconnected altogether with vivisection or experiments of any kind upon live animals ?—A great part of the work in a physiological laboratory is microscropic work, examination of the tissues.

488. And in these physiological laboratories the experiments that are made are not usually made by students ?—Not by the students. There is a thing which occurs to my mind, and which I will mention lest I should be misunderstood. Very early in my career at University College there was a very able student, a Mr. James Blake, who has long since been settled in California, who made experiments while he was an advanced student upon animals chiefly to determine the operation of medicinal and other agents introduced into the blood; and these experiments have been much esteemed and valued in physiology, and also by the medical profession.

489. It was an exceptional case ?—Quite an exceptional thing. At that time there was no physiological laboratory at all.

490. Then with regard to this point of the sensibility of animals, there are one or two questions which I should like to ask you. Are there other methods of producing insensibility to pain in living animals besides the administration of anæsthetics ?—Yes; by pithing the animal, destroying the upper part of the spinal cord.

491. That was the method of Sir Benjamin Brodie, was it not ?—Yes, it was very much practised by him. In that case all the body below the medulla oblongata is rendered quite insensible.

492. And the organic life of the animal can be maintained by artificial respiration ?—Yes.

493. So that the functions and the operations of the body go on as if the animal were alive although he is absolutely insensible ?—Yes.

494. And in some of the cold blooded animals, the frog for instance, the brain can be removed and the animal will live for a long time. For how long will it live without a brain ?—It might live for days; that is to say the circulation of the blood will go on and respiration.

495. But in a complete state of insensibility ?—Yes.

496. Have you been able to form any judgment, either from your own observation or from reading, about the relative sensibility of say, first of all, the vertebrate and invertebrate animals ?—The invertebrate creature, if you except perhaps insects, do not show great signs of sensibility; but of course they are endowed with sensibility. But when you come down to the invertebrated you come to some very low organisms.

497. Between cold and warm blooded animals, say for instance the frog and the dog, is there a difference in that respect ?—I should presume that the sensibility was more acute in the warm blooded animal. But it is an interesting fact that different animals of the same species show very different degrees of sensitiveness to pain; that has been noticed by experimenters. I think particularly the late Dr. Waller, who was a great experimenter on animals, used to point out that there were great differences in dogs, that some seemed to care very little about the operation. And a remark to a similar effect is made by Vesalius. He was a great vivisector; in his great work on anatomy he has a chapter upon vivisection, and the manner in which it should be performed; and he gives the figure of an animal arranged for the purpose of vivisection. He used dogs and he used also pigs for the purpose; and he says that in some cases, especially when you are wishing to make experiments on the voice, the pig is better adapted than the dog, because after there had been incisions made and the nerves of the voice exposed, he had great difficulty in irritating the dog so as to cause it to cry or endeavour to cry. He says that it neither barks nor howls; and apparently the inference was that it seemed to suffer very little.

498. What is the date of that work ?—It was published in 1543, I think; the first edition.

499. So that we may take it that in the 16th and 17th centuries vivisection was extensively practiced ?—Yes; indeed it goes further back; to the time of Galen. Galen says he has got a great number of different kinds of tables, on which animals of different sizes may be fastened, and he tells how they are to be fastened, and so on; and he made experiments on section of the intercostal muscles, and section of the intercostal nerves, difficult experiments, and also in cutting across the spinal cord; and he says that for that purpose you may use an adult animal; but that is not so easily managed on account of the difficulty of laying open the vertebral canal. He says he prefers a young pig a few days old, because it is easy to cut across the vertebræ over its spinal cord; and he says that he finds on cutting the upper part of the spinal cord close to the head that the animal is completely paralysed, respiration ceases, and the animal dies, but that if you cut it below the neck you merely stop the movement of the ribs, that the movement of the diaphragm goes on, and respiration goes on; and he mentions particularly the knife that he uses. He says that he prefers a knife of Noric steel, which was much esteemed in Rome.

500. That was in the second century ?—It was in the second century.

501. So that at that period, and again at the period of the revival of letters, vivisection was performed ?—Yes, and I may mention further that Vesalius speaks of showing these things to his auditors, but explains that they are shown to persons well versed in anatomy; and he says that he commonly, before making the experiment, explained to the auditors the different steps of the process, and the purposes of them, in order that he might not occupy time during the experiment by giving these explanations; and he speaks of those auditors that are near the table as being able to put their hand, and feel, for instance,

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the movements of the diaphragm, and those at a distance would see certain other phenomena which it was desirable for them to be acquainted with; so that he was in the habit evidently of exhibiting these experiments to a class.

502. You were speaking of the sensibility of different animals of the same species varying?—At least the manifestation of it. I cannot well tell what they feel. I do not know, for instance, what you feel; but it is the manifestations of feeling that I speak of.

503. The dogs that are used are usually curs and mongrel breeds, and are got at a very cheap price?—Yes; it is a mistake to suppose that physiologists are indifferent to those things. I remember perfectly well when I taught anatomy in Edinburgh, and made experiments on animals, particularly with poisons, there was a bitch that was brought to the establishment that had a litter of pups; and she remained so long that she became acquainted with the people, and of course she was sent away; one would not experiment upon her.

504. To come to that point, with reference to the employment of experiments in order to determine the action of poisons on the system, there are two classes of experiments with reference to drugs and poisons, one of a toxicological, the other of a therapeutic nature; as for instance the administration of calomel to animals, and we were told by an eminent physician yesterday that those experiments are somewhat untrustworthy; is that your opinion?—You mean, as I understand, what might be called therapeutic experiments. I am not prepared to say.

505. But with reference to the administration of poisons; for instance, would it be proper in your opinion to experiment upon an animal with a new substance which might be supposed to have medicinal or poisonous properties; to administer that substance in the first instance to animals rather than to try its effects on man?—I think common prudence would suggest that.

506. It is commonly done, and it is a proper course to pursue you think?—Yes.

507. Do you think also that it would be advisable that students should sometimes have an opportunity of seeing on animals the effect produced by certain poisons?—I think decidedly; such, for example, as strychnine, which is a severe poison.

508. So that they may recognize its effects if they were to happen to see them in man?—Yes.

509. Because the effects are the same in fact?—The effects are the same. It is a more striking thing if they should ever happen unhappily to see a case of a human being suffering from the effects of these poisons, particularly strychnine; but in the absence of that experience it is of moment that they should see the effects in an animal, and then one animal would suffice for a whole audience of 300 students.

510. (*Lord Winmarleigh.*) Would you make that experiment under anæsthetics?—It could not be well made under anæsthetics, it would frustrate the experiment.

511. (*Sir John Karslake.*) Death is almost instantaneous if you give strychnine, is it not?—It takes some time, but not many minutes.

512. (*Lord Winmarleigh.*) You say there is a difference between vertebrate and non-vertebrate animals in the way of manifestations of pain?—I should rather think I was venturing too far to say that, because there are great varieties among the invertebrate animals; I cannot say how much a worm for instance may suffer.

513. If you put a worm on a hook for fishing it writhes about very violently?—I do not know that the pain is to be measured by the writhing of the worm upon the hook, because that might be produced perhaps by the muscular excitement of the animal independently of any very great sensation of pain. I cannot venture to say as to that, but if you take such creatures as polypes and sea anemones, and the like, where you may cut them in pieces and every piece grows into a new animal, I do not suppose that that involves a great amount of suffering.

514. The writhing of the worm does not you think necessarily imply pain?—Not unequivocally. I am unable to say. It is rather a hazardous thing to say.

515. Have any experiments been made upon animals which led physiologists to believe that there is a difference in the degree of sensibility in different animals?—I dare say; but you find a difference in individuals of the same species. I pointed that out a moment ago.

516. But no experiments have been made which clearly prove that any animal of the creation is free from pain under these experiments?—No. Of course there are some exceedingly low organisms, very low in constitution, and very simple in their nature, in which it is extremely difficult to interpret the phenomena, and say whether they indicate sensation or not.

517. (*Mr. Erichsen.*) With regard to the practice of vivisection abroad as compared with this country, from special circumstances you may not have visited lately foreign schools, but I know that you are well acquainted with foreign literature, and in comparing the accounts of experiments on animals as published in the scientific literature of Germany, or of France, with that in this country, do you not think that vivisection is far more extensively practised in the schools abroad than here?—Far more extensively practised. The fact is that to the credit of the scientific men of Germany, it may be said that they are very zealous inquirers in science, and there are many centres of scientific activity in the different universities of Germany, and of course that gives scope for many more experiments of all kinds, physical, chemical, physiological, and so forth; so that naturally many more experiments are performed there than here.

518. It is rather Germany now that is the centre of that scientific movement than France; France in former days I take it, and Germany rather more at the present time?—Still there is a very eminent inquirer in France, Claude Bernard, who in his day has been one of the most active and one of the most successful promoters of physiology, and much of that result has been through experiments. I do not know how far he is active now.

519. Is it to investigate known truths that all this is done in Germany, or is it to advance science?—It is no doubt principally for the advancement of science.

520. Not so much for verification?—No doubt also for verification; the establishment of doctrines; because commonly you cannot establish a doctrine by one or two experiments, and so that involves sometimes repetition, under careful conditions.

521. In your opinion, if a man wished to establish a doctrine, or a point in physiology, it would be insufficient to base that upon one or two experiments; in order to avoid a repetition of unnecessary experiments afterwards it would be better that he should perform then a series of conclusive experiments?—I think so.

522. If it were possible to do so?—Yes; but of course that would not exactly be the course that things would take, because it is quite clear that some other scientific inquirer would be disposed to take up the subject, also with a view of verifying the results, and of correcting them if necessary.

523. In connection with that, might I ask whether in verifying these results new channels of discovery are not sometimes opened up in a very unexpected manner?—Yes; a man goes out in quest of one thing and finds another.

524. (*Chairman.*) Still you would not recommend the promiscuous exercise of a practice of subjecting live animals to painful experiments by uninstructed persons, in the hope that something might turn up?—Clearly not.

525. (*Mr. Hutton.*) I think from what you said that you agree with Dr. Michael Foster in this handbook, which he has dedicated to you, when he says at

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page 341 that "The methods of experiment and "observation are becoming year by year more "physical in character, and the observations them-"selves fundamental in their nature and having the "widest bearings in all the higher branches of phy-"siology, may for the most part be conducted on "frogs, may be repeated any number of times without "difficulty or expense, and so serve usefully as a "means of training students in physiological study and "inquiry?"—I think that is true.

526. The physical method is increasing rapidly?—Very rapidly indeed; but that is one of the great characteristics of modern physiology, the application of physics to the phenomena of life, particularly in making exact measurements and numerical determinations.

527. You were speaking of the German schools just now, you quite approve of the extension that this experimental method is taking in Germany?—Yes.

528. And you would like to see it extended in a similar way in England, so far as required by scientific inquiry?—Yes; the application of exact measurements and determinations by physical and chemical methods to the phenomena of life. I wish you to understand that I rather demur to the inference, that because a book was dedicated to me I adopt all the conclusions in it.

529. I did not intend to suggest that; I thought you might have read the book?—I had no fear that you would misunderstand it, but it might appear so to others.

530. Have you been able to read the book?—I have read some parts of it; I have read Professor Foster's part particularly.

531. You were saying just now with regard to anæsthetics, that anæsthetics have made a very great change, and that most experiments can be made without pain. I have looked through this book very carefully, and it seems to me that by far the larger number of experiments given in it are not made with anæsthetics at all, and that especially with experiments on frogs, which Dr. Michael Foster insists upon as the typical experiments; urari is almost the sole instrument used to render them motionless for the experiment?—Well, there is little occasion for that.

532. I am told that there is an occasion for it, that the effect of chloroform and other anæsthetics on these lower animals is so serious that it destroys the animal for the purpose of the experiment?—It may. I remember making a number of experiments on frogs myself some years ago with reference to the effects of certain poisons, and I began by trying the effect of alcohol, and found that it was such that I could not use the substances in tincture, that alcohol had a most highly narcotic effect upon the frog; but in most experiments on frogs you can destroy the brain.

533. But in a very great number of these cases given in this book, the brain is not destroyed I see, and a great many of the most instructive experiments are those of comparing frogs with the brain destroyed with those with the brain not destroyed, in order to show the difference of the results?—That is in reference chiefly to what are called the controlling centres of the reflex action; that is very true. In these cases they are not urarised frogs if it was for that purpose, but there are experiments in which the brain is partially removed. On since referring to the handbook I find that of 138 experiments directed by Dr. Foster, 113 are on (generally detached) limbs of frogs, and after destruction of the brain and spinal cord; eight on the nerve roots with or without previous destruction of the brain or pithing—in the latter case the *dissection* to be made under chloroform; seven on partial removal of the brain, the necessary previous dissection under chloroform; eight on urarised frogs; one on a rabbit on recurrent sensibility (not actually performed by Dr. Foster), the dissection under chloroform; and one on the ear of a pigeon.

534. I have read an opinion expressed by a respectable physiologist that, so far from anæsthetics having been really a blessing to animals for the purpose of these experiments, they have removed the odium of the experiments and only in a very slight degree saved the animals from pain, and certainly not the lowest order of animals?—If you take the experiments on dogs and rabbits, it has made a very great difference.

535. To them, no doubt; but as to these frogs has it?—A great many experiments on frogs can be made with destruction of the brain as I have said, and it is only now and then that these others are necessary.

536. However, I should say that in the majority of these cases given in this handbook the experiments were painful as far as I could see. Now I do not fully understand whether you justify painful experiments for the purposes of demonstration or not. Let me read this to you; it is one of Dr. Michael Foster's chapters to illustrate this proposition, that "the posterior roots are the channels of the centripetal (sensory), the anterior of centrifugal (motor) impulses," to which you referred I think. "Recurrent sensi-"bility. This is never witnessed in the frog. It can "only be shown in the higher animals, the cat or dog "being best adapted for the purpose. The method "adopted is very similar to the above, the arches of "one or two vertebræ being carefully sawn through or "cut through with the bone forceps, and the exposed "roots being very carefully freed from the connective "tissue surrounding them. If the animal be strong and "have thoroughly recovered from the chloroform, and "from the operation, irritation of the peripheral stump "of the anterior root causes not only contractions in "the muscles supplied by the nerve, but also move-"ments in other parts of the body indicative of pain or "of sensations. On dividing the mixed trunk at some "little distance from the junction of the roots, the con-"tractions of the muscles supplied by the nerve cease, "but the general signs of pain or of sensation still "remain." I suppose that is a well established experiment is it not; it is not one that is now necessary?—Certainly I do not think it is necessary for exhibition at any rate.

537. It strikes me that the intention of this book, certainly the one that ordinary readers would gather from it, is that these are experiments for the purpose of teaching a class, which should be demonstrated before a class. Would you justify that or not, supposing that is the intention of the book?—All I can say is that I should not perform that experiment before a class.

538. It strikes me that the alarm which has been caused, and which you seem to treat as a mere following a will-o'-the-wisp has been to a large extent caused by the beginning of a new school of physiology in this country, in which, besides using these experiments for purposes of inquiry, they use them also for demonstration, as seems to be the case with this book. Do you not think that that is a natural effect of the very large study of our physiologists abroad, who are bringing the views adopted abroad into this country very much?—I have no doubt that young physiologists who have been studying abroad, in the schools particularly at Leipsic and elsewhere, would naturally be induced to introduce methods followed there in their teaching in this country. I have no doubt of that; but still I do not believe that it goes on indiscriminately as has been alleged; that the experiments are carried on by persons who are not in the position of teachers for instance, or scientific investigators.

539. That I have assumed; but would you justify this, even if taught by people who are scientific investigators?—Do you mean that experiment?

540. That class of experiments, experiments of that kind; to illustrate to a class truths already ascertained, but which would be better realised no doubt by the help of an experiment?—I think that where the illustration involves anything like severe suffering to the animal it should be abstained from.

541. (*Lord Winmarleigh.*) Without any exception do you think?—I think for mere exhibition it should, unless in the case that Mr. Erichsen referred to, the

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case of exhibiting the phenomena produced by certain poisons.

542. (*Mr. Hutton.*) That you would justify for the sake of making the pupils apt in the study of disease?—Yes.

543. When you said that this method is purely for scientific ends, I understood you, to put it very largely, that you did not justify this simply for the purpose of opening up inquiries with a prospect of benefit to man, but for purely scientific ends. In that sense surely it is the fact, is it not, as Mr. Ray Lankester asserted not long ago, that the number of experiments of this kind which can be performed, and ought to be performed, will increase in almost geometrical progression as the science enlarges?—I cannot venture to predict how that may be, because the progress of science supersedes so many experiments.

544. But it suggests many more, does it not?—It may lead to fresh experiments, but it supersedes many. The progress of science has been through the discovery of anæsthetics the means of avoiding many painful experiments formerly necessary.

545. Of course in every experimental science every new light must suggest a vast number of new deviations of inquiry; and it is not the less so because the *modus operandi* is painful; it will be just the same here as in any other experimental science, will it not, that every new discovery will suggest a vast number of new methods of inquiry?—Of course every new discovery widens the prospect, and brings us into wider contact with the unknown.

546. Do you not think that that is rather an alarming prospect for this country, that the number of painful experiments are likely to multiply in geometrical progression as Mr. Lankester says?—"Geometrical progression" is a very strong expression. New directions may open up in which experiments are performed. New lines of inquiry, for instance, for which experiments may be needful; but then, as said before, the progress of science tends to supersede experiments previously performed. Moreover, I apprehend that all such experiments, however extensively they are performed, would be under the direction of some skilled and competent person.

547. That we are assuming; still what you look to is that the prospect of an indefinitely larger number of painful experiments will open upon us as science is pursued?—I do not see any real prospect of that indefinite number of painful experiments.

548. Now there is a class of experiments which seems to be rather a new one, the experiment on tissue, and to which the editors of this handbook seem to attach great value. Those experiments they seem to find necessary to conduct on the living animal; and surely that is a new class of experiments?—Which do you refer to?

549. Such an experiment as causing inflammation in the cornea of a frog's eye, and then removing that as a tissue for further dealings with?—A single operation of that kind on a frog's eye would probably serve two or three dozen pupils for the purpose of microscopic examination; and I do not know that it is of a very painful character. Besides, that experiment might be made by destroying the brain of the frog.

550. It does not appear to be so made?—It might be done.

551. Would not that interfere with the result in any degree?—No; I do not think it would.

552. Then you were referring to Ludwig's method of recording the pressure of the blood. Now I am told that Ludwig has an engine at work to secure artificial respiration, which is at work day and night, or at least to a very great extent; the scale of the operations is so large?—I have seen the apparatus; there is a similar one at University College. It is simply a gas engine.

553. It is for the purpose of keeping up artificial respiration in a considerable number of animals; it would not be necessary for a few?—But these animals may be perfectly well rendered insensible; and besides it is no great matter as to whether you use an engine for the purpose or whether you use the hand.

554. Of course not; but it is simply as a test of the number of experiments requiring to be made that I speak of it; you would not have an engine for a small number?—The engine is used for a variety of other purposes, for driving lathes, for cutting bones, and the like of that.

555. Still it is desirable for the purpose of keeping up artificial respiration?—I have seen it, and I know that they do put it to that use.

556. Is it not true that with regard to those experiments for recording arterial circulation chloroform interferes so much with the action of the heart that it cannot be used?—I do not think chloroform does so much as chloral. Besides opium may be used.

557. I am told that urari is used as the great agent in these experiments, simply because it does not interfere to the same extent with the arterial pressure; and if so all those experiments, which are sometimes very prolonged (in this book, for instance, there is one of that kind which it is said may be kept up for many hours), would be experiments of a very painful character?—Well, if there was no anæsthetic they would of course.

558. You were speaking of the scientific end being the true end. I am not wishing to put you into a dilemma at all; but how would you restrict it so that it should not be extended to man. I am not speaking of ordinary men who have their freedom, but say prisoners who are liable to a certain amount of suffering for their crimes. Surely if the scientific end is the great end, those experiments would be justfiable on human beings?—But it is not necessary to use human beings.

559. It would be very much more instructive, would it not, than to make them on animals?—I do not know that.

560. You would admit that there are a great many functions in the human being which are very different from those in animals?—Of course the great point about human beings is that they could tell you their feelings. But really that speculation never entered into my head; that has been done we are told.

561. I was referring to your very strong way of putting it; you said that physiology is not a reaping machine but a plough; that we must not look to the immediate end but to the great methods that it opens up to us. Now with respect to that, surely it is quite true that that method as applied to human beings would be infinitely more powerful than as applied to animals?—I do not think we have any right to apply it to human beings, unless they are willing.

562. Even if they had incurred the penalties of the law, do you think we cannot apply it to them?—No, because the penalty of the law may be death, but not the pain that would be incurred previously; and in other cases where the penalty of the law was not death there might be a danger of it.

563. Might you not submit those who had incurred minor penalties to experiments not endangering life, and those who had incurred the greater penalty to the more dangerous experiments?—I never thought of that; if I mistake not the first experiments of innoculation of the small-pox were made on criminals.

564. Is not that a precedent that might be repeated with great advantage to science?—I have not thought out that question.

565. (*Chairman.*) You are not prepared to recommend it to the Crown and Parliament?—Certainly not.

566. (*Mr. Hutton.*) But you would admit that the way in which you put it, that the scientific object is the great object, would lead to that logically?—I do not think it is very likely to lead to it actually.

567. I do not mean practically, but that it would suggest that?—It is alleged that in antiquity they did perform dissections upon living men. It is said that Herophilus, of Alexandria did, and he was denounced by Tertullian, one of the fathers of the church, as

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having made vivisections on human beings. But I do not know how far that is true, but yours is a speculative question which I really am not prepared to discuss, but I think it never will come up practically at all.

568. What suggested it to me was that you were saying that worari would be a good subject of experiments. It struck me that no experiment could be made with worari except on a being who could tell you what the result was, and that suggested that you should try it upon men; I think the logic would lead you to try it on a prisoner?—The purpose of trying it on man of course would be to ascertain whether it was really an anæsthetic, in order that you might judge how far you would relieve animals from pain in operations by the use of it; I do not see any other purpose for it.

569. It would also be very useful to know if you could relieve men from pain by the use of it?—We have other and safer means.

570. This is, I understood, to affect the heart very little, is it not?—Yes; but it may stop respiration.

571. And for various purposes therefore it might be a more useful means than those now used?—It was proposed as a remedy in tetanus, but I do not know that it ever was employed; I am not sure that it has not been employed on the human subject with that view.

572. As to those experiments on starving animals to death, was there any scientific end possible in them, to produce any scientific result of any kind?—Yes, they showed, for instance, the effect of the want of food upon the functions, upon the function of the production of heat, upon the function of respiration, and upon the order in which the tissues are consumed, the immediate cause of death, and so on.

573. They were valuable experiments, were they?—Well they were experiments that might be worth making once perhaps, but they were very severe experiments.

574. Which you would not condemn?—I should condemn the repetition of them; I should not have undertaken them; that is all I can say.

575. Now those experiments that you were mentioning as valuable at the hospitals were experiments on the gall bladder?—Those experiments that I referred to were begun by Schwann, of Louvain, a very distinguished physiologist in other branches of physiology than that, and they have been repeated by others; but I do not think that long continued experiments of that character are desirable. An experiment of that kind was made in Edinburgh, for the purpose of ascertaining the effect of mercury upon the flow of bile, under Doctor Bennett, the former professor of physiology in Edinburgh; and these were long and tedious experiments no doubt, and not very easily carried out.

576. Then even for a great scientific end you would not justify any lingering painful experiment?—I think for a great scientific end I should justify it, provided you could show that it was a great scientific end, and provided you could show that the experiment would probably give a decisive result.

577. But supposing that the experiment leads to more questions, as it usually does, than it solves. I thought you had been referring to some experiments at St. Bartholomew's Hospital, by Dr. Legg, on 16 cats, which were put to a lingering death, some of them lasting three weeks, in order to determine what the function of the liver was?—It is not right to speak of experiments which one has not read in detail; but I should think it quite possible that these experiments were not advisable.

578. Then your general position is that you would be very unwilling to have any lingering painful experiments?—Yes.

579. And that you would have them restricted, even though they might possibly lead to a scientific end, if the possible result was not clearly anticipated?—I think so.

580. But do you think that young scientific men, with the zeal of science upon them, would be likely to do what was undesirable without somebody to moderate their zeal, and to be responsible to?—I think they would. I think that if anyone had asked me my opinion upon conducting some of these experiments, I should have said that it was better not to do it.

581. Precisely; but you were not asked; and the zeal of the inquirer was too much for him. Do not you think that that suggests that some restrictive legislation is requisite, that some one should be put over these persons, or some inspector should be appointed to whom they would be responsible?—Who is competent to inspect and say what is a scientific result or not? You must trust to the persons that you permit to make these experiments. You must take care beforehand that they are persons on whose judgment and discretion you can rely.

582. But you might have an extremely scientific and even a brilliant man on whose judgment and discretion in that kind of thing you could not, I take it?—Yes; but that might happen in almost any relation of life.

583. And you would not be prepared to restrain such a man?—If you come to the question of legislation about it I think that such a person might be authorised or licensed to make these experiments; and when the experiments were painful and not done under anæsthetics that he should keep a record of them, and be prepared to report what he had done, and then the authority granting the license might from their own knowledge and by consultation restrain him from prosecuting objectionable experiments.

584. I am very glad to get that admission from you; but when you said that the greatest possible use of anæsthetics was always made by the physiologists it struck me that this handbook would hardly bear you out in that?—I cannot answer for the handbook; but I should be inclined to say that, so far as I know, in the institution which I am connected with the experiments are made under anæsthetics, or where the animal is in a state of insensibility in some way or other, and I do believe it is the case at other places; and I believe, in short, that it will be found upon inquiry that there is very little of painful experiment really practised in this country.

585. Do not you think that familiarity with these experiments breeds a certain contempt of the objection altogether, the humanitarian objection; that it creates a certain kind of scorn for the "milk sops" who feel so much for the lower animals; that there is a certain business feeling in favour of not interfering too much with the methods of science, which arises out of it?—Of course interference which would trammel scientific inquiry would be an evil; in fact it would be an act of barbarism committed at the instigation of certain people in order to prevent what they conceive to be barbarity. But I do not know that the feeling to which you refer springs out of the practice of experimenting. I think that it might arise quite independently of that. If anyone has cognizance of the nature of scientific experiments on animals, although he is not given to practise them, he might disapprove of what he would consider the unreasonable zeal of some who are commonly called humanitarians for interference. I think that may very well be; but I do not think that the practice of experiments on living animals really has the effect of blunting the feelings of those who are engaged in them or hardening their nature.

586. Not generally: but in regard to the particular experiments; they regard them as a necessary evil I suppose?—I should think if you look at the experimenters of the past you can see plainly that that notion of the moral effect of experiments made on animals is not at all justified.

587. (*Chairman.*) You have told us that in your youth you declined to attend the experiments of Mr. Magendie, although you were at that time ardent in the pursuit of science?—Unquestionably.

588. And you have told us that you reprobate every needless infliction of pain upon any animal?—Yes.

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589. And that you believe the eminent men with whom you are chiefly associated are of the same sentiment as yourself?—Yes.

590. That the great majority of the experiments which you consider necessary, can be performed without any suffering to the animals if anæsthetics are properly employed?—That they ought to be so in every case.

591. But that where experiments are to be tried, which must inflict more or less pain upon animals, you think it should be under some restrictions, and that you think competent persons only should be licensed by some authority; that they should keep a record of any experiments which they perform, and that the authority should have the means of withdrawing the license if any abuse can be shown to have occurred?—Yes. I approve of all that, always under this reservation: that I doubt very much if there are such abuses of vivisection in this country as require legislative interference in order to check them, and that perhaps it might be found after due deliberation that the better mode would be to trust to the influence of public opinion and the example of the leading physiologists of the time to prevent excesses.

592. But if it should be proved that there is any tendency to such abuses in this country, as you have told us exist in your own knowledge abroad, you would think it necessary that there should be a restraint to prevent any scandal of that kind?—Well, I think so. Perhaps you would allow me to ask the favour of this extract being read, it is the opinion of one of the greatest physiologists of all time, and who is the author of the grandest work on physiology perhaps that ever appeared, and who was a great vivisector, I mean Haller: "But it is not sufficient to "make the dissections of the dead bodies of animals. "It is necessary to incise them in the living state. "There is no action in the dead body; all movement "must be studied in the living animal, and the whole "of physiology turns on the motions, external and "internal, of the living body. Hence no progress "can be made in investigating the circulation of the "blood and its more recondite movements, or the "respiration, or the growth of the body and the bones, "the course of the chyle, or the motion of the intestines, "without the sacrifice of living animals. A single "experiment will sometimes refute the laborious "speculation of years. *Hæc crudelitas ad veram "physiologiam plus contulit, quam omnes fere aliæ "artes quarum conspirante opera nostra scientia "convaluit.*"

593. But that maxim of Haller's which you have just read to us is to be understood by us, subject to all the limitations which in your valuable evidence you have been so good as to lay before us?—Certainly, it merely goes to show the importance, the extreme value and indeed indispensable need of experiments on living animals for the cultivation and advance of physiology.

594. (*Mr. Erichsen.*) I was going to ask whether physiological experiments of an important character have not been made upon men, such for example as the experiments of Dr. Stark upon himself, and those of Dr. Parkes with regard to the administration of alcohol and matters connected with the weight of the body, and experiments such as I have made myself, in giving a person various medicinal agents to see how soon they appeared in the urine?—Yes.

595. The experiments with regard to anæsthetics which led to the discovery of the anæsthetic influence of chloroform by Sir James Simpson were made upon himself and his pupils or assistants and friends?—Yes.

596. And in surgical operations in hospital practice do we not practically make experiments on the living subject, that is to say, we have to perform operations there which have never been performed before; for instance, when John Hunter ligatured an artery for aneurisun he had never performed such an experiment, nor could he have done it on animals, aneurism not occuring in them?—That is so, and I may mention Spallanzani's experiments on digestion made on himself.

The witness withdrew.

Adjourned to Thursday next at 12 o'clock.

Thursday, 8th July 1875.

PRESENT:

THE RIGHT HON. VISCOUNT CARDWELL, IN THE CHAIR.

The Right Hon. LORD WINMARLEIGH.
The Right Hon. W. E. FORSTER.
The Right Hon. Sir JOHN KARSLAKE.
JOHN ERIC ERICHSEN, Esq.
RICHARD HOLT HUTTON, Esq.
Mr. N. BAKER, Secretary.

Professor GEORGE M. HUMPHRY called in and examined.

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596. (*Chairman.*) I think you are Professor of Anatomy in the University of Cambridge?—Yes.

597. You are also surgeon to Addenbrooke's Hospital, are you not?—Yes.

598. Has your attention been drawn to the subject into which this Commission is appointed to inquire?—Yes.

599. Will you have the kindness to state your general view on the subject?—My general view is that forasmuch as a large part of the animal kingdom lives and maintains its perfection by the death of other animals, which is necessarily attended with more or less of pain, it is quite a justifiable thing for man to inflict death and a certain amount of pain on other animals when there is a reasonable prospect of his condition being benefited by it.

600. Do you qualify that by the suggestion that any such pain should always be as closely limited as the circumstances of the case will admit?—Certainly.

601. Do you consider that experiments on living animals fall within the description that you have given?—Certainly, provided it can be shown that they are likely to promote the benefit of man.

602. Can you quote any cases which would illustrate that view of the general principle—I mean cases to show that experiments such as you would advocate are of essential importance to the welfare of the human race?—I think they are of essential importance to the promotion and knowledge of physiology, forasmuch as physiology is the study of processes going on, *i.e.*, of organic functions. Now we can attain a knowledge of anatomy and the structure of the parts by examining them after death, but we cannot attain a certain knowledge of the vital relations of those parts to one another, and their bearing on one another, except while the functions are going on; that is to say, during life.

603. Now has the discovery of anæsthetics ma-

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terially affected this question?—Unquestionably by removing a great objection to such experiments. We never could have attained to the knowledge of the action of the nervous system on the heart, and still less of that which is very important, that is to say, the action of the nervous system on the blood vessels,—an acquaintance with which really lies at the basis of our knowledge of the processes of inflammation,—without experiments on living animals; indeed, we never could have thought or even dreamt of the effects of the nerves on the vessels without actually witnessing experiments instituted with reference to this on animals in the living state.

604. Has the discovery of anæsthetics made any difference in your views on this subject?—The discovery of anæsthetics has made an enormous difference, in fact it has specially facilitated the thing; it has increased the number of experiments, and it has increased the amount of knowledge derived from them. A very great number, in fact nearly all physiological experiments can be performed as well under anæsthesia as without it. So far as my knowledge goes, they almost invariably are so; and really a physiological vivisection experiment is attended with no more pain and often with less pain than the killing of animals which is daily going on for the purposes of food.

605. Are there in your opinion very few experiments in which the pain cannot be removed by the proper application of anæsthetics?—Very few, and they would be those cases in which consciousness was a necessary feature; cases in which consciousness made a great difference in the physiological experiment, and those instances are really very few.

606. I suppose you would say that they ought therefore under any point of view to be very seldom performed?—They need very seldom to be performed.

607. I suppose if any such experiments were performed by any but the most competent people, that would meet with your reprehension?—Entirely, for this reason, that they would be valueless; it requires considerable knowledge on the subject to be able to form correct views from vivisectional experiments at all, and especially from vivisectional experiments under consciousness, for they are very much more difficult.

608. Looking at the object which we may be said specially to have in view, namely, the prevention of suffering to animals, such an incompetent person in making such experiments might, besides their leading to no practical results, inflict a great amount of unnecessary suffering, might he not?—Yes, a very much greater amount of unnecessary suffering than a competent person.

609. Now do you think that eminent scientific men in this country may be expected to take a very decided view in favour of treating animals with humanity?—I think unquestionably so. Many of them are very fond of animals.

610. And that they would be likely therefore to give their support to any reasonable measures which the Crown and Parliament might think necessary for such a purpose?—No doubt. I think they have already shown their willingness to do so. I apprehend that to them inflicting pain on an animal would be quite as repugnant as to any other persons.

611. You have spoken hitherto only of physiological experiments. Have you any observations to make on pathological experiments?—I think pathological experiments are and will continue to be very important and very useful for the welfare of mankind, in giving us a better knowledge of disease and the means of treating it; indeed, I cannot help thinking that the scientific knowledge of medicine is to be based to a very considerable extent on experiments upon animals.

612. There is a great deal of pathological observation obtained from seeing patients in hospital?—Certainly, but we cannot observe pathological processes in such patients, or only to a very slight extent. Take for instance the most common of all morbid processes, namely inflammation, we only learn what it is, and how it is to be induced, and what agents will affect it, by observations on the living animal.

613. Are those observations necessarily very painful?—Not necessarily so; some of them are likely to be attended with suffering.

614. But I should suppose that you would apply to pathological experiments the same limitation that you apply to physiological experiments, namely, that in order to be of any use they must be in the hands of very superior persons?—I would say that that limitation would apply even more strongly, because the pathological processes are more difficult to observe correctly than the physiological processes.

615. Then the instituting of experiments on living animals for the purpose of watching a pathological process by an uninstructed or comparatively uninstructed person, would result in a purposeless waste of animal suffering?—Quite so, and it would be very reprehensible. I may here state perhaps, that many of those pathological experiments cannot be made under anæsthesia. I think for the welfare of mankind and the advantage of medical service it will be necessary to induce diseases in animals in order that we may learn the nature and the processes of those diseases and the means of correcting them. For instance, we never could otherwise have known that in inflammation the globules of the blood could pass through the blood vessels into the tissues and become diseased, and find their way back again, and so enter the blood. The blood globules lie within the vessels and are confined by the walls of the vessels, which are membranes. Formerly we had no idea that the blood globules could find their way through the walls of the vessels into the tissues, and that the diseased globules in the diseased subject could find their way back again into the blood vessels. That could never have been discovered except by observations of the actual facts. It is a most important fact, lying at the root probably of a vast amount of disease. By experiments on living animals and by diseases instituted in them we are, I believe, beginning to get an idea of the nature of tubercle, and I feel it to be within the bounds of hope that one day we may learn the nature and possibly the means of preventing that most awful of all maladies, namely, cancer. I think we may look forward to that, and if it is to be found, it is more likely that we shall find it through experiments on animals than in any other way.

616. Now are those pathological experiments to which you point necessarily very painful?—I do not imagine that disease in an animal is so painful as it is in a man; still it must be attended with more or less distress.

617. But the particular distress to which you are pointing now, is not the distress of the knife?—No.

618. It is the distress of the malady which has been superinduced?—Quite so. It is quite inconceivable that anyone should enter on an investigation of that kind without very serious views with regard to it, and with great forethought and definite ideas with reference to the result to be obtained or sought.

619. So far as your own personal experience has gone, I suppose you would say with some confidence that a reckless, inhuman, and indifferent way of conducting experiments, at the cost of much suffering to the lower animals, is not to be imputed to any eminent men of this country?—I am quite unaware of its existence in this country at all. My belief is that the public statements with regard to that are very greatly exaggerated. I do not believe that those whose work it is to perform investigations of this kind would be at all more thoughtless or inconsiderate about it than other persons.

620. Those with whom you have been acquainted are some of the most eminent of the scientific men of this country, are they not?—Yes. I am not aware of vivisection being performed by other than competent men.

621. If vivisection were performed by persons who were not very competent, and who were not considerate, and who did not care to mitigate the suffering inflicted on the animal to the utmost of their power, I think you have said nobody would be so much in-

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clined to condemn it as the eminent men with whose practice and experiments you have been acquainted?—I think none would be so much inclined to condemn it as those gentlemen.

622. You would agree that anything of that kind is an abuse that ought to be redressed?—Certainly. So far as I know the experimenters do not become hardened by it. I may mention for my own part that I was very fond of shooting, and one of the things which ultimately led to my giving it up altogether, was the pain consequent on it to animals, the lingering suffering of the animals after they were wounded.

623. Have you any observations to make on the educational part of the subject?—I think it is rarely necessary in the education of the student to practice vivisection.

624. I suppose it might always be done under anæsthesis, or almost always?—I should think it might always be done.

625. And therefore to do it for the purpose of instruction otherwise than under anæsthesis would be an abuse?—Yes.

626. (*Lord Winmarleigh.*) Are there no exceptions to that rule?—There may be exceptions.

627. I mean in those cases which you have just described?—Referring to physiological processes, there is this point to be considered, that it is not easy to draw the limits of what we mean by the education of a student, for the teacher would be likely to associate his better students with him in the work of investigation and observation and reflection, which constitutes after all the best part of the education of a student.

628. (*Chairman.*) I apprehend you would say that any such investigation, if carried on at all, ought to be under the strict control of a responsible professor?—Quite so.

629. And that any latitude which permitted students to have any individual share in such matters, otherwise than under the immediate control of a professor, would also be an abuse?—Quite so.

630. Then the upshot of your opinion is, I think, that some experiments on living animals are necessary for the purpose of the development of the healing art?—Yes.

631. And that anæsthesis enables most of those which are physiological—indeed nearly all of them—to be performed without consciousness in the animal?—Yes, quite so.

632. And that in pathological experiments, the vivisectional part, if there be any, will fall under the same category?—Quite so.

633. And that the distress which would be caused to the animal would be distress of the same nature as the distress which falls on a human person when subjected to that disease?—Quite so. The only object of such an experiment should be the prevention of that human suffering.

634. And that the control of any such experiments ought to be in the hands of the most competent persons, and that any practice of anything of the kind except by such persons is a reprehensible abuse?—Yes, certainly.

635. So far as your knowledge of the principal and the most eminent scientific men of the country is concerned, you think they might be expected to co-operate in any measure which might be designed for the purpose of producing such a result?—Quite so. I may add this:—Pathological experiments I think, become more and more necessary as civilization advances. Civilization is the great engenderer of disease, and unless the healing art is made to advance in proportion, there will be as the result of civilization a distinct degeneration of man, physical and moral. I think there is no doubt of that. And therefore it is necessary to take every means possible most earnestly and anxiously to understand the nature of disease and to prevent it. It is often said that those experiments ought not to be repeated. Now we all know how very difficult it is to ascertain a fact, and those experiments on living animals are associated with observations of very complicated facts; they are often very difficult observations, and therefore they have to to be repeated and confirmed many times before the fact is really established. I might mention as an illustration of the difficulty of observation of facts, that I remember the Astronomer Royal stating that in the simple observation of an eclipse, he himself and his three assistants observed quite differently, though in that case the facts were going on before their eyes. The correct observation of the facts in such a process as vivisection is much more difficult.

636. We were told the other day that the experiments of M. Majendie established the correctness of Sir Charles Bell's views about the nerves, and that to have repeated that experiment would have been an abuse. Would you differ from that opinion?—I would not like to put the fact in quite so blank a form as that, forasmuch as there have been complications even in that very experiment,—it has not been quite so simple a matter as may be supposed,—nor have the inferences drawn from it. I do not think that the so called establishment of the facts by two observers would even in that instance have been sufficient to render further experiments unnecessary. That, however, was one of the most simple cases.

637. (*Lord Winmarleigh.*) Did I understand you to say that you did not think that these practises of vivisection were going on to any great extent in this country?—I said I did not think they were going on in an improper manner in this country.

638. Do you think that the practice is carried on to any extent in other places than in the great schools of physiology?—I am not aware of it.

639. Do you think in the course of your practice that if that were the case it would naturally come to your knowledge?—I think I should have known it to some extent.

640. Can you speak to any instance in any town or large society in England in which the practice of vivisection is carried on otherwise than under the control of some great school?—No, I am not aware of any such case.

641. You said you believed that the animal creation did not suffer so acutely as the human race?—Just so.

642. On what do you found that observation?—Their nervous system is not so large, it does not bear so large a proportion to their bodies, and we have reason to think that it is not so sensitive.

643. In what way is the nervous system not so large in animals as it is in the human race?—The nerves and the brain are not nearly so large in animals, in proportion to their bodies, as they are in men.

644. Is not it possible that a very small nerve may carry the same amount of pain as a large one?—It is not probable. It is probable that where the nervous system is largest, the sensibility would be greatest.

645. Take diseases of the face; the nerves of the face are the smallest in the body, are they not?—They are very numerous.

646. Take tic-doloreux, for instance, is not that a most acute pain?—The nerves in the face bear a greater proportion to the face than the nerves in any other part of the body bear to that other part.

647. And they give about the most acute pain that we know?—Yes, most acute pain.

648. And those are the smallest nerves, are they not?—All the nerves are about the same size.

649. If you took a worm and put it on a hook, the violent contortions of the worm would indicate to a person that was ignorant the existence of pain, would it not?—But not necessarily correctly; for there may be violent contortions and no suffering whatever; that we learn from man.

650. Could you give an instance of that?—A man may be under chloroform to such an extent as to suffer no pain, but he may exhibit violent contortions. The connection of the part with the brain might be severed altogether, so that there could be no consciousness of pain, and yet strong muscular action may be excited.

651. Take a man in a healthy state, could you give an instance in which he would be subject to great

contortions without suffering pain, supposing him not to be under anæsthesia?—In epilepsy there are violent contortions.

652. And no pain?—None; and yet there is biting of the tongue, and tearing of the muscles; it is an important point to observe that contortion is by no means a certain indication of suffering.

653. (*Chairman.*) Without affecting to be competent to ask surgical questions, I will ask this. If the nerves are divided into motor nerves and sensory nerves, then where the sensory nerve has perished, or where it has been cut off, there may be no pain and yet motion may remain, I suppose?—Yes, certainly.

654. But in an ordinary state of the body, where there is no such interruption of either class of nerves, violent motion would be *primâ facie* evidence of violent sensation, would it not?—Not necessarily, but usually it would be so.

655. That would be the presumption?—Yes, usually. Might I add here that a very vast amount of our pain is in anticipation and retrospect, which is entirely absent in animals.

656. (*Lord Winmarleigh.*) You state that you do not think that there is any very great cruelty going on at the present time under physiological practice in this country. I have in my hand an extract from (I presume) a newspaper called "The Doctor," dated June 1st, 1874, and under the head "Second experiment" I find the following: "Press your finger under " a frog's mouth until the eyeball protrudes. Then " pull down his nictitating membrane with a pair of " forceps. Next, scrape off with a sharp knife the " three layers of anterior epithelium. Now let him " rest ten minutes, you may then observe his actions; " he pokes his head down between his fore legs, " jumps, or turns over, wriggles, and otherwise acts " in a strange manner. Having watched this, take " him again and push out the eyeball once more. " Again hold down the protecting membrane, and " rub a stick of solid lunar caustic all over the eye " until the aqueous humour of the anterior chamber " shows a precipitate. Now release your frog, and " his actions will be similar to those previously " watched, but more intense; in fact, he plays such " fantastic tricks as few could look upon without that " blunting of sensibility on which so many have " dilated; it is necessary for him to be left for ten " minutes, after which his head is cut off with a pair " of scissors, and so his suffering ends. The object " is to demonstrate the structure of the cornea by " staining the intercellular substance." Do you believe that that sort of experiment is carried on in this country?—I do not understand that as a physiological experiment at all, it is a meaningless experiment.

657. You do not believe that that illustrates anything in physiology?—No.

658. (*Mr. Hutton.*) Is there not the same experiment described in a well-known handbook that has just been published?—I suppose it relates to inflammation of the cornea.

659. The inflammation being produced artificially by the nitrate of silver, is not that so?—I have glanced at the handbook and I do not think this account at all indicates that it is not done under anæsthesia. The fact of the animal being under anæsthesia is usually omitted in this book, it is taken for granted.

660. (*Lord Winmarleigh.*) Is there anything in the case which I have just put to you which pre-supposes that anæsthesia was not used?—The account would rather pre-suppose that anæsthesia was not used in that instance, the word suffering is used there.

661. From your previous answers I gather that you would say that this was a most improper experiment to make under the circumstances here related?—I think that might be done under anæsthesia.

662. But if it is not so, do you think that the experiments would be recognised by the great body of medical practitioners in this country, or the great physiologists of the day, without such an application as would diminish pain to the greatest possible extent?—I should think not. I have not considered the experiment very carefully, or the bearing of it. It seems to me very extraordinary as being performed by physiologists. I apprehend the object is to ascertain the staining of the intercellular tissue.

663. Can that be made useful for the benefit of the human race?—Yes, but the style in which this is written is I think rather peculiar: "You may then " observe his actions; he pokes his head down be- " tween his fore legs, jumps, or turns over, wriggles, " and otherwise acts in a strange manner." It would seem that that was really written for the mere purpose of exciting the feelings.

664. (*Mr. Hutton.*) Perhaps that is what might be called a sensational account?—Yes, I should doubt the veracity of it.

665. (*Lord Winmarleigh.*) You having read that, do you in fact believe that is a true statement of what has occurred?—No, I do not; I doubt it. I should imagine it to be a sensational description.

666. (*Mr. Hutton.*) There would be no doubt that an experiment of that kind would give the frog considerable pain if made without anæsthesia, I suppose?—It would give it pain, but pain in a frog is not to be measured by pain in ourselves. Still I think no pain should be given when it is unnecessary.

667. (*Lord Winmarleigh.*) You implied, I think, that pathological experiments were sometimes as painful as physiological experiments; did I understand you to say that the anæsthetic practice is as applicable to pathology as it is to physiology?—I think not, because the process may have to be observed during some days.

668. (*Mr. Hutton.*) Or weeks?—Yes, or weeks.

669. (*Lord Winmarleigh.*) You do not think that the pain of pathological experiments can be alleviated in the same manner?—Not to the same extent.

670. Would you propose to make any distinction in legislative language for this difference between pathological and physiological experiments?—I would indeed.

671. What would be the nature of the distinction which you would propose to make?—Supposing there were to be any legislation to the effect that all experiments are to be made under anæsthesia, a very great exception should be made in instances in which the process is to be extended and watched.

672. Would you make any difference in legislation with regard to the practice of pathology in these respects, so as to place a strict limit on it, or on the practice of physiology?—I should have thought that the limitation that it was to be done under responsible and competent persons would be sufficient. I confess that I have far more confidence in the good feeling of those who do such things than the public may have. I think that by limitations and restrictions you, to some extent, take away responsibility and mar good feeling.

673. (*Chairman.*) You think that if the whole subject where placed on the responsibility of one of those eminent persons to whom you have previously referred, such an experiment as you have just had submitted to you (whether it was ever performed or not, is for the moment indifferent) never would be performed at all?—Certainly not.

674. (*Sir John Karslake.*) With regard to that experiment, as far as I understand it, it was merely to show that an established fact existed which had been ascertained before, is that so according to your reading of it?—It did not appear so there, I think, but I am not quite sure.

675. I think I understood you to express an opinion that experiments of this description have increased since the use of anæsthetics became known?—Certainly they have done so, and for two reasons. First, because there are a greater number of investigators in physiology; and, secondly, the fact of experiments being possible under anæsthesia renders physiologists so much less reluctant to undertake them.

676. The latter reason seems to indicate that the medical profession generally did hesitate considerably

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to inflict pain in these cases, before the use of anæsthetics was discovered?—Certainly. I cannot conceive there being any difference of opinion about that. I cannot believe that any persons of education would voluntarily inflict pain on animals in this country, without some good reason and some great ends to be attained.

677. Do you think that those great results have been attained in consequence of those researches under the use of anæsthetics?—I think very great results are being attained, and to some extent have been attained.

678. From what date or for how many years back should you consider that the use of anæsthetics has been practically known. I do not mean as to when it was first discovered, but practically used for this purpose of vivisection?—As long as it has been known to man, that is to say about 28 years. It was about the year 1845.

679. I suppose we may take it that from the year 1845, or a few years afterwards, the increase which you think has taken place in those experiments may be dated?—Yes, certainly.

680. Though you say that when a fact has been established an investigation in this way should not go on further, is it not the case that frequently a large number of experiments have to be made in order even to partially establish a particular thing?—It is very difficult to know when a fact has been established.

681. Take a case in which you perform some operation and watch the results, and then only ascertain the fact that a particular thing has occurred in 20 cases out of 22. Is not that a common thing?—Yes; that is frequently the case.

682. You do not find the results uniform, but you find a large average in favour of a particular theory?—Yes; that is the case sometimes.

683. Now as to the use of vivisection, though we have spoken of that with regard more particularly to the benefit of human beings, you would not exclude, I suppose, the use of those experiments for the purpose of saving the lives of animals?—Certainly not.

684. You think it justifiable in those cases, subject to the same reserve as in other cases?—Yes, certainly; it is part of the law of nature that animals as well as man benefit by the death as well as the pain of others.

685. (*Mr. Erichsen.*) Referring to recent progress in physiology, made largely through these experiments, and also in pathology, could you form any idea what our present position of medical science would be if all that has been attained by vivisection were blotted out of human knowledge?—Our knowledge would be in a state of comparative barbarism. Supposing civilization to go on and vivisection not to go on, our knowledge of physiology and pathology would remain in that state of comparative barbarism.

686. It would not be sufficient for the progress of medicine or medical science that physicians should trust solely to clinical observation, or to the pathological knowledge that can be derived from observation in the deadhouse?—Certainly not; that would leave us entirely ignorant of the processes.

687. The vital processes?—Yes; the morbid process, on a knowledge of which, after all, scientific medicine must be based.

688. In the deadhouse one sees the ultimate result, but in the physiological laboratory one studies the process which leads to that result?—Yes.

689. Is there a physiological laboratory at Cambridge?—There is.

690. Are you connected with it?—Not directly.

691. But you know the proceedings there, I suppose?—Yes.

692. In that physiological laboratory are there a great many proceedings carried on that have no relation whatever to vivisection?—Of course.

693. And the students pursue their studies there?—Yes.

694. And are they allowed to vivisect or perform cutting operations on living animals?—Never, except when the animals are under anæsthesia; so far as I know it is done only by the senior students who have been deputed to do it by the lecturer on physiology, and for the purpose of ascertaining some definite point.

695. But there are many students who are not exactly *in statu pupillari*?—Yes.

696. Men advanced in years, but still students?—Yes; I should say it was impossible to define the word student arbitrarily.

697. With regard to the great advantages that vivisection has conferred on physiology, pathology, and the practice of medicine, should we not consider its bearing on medico-legal investigations. Taking experiments on animals by vivisection, do you consider them justifiable with regard to such things as the determination of the action of poisons?—Yes, certainly.

698. And the exhibition of such action?—Yes; I consider that comes rather under the division of pathology; that is to say, the effect of agents on the body.

699. All medicinal agents, for instance?—Yes; that is most important. The old notion of the effect of mercury on the liver, for instance, is disproved by investigations and experiments on living animals lately carried on in Edinburgh.

700. In those experiments, of course anæsthetics cannot often be taken?—No; not in many of them.

701. Many of those experiments are not painful, I believe?—That is so; many of them are not painful.

702. You made some reference to the comparative extent of the nervous system in different animals. I will ask you whether you have formed any opinion with regard to the difference of sensibility between warm and cold blooded animals?—Warm-blooded animals are the more sensitive.

703. You would find a difference between the sensibility of a frog and a dog?—Undoubtedly.

704. Among the warm-blooded animals, do you think it fair to compare the sensibility of the lower and higher animals, or at all events do you think that there is an equality?—The sensibility of man is greater than that of the lower animals. The question of anticipation and retrospection which I have already mentioned is also a very important point.

705. We see that, do we not, in operating on infants for harelip, for instance?—Yes.

706. It is a very severe operation, is it not?—Yes; it is less important to give chloroform to infants than to adults on that ground; there is not the dread of it, and there is not the recollection.

707. In many of those cutting experiments on animals, say, for instance, such experiments as are rendered necessary for the exposure of the carotid artery, does the animal evince much suffering?—An animal fixed down and rendered unable to move evinces very little suffering. I have often wondered at it, not being able to satisfy myself to what extent they were suffering; that was in the days when I had had some experience in vivisection before chloroform was used. Indeed, I may say with regard to the case which has attracted so much attention, namely, the case of the dogs at Norwich, I was present during a part of that experiment, and I could not tell whether the animal was suffering or not.

708. That was the dog to which absinthe was administered, was it not?—Yes, it did not appear to be suffering much.

709. (*Chairman.*) When a horse is struck with a sharp whip you can usually discern whether he suffers or not, can you not?—Yes.

710. Suppose the horse's limbs were fastened down, would you still be able to discern it?—I cannot say about the whip; I have only seen experiments with the knife, and it is remarkable how still the subjects often are.

711. Would you think it a reasonable presumption

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about a horse who was struck with a whip that we should draw the inference that he suffered a great deal of pain when he was able to move, but that if the same blow was given when he was tied down and could not move we should infer that he did not suffer much pain?—I think we cannot draw that inference. The question with me was to what extent the animals suffered after the skin was divided and in the subsequent process.

712. In the human being the external skin is the great seat of sensibility, is it not?—It is usually said to be the most sensitive, but that does not quite correspond with my own observation certainly.

713. In the days when flogging was a very customary punishment, was it not believed that the agony of the sufferer was chiefly while the skin was being perforated by the lash, and not afterwards, when the lash in a very revolting manner to the spectator ploughed into the flesh?—I do not know that.

714. Should you doubt it?—I should doubt it.

715. (*Sir John Karslake.*) The sensitiveness of the skin is very different in different parts of the body, is it not?—Yes.

716. (*Mr. Hutton.*) As I believe you were present at that experiment at Norwich which has been referred to, I will ask you a question. Do you consider that that experiment was justifiable and that the scientific object of it was adequate or not?—I was merely a bystander at part of the experiment. I heard that something was going on and went into the room, but I very soon had to leave it. I merely saw an animal on the table and some one doing something to it; what they were doing I did not know at all, and I was unable to remain and ascertain.

717. But still you understood, did you not, that the scientific object of that experiment was to try the effect of injecting absinthe into the veins or arteries of the animal?—Yes, I learned that subsequently.

718. Was that an experiment which you would consider of great scientific value?—Not of great scientific value, but of some scientific value.

719. Would not the effect of absinthe in the veins be very different from the effect of absinthe taken into the stomach?—I should apprehend not very different.

720. But it might be different?—It might be different.

721. Is there any reason to suppose that because absinthe taken into the veins would produce epilepsy, absinthe taken into the stomach would do the same?—It would not necessarily follow; but many substances produce the same or a similar effect, whether introduced into the veins or into the stomach.

722. The result of such experiments would at all events be uncertain, I suppose?—I think the fact that absinthe injected into the veins produces epilepsy is a fact of some value and importance.

723. But not as leading up to the effects of absinthe taken into the stomach?—It would have a relation to that.

724. I gather the general effect of your evidence to be, that on every account the number of these experiments must increase very rapidly in proportion as civilization which causes disease, and science which tries to mitigate disease, go on?—Yes, especially the pathological experiments.

725. I think you stated that the effect of the discovery of anæsthetics had very much diminished the pain of these experiments?—Yes, and in the physiological experiments especially.

726. Would you not rather say that it had increased the number of experiments that could be made without pain, than that it had decreased the number of experiments made with pain?—It has increased very much the number of experiments that can be performed without pain.

727. Is it not very difficult to administer anæsthetics to the frog and other lower animals?—No, I think not.

728. What is your view of the poison which is called curari or woorali. I find in Watt's Dictionary of Chemistry the following,—"This is used by the "Indians of South America for poisoning their arrows. "Curari acts chiefly on the motor nerves, the functions "of which it completely arrests, while the sensorial "nerves retain their activity." Would you agree with that view?—I have not experimented with the curari myself, and therefore I do not know, but it is quite possible for a poison to suspend motor action without suspending sensory action.

729. I find in this "Handbook for the Physiological Laboratory," it is stated that curari is largely used in experimenting on frogs. If that be so, and if the account I have just read of the action of the curari is correct, these frogs would not be under anæsthesia at all, would they?—If that is the case then they would not be.

730. Dr. Michael Foster, who is a colleague of yours, states that the number of these observations must very rapidly increase, and that very many of them may be made on frogs. In fact, I imagine that the number of experiments actually conducted on frogs is very much larger than on any other class of animals; is not that so?—There is a very considerable number of experiments on frogs.

731. I was told by a great physiologist, Dr. Klein, that there is great difficulty in administering true anæsthetics to frogs, and that many of them are killed in the process. Is that your opinion?—The lower animals are comparatively easily killed under the effects of anæsthetics.

732. The lower animals more than the higher?—The lower animals more than the higher; but nevertheless they may be put under anæsthesia.

733. But still, for the purposes of many experiments, it would be so difficult that it would hardly be thought worth while to try anæsthetics; is not that so?—I cannot say.

734. Your experience has not gone much in that direction, perhaps?—No.

735. I think I understood you to express an opinion that the line between demonstrative experiments and investigative experiments was rather a shadowy line, and one which was likely to grow fainter and fainter as medical education got better?—I think that no definite line can be drawn.

736. You would probably think that a thoroughly educated medical man ought to go through a physiological laboratory?—Yes; and that he ought to see not simply the demonstrative experiments, but possibly some of the painful experiments. Except in pathology, there would be very few painful experiments, which would be necessary for that purpose.

737. That is always supposing that the lower animals can have complete anæsthesia?—Yes.

738. At all events, I understand you to say that all medical students who are to be thoroughly educated should pass through a physiological laboratory?—Yes, certainly they should have a knowledge of the means by which certain information is acquired.

739. So that the distinction between demonstrative experiments and investigative experiments is not a very real distinction?—I think not.

740. Do you agree with Mr. Ray Lankester that the number of these experiments must increase very rapidly if the progress of science is to be kept up?—I think so; the pathological experiments especially.

741. And the pathological experiments are necessarily the most painful?—They involve a certain amount of distress to the animal.

742. They involve illness to the animal, in fact?—Yes, some of them; not all.

743. With regard to inflammation, you said that the discovery that the blood globules passed through the walls of the blood vessels could never have been made without experiments on living animals. Will you kindly tell me why that could not have been discovered by the examination of human beings who had died from inflammation?—You could not possibly tell that a given corpuscle had been in a blood vessel unless you had seen it pass through or had some knowledge that it had been previously in the blood

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vessel. Now, in an animal the blood corpuscle could be stained and be seen passing through the blood vessel, or could be found outside the blood vessel.

744. That involves not only pathological experiments, but physiological experiments on animals in a state of disease?—Yes.

745. It is not simply pathological experiments, but physiological experiments on animals artificially subjected to illness which are necessary for the purpose?—It need not be attended with very much pain; a little fluid might be thrown into the blood to colour the blood, and the investigation would soon be terminated.

746. Will you kindly turn to the account given at page 403, in the handbook to which I have already referred, and tell me whether you think the experiment there described is an experiment which ought to be exhibited to an ordinary class of students. "Recurrent sensibility is never witnessed in the frog; "it can only be shown in the higher animals, the cat "or dog being the best adapted for the purpose. The "arches of one or two of the vertebræ are carefully "sawn through or cut through with a bone forceps"?—That would be under anæsthesia.

747. But not the other part of it, I suppose?—"And the exposed roots are very carefully freed "from the connective tissue surrounding them; if "the animal be strong and have thoroughly recovered "from the chloroform and from the operation, the "irritation of the peripheral stumps of the anterior "roots causes not only contractions in the muscles "supplied by the nerves, but also movements in other "parts of the body, indicative of pain or of sensation. "On dividing the mixed trunk at some little distance "from the junction of the roots, the contractions of "the muscles supplied by the nerve cease, but the "general signs of pain or of sensation still remain." It would very rarely be necessary either to perform or exhibit that experiment.

748. Is not that given in the book as a demonstrative experiment which it is desirable to exhibit to a class in physiology?—I do not suppose that it is meant at all that those experiments are to be repeated; it is a direct experiment on the point of sensation, and sensation of course cannot be determined while the animal is unconscious.

749. Are there not a good many other classes of experiments which are very much interfered with by the use of anæsthetics; take, for instance, the experiments in the same book on arterial pressure, does not anæsthesia interfere with the action of the heart very much?—As far as I know such experiments are always carried on under anæsthesia, and I have seen a good many of them.

750. (*Chairman.*) And in your opinion, I suppose, they ought to be carried on under anæsthesia?—So far as I know, they ought to be.

751. You have been asked about the difference of sensibility between man and animals, does not sensibility differ very much in different individuals of the same species?—I think there can be no doubt about that with regard to man; I do not know about other animals. I should say that a blood-horse would suffer more than a cart-horse; but I should have thought there was not much difference between different cart-horses.

752. (*Sir John Karslake.*) Perhaps a toy terrier would suffer more than a bulldog?—About the same, I should think; but nobody can tell.

753. (*Chairman.*) If there were a restriction imposed, such as you, I think, have referred to in the earlier portion of your evidence, no students or pupils of your own would be allowed to perform any of those experiments which have been referred to, except with your own sanction?—Certainly not.

754. It would be under your own control and direction?—Just so; that is the case now. I do not know that in Cambridge any pupil ever does perform such experiments, except under control and direction.

755. In your belief then, the actual present assent of the profession to the proposition implied has already provided such a restriction?—Yes, quite so.

756. It would, therefore, not be an additional burden imposed upon them, if it were required now?—No, not at all.

757. (*Lord Winmarleigh.*) In the school of anatomy in which you practice, is there anyone besides yourself who would be authorised to perform those experiments?—Dr. Michael Foster has the physiological laboratory rather under his charge. He is specially appointed by Trinity College the prælector on physiology.

758. There is no one else who would be authorised to practise those experiments without your authority, or Mr. Foster's authority, is that so?—I do not know that I can limit it so strictly. There is a professor of medicine, and I could quite conceive that pathological experiments might be carried on under his direction.

759. In brief, would anybody who does not hold some public office be allowed to perform those experiments?—No.

760. (*Mr. Erichsen.*) I suppose we may take it that the students at Cambridge do not practise such experiments on their own responsibility and at their own option?—Certainly.

761. Such a thing is unknown?—Yes.

762. (*Chairman.*) And would be universally condemned, if known?—It would be universally condemned. I only wish that people knew exactly what does take place. I think there would not have been so much exaggeration and mis-statement if they knew.

763. (*Mr. Forster.*) Do you think that these experiments are practised in every school of anatomy?—I think not.

764. How many schools of physiology do you think there are of different classes in England?—Of schools of practical physiology I think there are very few.

765. A dozen?—I think not; I do not know half a dozen.

766. Do you suppose that these experiments of which we have been speaking are much practised by investigators in their own houses?—I think not much. I think such investigators in this country are not numerous. I would almost say, I am *afraid* they are not numerous. There is not very much physiological work done in this country.

767. Would you think that many students of physiology, or students of medicine or surgery, would in consequence of reading that handbook, which seems to recommend those experiments, try them by themselves?—I think not.

768. Do you know of such cases?—No, I do not know of any such cases. The amount of physiological investigation done in this country is not very great. I conduct a journal of anatomy and physiology, and that is the conclusion I have come to in that way.

769. I suppose you hear from men interested in physiology, medicine, and surgery various suggestions; they write to you, I suppose, as editor?—Yes, quite so. I know those who are at work at it.

770. Do you often get accounts of such experiments sent you?—Not very often; there are a few persons who make them, but it is always with regard to some especial point.

771. Can you say, as between the two classes of experiments, physiological experiments and pathological experiments by actual vivisection, which are the most generally practised?—Pathological experiments are very rare indeed; there are only one or two persons in the country carrying them out, I think.

772. (*Sir John Karslake.*) You have said, I think, that in your opinion, where a fact has been thoroughly well established, no further experiments should be made simply for the purpose of confirmation?—No further experiment without anæsthesia; but I think it is a very difficult thing to state when a fact is really established.

773. But assuming it is established, do you think you ought not to continue to put animals to pain

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merely for the purpose of ascertaining the truth of an alleged fact?—Yes, that would be a useless experiment.

774. In your profession, are the results of those experiments generally made public soon after the results have been ascertained, I mean in the different medical journals?—Any important results are of course made public. Of course there are many experiments from which results cannot be drawn.

775. But where an important result has been obtained by putting an animal to pain, it is generally published to the profession soon after it is known, I suppose?—The investigator is generally quite anxious enough to make himself known as the discoverer.

776. He is not like a patentee who wishes to keep his discovery secret?—Certainly not.

777. (*Mr. Forster.*) Have you seen any experiments performed under the curari poison?—I cannot say positively that I have. I do not know much about it.

778. (*Mr. Hutton.*) You would, I think, put the object of these experiments just as Dr. Sharpey did. You do not limit them to cases where you see a prospect of really alleviating human suffering, but you would say that simply for the purpose of extending science such experiments were justifiable?—The extension of science is for the purpose of relieving human suffering.

779. But if you did not see that object directly in view, you could justify these experiments for the other reason?—Yes, certainly, because all science has relation to the advancement of man.

The witness withdrew.

Adjourned to Saturday next at 12 o'clock.

Saturday, 10th July 1875.

PRESENT:

THE RIGHT HON. VISCOUNT CARDWELL, IN THE CHAIR.

The Right Hon. LORD WINMARLEIGH.
The Right Hon. W. E. FORSTER, M.P.
Sir J. B. KARSLAKE, M.P.
RICHARD HOLT HUTTON, Esq.

N. BAKER, Esq., Secretary.

Mr. WILLIAM PRITCHARD, M.R.C.V.S., F.C.S., called in and examined.

Mr. W. Pritchard, M.R.C.V.S., F.C.S.

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780. (*Chairman.*) You are professor of anatomy in the Royal Veterinary College?—Yes.

781. Have you the care of the greater part of the infirmary?—I have.

782. Do you perform most of the operations that are performed there?—I do.

783. The late Professor Spooner was the principal there at the time of its institution, was he not?—No, he was preceded by Mr. Sewell, and Mr. Sewell by Professor Coleman.

784. But from the time of its institution the principals have been Mr. Coleman, Mr. Sewell, Mr. Spooner, and now Professor Simonds?—Yes.

785. Are the operations of which you speak performed for the cure of the animals?—Yes, entirely.

786. You have no operations for the purpose of experiments?—None.

787. Do you think it proper that there should be such experiments?—No, I think it very improper.

788. As far as your knowledge is concerned do you believe that such experiments are performed in this country by veterinary surgeons?—I am not aware of it, and I do not think it is the practice.

789. If it should in the course of our inquiry be proved to us that such experiments are so performed, you would think it very improper?—I should.

790. And you would think that persons in your own position, and such names as you have already mentioned to us in connection with your college, would be ready to support the Crown and the Government in any reasonable measures for preventing such things?—I quite think so.

791. Can you tell us what use you have made in your practice of anæsthetics?—We are in the habit of occasionally administering chloroform in such quantity as to render the animal under operation insensible, and we occasionally have recourse to the use of local anæsthesia.

792. By "local anæsthesia" do you mean such means as you think render the animal in that part unconscious?—Yes.

793. Such as what for instance?—Ether spray is introduced; continued application of cold is sometimes had recourse to,—cold water, iced water.

794. What is the reason why you do not universally employ chloroform in painful operations?—My own reason is because my experience tells me that horses do not recover from wounds so well after the administration of it, as when operated upon without it; simply because we have to administer so large a quantity that it becomes poison to a considerable extent; it poisons the blood of the horse.

795. May we take it as a fact that there is a broad distinction between the proportionate quantity of chloroform which must be taken by the horse and that taken by the human being?—I think there cannot be a doubt about it, because it is almost impossible to destroy a horse by chloroform; I believe it has been done. I have several times had horses under it for hours together.

796. Does your experience extend to dogs?—Yes.

797. What is your opinion about the sort of questions which I have put to you when applied not to horses but to dogs?—With regard to dogs I should never think of applying chloroform at all; I should think it very unsafe to do so. The dog has an intermittent pulsation; the heart's action is intermittent.

798. (*Lord Winmarleigh.*) Invariably?—Invariably. They appear for some time not to be under the influence of it at all, and then suddenly they come under the influence of it and we find it impossible to bring them round.

799. Does any cruelty attach to the death under those circumstances?—I should say not; it is an awkward thing for the operator and for the owner.

800. (*Mr. Forster.*) With regard to cats what should you say as to the use of chloroform?—I have never administered it to cats; I do not think there would be the same risk.

801. (*Chairman.*) Supposing you had a painful operation to perform on a cat, you would use anæsthetics?—I think so.

802. (*Mr. Forster.*) Ether you would apply to a dog, would you not?—My experience would not allow me to answer.

803. (*Chairman.*) Do I rightly understand you to say that the circulation of the dog being intermittent, there is much more danger that the animal would never revive than there would be in the human being?—Quite so.

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804. Then may I take it that your opinion is this, that anæsthetics ought always to be used to animals where it is possible, but that you have found more difficulty in applying them to a horse or dog than there is in applying them to a human being ?—There is more risk in the case of the dog ; and the objection I have to it in the horse is the quantity we are obliged to give.

805. Not because of the consumption of the quantity, but because of the injury done to the horse by taking that quantity ?—Quite so.

806. (*Lord Winmarleigh.*) In the Veterinary College do you ever admit surgeons in common practice to attend your lectures, or do they ever attend them ?—Occasionally they attend ; but if they desired to come continuously, that is to say, to attend a course of lectures, then they would be expected to enter as ordinary pupils.

807. Do you ever allow them to operate experimentally ?—No, never.

808. And there is nobody but yourself that is authorised to operate upon horses and dogs ?—The principal, Professor Simonds, and the assistant professor, Mr. Axe.

809. Those are officially authorised ?—Yes ; they are officers of the College.

810. Do you ever allow the pupils to operate ?—Never.

811. However far advanced they may be ?—Quite so. They administer the medicines under superintendence, but are never allowed to perform operations.

812. You cannot speak to any results except those that you have derived from horses and dogs ?—And oxen and sheep.

813. In the case of oxen and sheep, do you administer chloroform ?—Occasionally.

814. The same quantity as to the horse ?—They do not take so large a quantity as the horse, particularly the sheep.

815. Do you ever use curari ?—It has been used, but not during the time that I have been at the college.

816. You have no experience of what the effect of it is upon an animal ?—No, I have not ; no personal experience.

817. Have you any reports in the college on experiments of that kind ?—Yes.

818. What is the tendency of those reports with regard to that poison ?—Unfavourable, so far as my memory serves me.

819. Are they to the effect that it deadens pain ?—I could not say. To the best of my recollection it produced violent muscular contraction, spasmodic contraction.

820. (*Mr. Forster.*) Supposing that there was an experiment which had to be performed upon a living animal, and that the object was to do it with as little pain as possible, and that there was no hope of saving the life of the animal, what anæsthetic would you then recommend ?—Chloroform.

821. Do you think that chloroform really destroys the consciousness of a dog ?—Yes.

822. Then the fact of the circulation being intermittent would not prevent that result. I understood you rather to say that they appeared sometimes as if it had no effect on them ?—Just so ; but when chloroform is given they suddenly, as a rule, become unconscious, and we frequently find that it is impossible to revive them.

823. And if you were obliged to try such an experiment (I understand you to say that you do not think it at all necessary), you would put a dog under chloroform ?—I think you are misunderstanding the previous answer I gave. If an experiment could be performed on an animal which would bring about such a result that other animals in large numbers would be benefited, I should advocate the performance of it.

824. I was going to ask you, do you know of any cases in your study of the treatment of animals in which experiments upon living animals have enabled you to treat them with greater success ?—Yes.

825. Will you tell us what they were ?—I divided the nerve of sensation supplying a bony tumour.

826. I am afraid I did not make myself understood. It is stated, you know, by the advocates of these experiments, that not only the lives of human beings are preserved and their pain diminished by the experiment, but also that animals are better cured by the help of it. Do you know of any cases in which your treatment of animals, or the treatment of them by other surgeons, has been made better by the results of experiments on animals ?—Yes ; in cases of shortened tendons from disease, or from the shortening of them during the life in the womb. An experiment was tried some years ago of dividing those tendons, and seeing if the gap would fill up with new material, and so bring about a recovery ; but that experiment was tried upon a case, and not upon a healthy animal. It was simply this, that the animal was useless as it was, and the operation was performed for the purpose of seeing whether he might be rendered useful.

827. (*Chairman.*) What you have now called an experiment, was the case of a diseased animal, which, coming before you, you endeavoured to cure ?—Quite so.

828. It was not a case of taking a healthy animal and treating it medically for the purpose of experiment ?—No ; I have no recollection of that being done.

829. (*Mr. Forster.*) There are experiments that are made, we are told, upon animals, which are pathological experiments, not what is called vivisection, but giving them diseases and watching the process. Have you anything of that kind done at the college ?—A little has been.

830. To what extent and in what way ?—Dr. Cobbold some years ago made some experiments upon animals with parasites, with a view of determining how the animals became possessed of them.

831. Did information of value come from those experiments ?—He considered so.

832. Have there not been some experiments to try and trace the origin of the cattle plague ?—Not that I can recollect.

833. (*Lord Winmarleigh.*) There have for glanders, have there not ?—Yes.

834. (*Mr. Forster.*) Do you know anything about the Brown's Institute ?—No, I do not.

835. You cannot speak to that at all ?—Only from hearsay.

836. (*Sir John Karslake.*) As to the case that you have mentioned in which the experiment was tried on a colt with shortened tendons, what was that experiment ?—A division of the tendons midway between the knee and the fetlock ; the tendon at the back of the leg was divided to a certain extent, and then the foot brought into its normal condition ; and the question to be decided was whether the gap which existed after the foot had got into its proper condition would be filled up with such material as would allow of the animal travelling and being of service.

836*a*. Did it succeed ?—It did.

837. I suppose in that case there was reason to anticipate before the experiment was tried that it would be successful ?—Yes. I may add to that, that in every case, unless I had some pretty good faith that what I was about to try would bring about a beneficial result, I should not do it.

838. Where you were trying to cure an animal you never would submit the animal to a painful experiment unless you hoped there would be a good result ?—Certainly not.

839. Do I rightly understand you that none of the pupils in the college perform operations themselves ?—None.

840. Not even the simplest, for instance, firing ?—No ; we should call the minor operations the giving a horse a ball or drenching it ; but we should never let them use the knife.

841. (*Mr. Forster.*) When you said that some time

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ago there had been some pathological experiments, I suppose there have been no such experiments for the sake of general information as to the use of medicine of human beings. They have all been experiments bearing upon the diseases of animals, I suppose?—Directly; but I have no doubt that the human being would indirectly benefit from them.

842. Is it, or is it not, the case that at present you make use of your college for the purpose of such experiments, giving diseases to animals?—It is occasionally done. At the present time there are experiments being carried out with a view of determining some facts in connection with typhoid fever in pigs; but such experiments are not repeated for the sake of illustrations to the class, or anything of that kind.

843. But you do give a pig such a disease?—In order to determine a point which we are doubtful upon, and as to which we cannot obtain the information from any other source.

844. (*Chairman.*) In such an experiment as you spoke of about the tendons would the pain be diminished or taken away by a local application of ether?—Yes.

845. I understand from your former answers that in your operations ether would accordingly be used?—Undoubtedly.

846. (*Mr. Forster.*) You must have studied the animal frame very attentively; have you formed any definite idea as to the comparative sensitiveness to pain of different animals, as for instance between a horse and a dog?—Well, I have performed some thousands of operations on them, and I have never yet been able to detect any difference in sensation between the skin of either one or the other, and the human subject, beyond this, that the cuticle or external covering of the skin is thicker in some animals than in others, and of course the knife has to penetrate deeper to reach the sensitive structure; but when once it has reached the sensitive structure I think it is as sensitive in the one animal as the other.

847. And you think that as regards the mere physical sensation of pain it would be equal to that in a human being?—Yes, I have never seen anything to lead me to think otherwise.

848. Have you any opinion as to what it would be in the case of frogs?—I think there would be sensation to a similar extent.

849. (*Lord Winmarleigh.*) That they would be as sensitive as horses?—Yes.

850. What is your reason for thinking so?—We find that the irritation of a parasite on the external surface produces as great an irritation in the small animals as it does in the larger ones.

851. (*Sir John Karslake.*) Have you had to perform any experiments at all for the purpose of ascertaining the effects of poisons on animals; I am talking of experiments strictly speaking,—killing animals by poison in order to ascertain the effect of the poisons upon them?—Not that I can recall to mind; I do not recollect that I ever did.

852. With regard to diseases that you have given to animals, I understand that has been the case?—That has been done in the college, not by myself.

853. Have any experiments been tried in the college upon sheep for the purpose of ascertaining what the fluke disease is?—Yes.

854. What sort of experiments?—They have been experiments for the purpose of determining the early formation of the fluke.

855. Were those experiments which rendered it necessary to kill the animal in the course of the experiments?—No, but an animal has been subsequently destroyed to see the effect of what has been done.

856. The fluke will actually kill the animal sooner or later, will it not?—Yes.

857. And have you ever allowed an animal to take the pupa for the purpose of engendering the fluke in the liver or the biliary ducts in order to ascertain how long it will take to kill the animal by means of the fluke?—The effect of the administration would be noticed before the disease had advanced to such an extent, and then the animal would be destroyed.

858. That is to say, the effect would be noticed by the dropping off of the wool and the emaciation of the animal?—The first effect is the reverse of emaciation; the animal is fattened.

859. And then soon after it has fattened very much indeed emaciation comes on very rapidly?—Yes.

860. And when you have ascertained that, you kill the animal to put it out of pain?—Yes.

861. (*Mr. Hutton.*) Have you seen anything of that parasite, the trichiniæ, which caused so much alarm and so many fatal cases in Germany?—I have not made experiments with them myself, but superintended some for Dr. Cobbold some years ago.

862. You have given them to healthy animals, a I understand you?—With a view of determining the animals they inhabited, and in what way they could be conveyed to the human subject.

863. And was it with a view to determining any possible remedy or cure?—With a view to prevention.

864. Were those experiments in the result beneficial?—Yes.

865. Did you find any prevention for the evil?—If laws were carried out to prevent it under the conditions determined by these experiments, I think they would have that effect.

866. You mean laws preventing the importations of unhealthy animals—something of that kind?—Yes.

867. But no means have been discovered of curing the diseased animal when once it has got the disease?—I am not aware of any.

868. Nothing was discovered on that subject?—Not that I am aware of.

869. What is your feeling about having a physiological laboratory for experiments of this kind in connection with your institution, the Veterinary College?—My feeling is rather adverse to that.

870. You do not think that the result would be useful enough to warrant the number of painful experiments that would have to be carried out?—No; I do not think the result would be sufficiently beneficial. And again I think it would be very difficult to control or to manage it so as to prevent useless cruelty.

871. That is to say, that once an institution of that kind was set up there would be so many scientific questions put, that needless investigations would be pursued?—That is the opinion I have.

872. Have you seen any institutions of the kind, either abroad or at home, in connection with veterinary colleges?—No; my experience when I was abroad, although I cannot say but what they behaved very courteously to me, was that I was not allowed to see anything that might have been going on of that kind; that is the impression I had.

873. Will you tell me what the operation for navicular disease is, and whether you consider it a beneficial one?—It consists of dividing the nerve of sensation supplying the foot or the feet.

874. And it is not injurious to the animal eventually, you think?—I think not, not if the case is properly selected.

875. It is sometimes said that it is giving the animal intense torture, simply to make it appear to grow better; what do you say to that?—That is a very erroneous impression. The wound in the skin is not more than half an inch in length, and the division of the nerve is instantaneous; and the leg is prepared anæsthetically.

876. I did not mean the intense torture of the operation, but the subsequent effect; is it great pain to the horse?—No.

877. Not at all?—No.

878. And has it no bad effect on the nutrition of the bone?—No.

879. You know of no experiments on healthy horses for the sake of investigating the cure of that disease?

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—No; I never recollect one at all. I am sorry to say we have too many diseased cases to need any experiments on healthy ones.

880. In fact you have quite enough experience without trying it on healthy ones?—Yes; I am afraid that the majority of horses at the present time in London are affected with navicular disease.

881. (*Mr. Forster.*) What is that?—It is an ulceration of the little bone which we call the navicular bone.

882. (*Mr. Hutton.*) You do not, as I gather, look for much good to animals from a physiological laboratory where these experiments might be performed for inducing disease; but it would be rather for the benefit of men than of animals?—I should not like to say that they would produce no beneficial effect; but I do not think that the effect would be of sufficient importance to lead me to advise that such an addendum should be made to our institution.

883. (*Mr. Forster.*) And also you find that the information is obtained in the process of trying to cure the animal which supplies the place of experiments?—To a great extent.

884. (*Mr. Hutton.*) And much more to the point, I presume?—Far more to the point.

885. (*Mr. Forster.*) You know that there are two theories with regard to curari, one that it does destroy consciousness of pain, the other that it does not, but merely prevents muscular action?—Yes.

886. Have you any opinion on the matter?—I have not.

887. In that state of doubt would you think it safe to use it as an anæsthetic?—I should not use it.

888. (*Mr. Hutton.*) Would you inject opium into the veins for dulling the pain? Do you ever do that with animals?—I think it was done sometimes in the cattle plague, but the effect produced did not guarantee our repeating it.

889. Practically you use nothing but chloroform and ether?—I myself use chloroform, ether, and local anæsthesia.

890. (*Lord Winmarleigh.*) I think you said that you were not conversant with any experiments with a view to ascertaining the nature of the cattle plague?—I was engaged for the Government myself in connection with Professor Varnell, then an officer of the college, to endeavour to find out a cure for it.

891. What experiments did you make with that object?—We administered several classes of medicines, and had recourse to outward appliances of different kinds and character.

892. You never made use of the disease itself by inoculation or anything of that kind, I suppose?—I could not say without referring to my notes; but I believe that inoculation was resorted to with a view to determine whether a milder attack would not be the result; but we did not find that it succeeded.

893. Did you find that any of your experiments succeeded with a view to stopping or alleviating the cattle plague?—No, they entirely failed.

894. Were any of those experiments which you made of a painful nature to the animal?—No, I do not think so, beyond the pain of injecting the material into the veins.

895. (*Chairman.*) The experiment was followed by whatever distress the cattle plague itself involved?—Quite so.

896. (*Mr. Forster.*) There have been experiments with inoculation for pleuro-pneumonia, have there not?—Yes.

897. (*Mr. Hutton.*) Have you had any experiments for the foot and mouth disease?—I have not personally carried them out, but I know there have been some carried out at the college.

898. You do not know with what result?—I could not speak as to the results.

899. (*Sir John Karslake.*) Were they experiments on diseased animals?—No, on healthy animals.

900. (*Lord Winmarleigh.*) Have you any belief that any future experiments that may be made on living animals may tend to the cure of the cattle plague, if it should occur again in this country?—No.

901. (*Chairman.*) Your opinion with respect to experiments on healthy animals, for the purpose of curing or relieving the sufferings of other animals, is an unfavourable one?—Yes; as to the majority of diseases there are some of which the spread may be prevented or avoided, but in the majority of cases the results are unfavourable.

902. But how can the experiment on the healthy animal prevent the spread of the disease to other animals?—By determining whether the disease is contagious or infectious.

903. That is to say, discovering whether it is contagious or infectious leads to greater care in the management of the disease?—Yes; and so prevents its spreading.

904. (*Mr. Forster.*) As regards inoculation, if you could have found that inoculation was a cure for cattle plague it might have been of immense advantage to animals?—Yes, as has been the case with small pox in sheep.

905. There it answers?—There it answers.

906. As regards pleuro-pneumonia it is still a question of doubt whether it answers or not, is it not?—Yes; the experiments have not led to any decided results at present.

907. (*Mr. Hutton.*) As regards drugs, do you not find that the effect of drugs on animals and on the human frame are exceedingly different; for instance, you can give much larger doses of aloes to dogs than to human beings, can you not?—Yes.

908. And generally would you say that the effects of experiments with drugs on animals are instructive, or not instructive, with regard to their effect on human beings?—I do not think that the use of drugs on animals can be taken as a guide to the doses or the action of the same drugs on human subjects; and therefore I do not think they are instructive, or only to a slight extent.

909. (*Lord Winmarleigh.*) At any rate they cannot be depended upon?—They cannot be depended upon.

910. (*Mr. Hutton.*) That does not apply to the severer poisons, but to drugs meant for healing?—Some things may be learned from those experiments, but they are not to be depended upon.

The witness withdrew.

Adjourned to Monday next at 12 o'clock.

Monday, 12th July 1875.

PRESENT:

THE RIGHT HON. VISCOUNT CARDWELL, IN THE CHAIR.

The Right Hon. Lord WINMARLEIGH.
The Right Hon. W. E. FORSTER, M.P.
Sir J. B. KARSLAKE, M.P.
JOHN ERIC ERICHSEN, Esq.
RICHARD HOLT HUTTON, Esq.
N. BAKER, Esq., Secretary.

MR. HENRY WENTWORTH ACLAND, M.D., F.R.S., called in and examined.

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911. (*Chairman.*) You are Regius Professor of Medicine in the University of Oxford ?—Yes.

912. And a Fellow of the Royal Society ?—Yes.

913. And President of the Medical Council ?—Yes.

914. Has your attention been drawn to the subject which is referred to this Commission ?—Yes, it has been.

915. For a long period of time ?—I think that I first considered the matter about 33 or 34 years ago.

916. And what drew your attention to it at that time ?—I was then a medical student, and the subject came before me in the course of my anatomical and physiological studies.

917. And what impression have you formed in consequence ?—That question is so large an one that I scarcely know in what way to answer it.

918. Is it your opinion that experiments upon living animals are or are not necessary for the investigation of medical subjects ?—The first answer I should give to that would be, that human knowledge would be entirely in a different position from that which it now is if such experiments and observations had not been made.

919. Then you consider that they are necessary ?—That does not follow from that statement.

920. Then what may the Commission consider to be your view as to the necessity of them ?—I think that if it is essential thoroughly to extend human knowledge in that direction, then observations and experiments of this kind may be counted to be necessary.

921. Do you know the resolutions of the committee of the British Association ?—Yes, I have them with me here. The first of them refers to the influence of anæsthetics in the conduct of such experiments. But I would observe that by far the greater number of the experiments which have added to human knowledge in this direction, were conducted without anæsthetics. I merely wish to note that as a matter of fact, and that at the time when I first seriously considered the subject they were necessarily so done, for anæsthetics were not known. At that time there were a number of experiments and observations of the highest class, which had been conducted by Sir Benjamin Brodie, Doctor Hope, and others; those experiments related to the nervous system, to the circulation of the blood, and to the structure of the heart. They were of such a kind that I suppose their results could not have been attained in any other way. They were made (and I am anxious with your permission to make this statement) by persons not only of great intellectual power, but of tender and gentle natures; they were in themselves experiments of a revolting and grave nature—but they were done by such men. They no doubt did produce great results to medical science. There is no question about that. But it is right to add that from their nature some were made upon animals just killed, and, therefore, they were not, properly speaking, upon living animals. When I knew that the Commission desired to ask me questions on this subject, I felt that the first thing that I should have to ascertain was what exactly the Commission means by "vivisection."

922. The word vivisection is not used in our Commission; the term is "experiments on living animals"? —Then that comes exactly to a point about which, at the outset of anything that I have to say, I should desire to be one at with the Commission. There are observations made upon living animals which are absolutely harmless in every respect. I will mention at once one class which is well known to the members of the Commission,—observations upon the circulation of the blood made in transparent textures. These are a series of observations which have been made on the circulation, first in vegetables, and secondly in animals. The observations on circulation in vegetables manifestly are of course harmless, because we presume that vegetables suffer no pain; we do not perhaps certainly know that, but we presume it. The next are observations upon transparent textures of animals, of which the most notable is on the web of the frog. Now an ordinarily careful person will tie the toes of the frog with silk threads and keep them open, and so examine the circulation in the frog's foot, and this I understand is considered not cruel. Yet I have heard objections taken, and they have been taken against myself, for tying the toes of the frog in that manner for the observation of this web. That I think is an absurd objection. But now comes this, — that in order to observe these transparent membranes, some persons I believe, with entire indifference, take a living frog, open the abdomen of the frog, spread out the interior living membrane, called the peritoneum or mesentery, and there observe the course of the vessels. Now if the frog suffers pain, that is as painful, I suppose, a thing as can be done. So when we want to observe the circulation, we observe it in vegetables, we observe it in an almost painless way in an animal, and we may observe it in a painful way in an animal; and now mark the result. About the time I allude to, 30 years ago, there was a great impulse in every department of biological knowledge from the manufacture of improved microscopes. Therefore a great number of experiments and observations came to be made of such a nature as I have described, which could be performed before either painfully or not. I will not say that the painful experiments were needless, because I think there were some points that could be made out by painful experiments, that perhaps were not to be made out by others. At all events the amount of knowledge that was gained by this practical observation of the circulation in living animal structures is such that it transformed the whole of our conceptions of the animal economy, and made things, which were matters of speculation before, matters of certainty, just as certain as that the despatch box before you is on your table. I mention these simple cases as an illustration of what seems to me to be the graduated difficulty of the question before you.

923. But you said that some of these experiments were painless, that some were severe, and that nearly the same result might have been obtained from the painless experiments as from those which involved severe suffering ?—I did; but if we were to attempt to go into the whole history, either of the experiments or of the actual knowledge which has been obtained from them, and to sift out of that mass of information and of history all that was serviceable and all that was not serviceable, all that was needlessly cruel and all that was necessary torture, we should find ourselves involved in a task which would occupy me, at all events, if I were to undertake it, many months. I do not know whether I could ever complete it, for this reason, that it has been necessary in the

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pursuit of physiological knowledge to make experiments in order to know what would be their result; and, therefore, when a person originally took the mesentery of a frog in order to examine it, I do not necessarily say that he was cruel. What I do think cruel is this (and this it is of importance for the Commission to observe), that when a thing is once established, and when we know all about it, needlessly to repeat the experiment over and over again.

924. Without stating positively that a particular experiment is in itself so severe that it ought not to be practised at all, you are quite prepared, are you not, to say that if a severe experiment has once been satisfactorily performed it is a needless cruelty to repeat it?—I am not prepared to say that absolutely and without reserve. I have no doubt that that would be excepted to by scientific men; because an experiment which is entirely satisfactory at one period of science may afterwards turn out to have been done under conditions from which something or another was excluded which modifies it. Therefore I would not say that Sir Benjamin Brodie's experiments, or other experiments which seemed to have settled and which did settle points at one period, might not require to be repeated at a subsequent one. With that reservation I should agree to the doctrine.

925. As a general doctrine you agree that an experiment involving severe suffering ought not to be tried upon a living animal, unless it were for the purpose of establishing some new and very important doctrine?—Generally speaking, I should say certainly.

926. And did the Commission rightly collect your impression that there was not 34 years ago on the part of everybody the desire that there might have been to limit painful experiments?—I do not think that I could answer that question with certainty, because it would imply a considerable personal knowledge of the individuals; but this I will say, that I am quite convinced that the men most to be depended upon in science, first of all would never waste their time, and secondly would never perform a single painful act which would needlessly give suffering to any creature—not such persons as Brodie or Hope, and I might mention many others. I am sure they never do so.

927. Do you draw from that the inference that such experiments should, as far as possible, be limited to persons of that degree of eminence?—Well I am inclined to think so now, but I am not therefore inclined to say that 25 or 30 years ago, when the first burst of this knowledge came upon the world, students were wrong in verifying observations; at all events I do not think they were morally wrong. I saw several myself and joined in them myself, and I do not think that I was morally wrong in doing so.

928. But without committing you to that opinion, are you of opinion that a reasonable limitation of such practices might be entertained by the Crown and Parliament?—Certainly, it might be entertained. Whether I am prepared to say that observations made for purely scientific purposes should be under the control of ordinary legislation is another question. I have not made up my mind that they ought.

929. If it were to appear that persons of comparatively inferior scientific knowledge and training resorted to such experiments, would you think that that was a state of affairs which called for the intervention of Parliament?—That seems to me to be a question which the Legislature and the Government have to settle. A scientific man ought to be slow to admit that it is necessary that the law should step in to regulate his mode of study. If the law steps in and physically coerces my mode of study, I must submit to that law the same as to any other. Yet I have a great unwillingness, as a scientific man, to admit the necessity of the law interfering.

930. The question that I put to you did not refer to persons of high scientific knowledge and attainments, the question was whether, if it should come to the knowledge of the Crown and Parliament that persons who were inferior in those respects nevertheless took upon themselves to practice painful experiments, you would see any objection to an interference with that latitude on their part?—No, I should see no objection to that at all, if a case can be clearly made out; but the question would arise at once in practice, who were the persons who were to be so coerced or so looked after as a matter of police regulation. In any well-conducted medical school I do not think that it ought to happen. I am quite sure, for instance, that at Oxford it could not happen now, because my colleague, Professor Rolleston, a man of great acuteness, and of still greater benevolence, would not allow anything of the sort in his establishment. And I should say the same of many whom I could point out. It is invidious perhaps to name living persons, but I will say the same of two of the most eminent observers and physiologists living, Dr. Allen Thompson of Glasgow, and Dr. Sharpey, who has lately retired from University College in London. It is inconceivable of these persons that anything should go on within their knowledge, otherwise than what it is right for them morally and intellectually to permit. And now that the scientific sense of England is called on to decide on this matter (and I am very glad that it is), I should much doubt whether the best judges of what class of vivisections should be performed, would not be either the heads of the chief departments of science or some scientific body, or somebody nominated by them for the purpose. I am not sure about that, because I think the Commission might at once ask me if I disapproved of the Anatomy Act, and if I disapproved of the inspection of the dissecting or anatomy schools, which I do not.

931. I am sure you will forgive me for repeating the question, because it is a very delicate part of the inquiry, and I only wish to get at the bottom of your thoughts by constantly dwelling upon the points until I have done so. What you have now said is that whatever should be done at Oxford for the purpose, or at the London University or at Glasgow, under such names as you have given, should be left to the discretion of those persons; but you have not yet said whether, if it should appear that persons of no such discretion, no such attainments, and no such general competency as those persons, do practice experiments upon living animals, it would be unreasonable and improper to take measures for controlling those persons?—I meant emphatically to answer that in the affirmative. I meant distinctly to say certainly if that can be established; because persons who wantonly or carelessly, for no good to mankind, and perhaps with great injury to themselves as well as to the animals, did so, ought to be dealt with as you deal with the other brutal part of the population. My reason for directing my answer to scientific bodies and the managers of schools is because, upon the whole, these experiments which, when conducted to any good purpose, are matters of extreme delicacy and difficulty, are only done in scientific establishments.

932. Now, since the period to which you have adverted, namely 34 years ago, anæsthetics have been discovered?—Yes.

933. Have they very materially changed the bearings of this question?—They have changed them so far as this, that you can perform most experiments without causing pain to the animal. Of course that is a great point gained; but I am not at all prepared to say that I think that even experiments causing painless death needlessly ought to be performed. I know there are two opinions about that; I know some persons think that it is a matter of entire indifference to take away the life of an animal. I do not think so; I should act in a way which would do great injury to my conscience if I were needlessly to put to death the meanest animal; I could not do it. And, therefore, I cannot say that because I can put an animal to death without pain, therefore I may unnecessarily destroy an animal, in order to show a thing which I can show just as well upon a diagram.

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934. But some experiments which would fall into the category of the necessary experiments, as you have laid it down, can be performed now under anæsthetics, can they not ?—Yes.

935. A great number of such experiments ?—Yes.

936. Therefore, whereas it was necessary 34 years ago in your opinion to inflict pain, it is no longer necessary to do so in those cases ?—I imagine that to be the case.

937. Are there not a great number of other experiments of which at least the greater part of the pain may be removed by anæsthetics ?—Surely ; probably there are very few that cannot be performed under anæsthetics.

938. Then I think we may collect from your evidence that it is the bounden duty of anybody performing such operations to minimise the pain as far as possible ?—I presume that that is generally if not universally so done in this country.

939. At any rate you think it ought to be done ?—Certainly.

940. And if there were any cases established in which that was not the practice, it would appear to you to be a circumstance calling for the consideration of the authorities ?— For their consideration. Observe again, I do not say for legislation, but I say for consideration. I believe that any such persons would meet their own punishment. I will undertake to say that such cases could very rarely happen, for instance, in London ; that a thing which would so shock the sense of the medical profession as operations of this kind performed habitually without chloroform, to the best of my belief (I may be misinformed), would simply produce a great disturbance ; and it would so shock any assembly in which it was done, that I do not imagine that there is an operating theatre or dissecting room where such a thing would be tolerated as performing any cardinal operation causing suffering to any extent without anæsthetics ; and if it be so I should be truly sorry to hear it, and it is a new fact to me.

941. I am not suggesting that such is the fact ?—It is so abroad. I have every reason for believing that it is often done abroad, with what I should call an unscientific carelessness, which I believe would be so hurtful to the moral sense of England that it would not be endured. But with regard to these facts, I ought to add that I have been so occupied in other departments since I was reader in anatomy at Oxford, that I have not visited the foreign physiological laboratories of late ; and, therefore, testimony on this point should be got from persons such as Dr. Rolleston and Dr. Humphry, and others who are still teachers of anatomy and physiology.

942. But it is your confident opinion that the public feeling in England secures the English community against the infliction of purposeless and uncalled for suffering upon living animals ?—In the medical profession, as far as I know, that is the case.

943. (*Lord Winmarleigh.*) That observation does not apply to the public generally ?—I cannot answer for them.

944. You cannot answer for the public generally. Your observation applies to the medical profession ?—I say that, because I imagine that these inquiries are for the most part confined to the medical profession, or persons allied to the medical profession. There are, however, biologists now in the country who are not medical men, and they are increasing in number ; I think that the Commission ought to be aware that the number of persons in this and other countries who are becoming biologists, without being medical men, is very much increasing. Modern civilization seems to be set upon acquiring, almost universally, what is called biological knowledge ; and one of the consequences of that is, that whereas medical men are constantly engaged in the study of anatomy and physiology for a humane purpose,—that is for the purpose of doing immediate good to mankind,—there are a number of persons now who are engaged in the pursuit of these subjects for the purpose of acquiring abstract knowledge. That is quite a different thing. I am not at all sure that the mere acquisition of knowledge is not a thing having some dangerous and mischievous tendencies in it. I know that would be thought by many persons a very *arriérée* and foolish opinion. Nevertheless I am not at all prepared to say that the mere desire to attain so much more knowledge is a good condition of mind for a man ; at all events it was not the way in which all this mass of physiological and biological learning was added to the world, because it was added to the world by men who were obtaining it for the sake of doing good with it—doing good I mean to their fellow creatures by relieving suffering ; that was a chief incentive to biological studies. But now it has become a profession to discover ; and I have often met persons who think that a man who is engaged in original research for the sake of adding to knowledge is therefore a far superior being to a practising physician who is simply trying to do good with his knowledge ; that he is a superior being, because he is devoted to pure research. Now that introduces a new element into the question, and I believe it will be found that a great part—not the whole, observe—of this difficulty has arisen because there has come to be a pursuit of knowledge in this direction, just as you pursue knowledge of metals with the ordinary apparatus of a chemical laboratory. So many persons have got to deal with these wonderful and beautiful organisms just as they deal with physical bodies that have no feeling and no consciousness. The multiplicity of these investigations, I have no doubt, has in a great measure arisen in that way.

945. (*Chairman.*) This desire for knowledge on this subject is spreading all over Europe, including this country ?—Yes ; certainly.

946. And although, so far as you know, there has not yet grown up any indifference to animal suffering in this country, yet it is your opinion that a great deal is done on the continent in the way of experiment, which in your opinion would be very shocking to the sentiment of the people of this country if it were done here ?—Yes, according to what I am informed on what seems to be credible authority. If things were done in London which are, as I am informed, habitually done in some one or more laboratories on the continent (I do not specify them, because I have not visited them), I believe they would not be tolerated. Whether that be true or not the Commission can easily ascertain from other witnesses.

947. I asked you a little while ago whether you were acquainted with the resolutions of the Committee of the British Association. Do you agree with them ? —Upon the whole. I think the first, which says that " No experiment which can be performed under the " influence of an anæsthetic ought to be done with- " out it " is so obvious that I am almost ashamed of having to assent to it. Of course it ought. Then the second is, that " No painful experiment is justifiable " for the mere purpose of illustrating a law or fact " already demonstrated, in other words, experimen- " tation without the employment of anæsthetics is " not a fitting subject for teaching purposes." I assent to that. In fact I go further than that, because I do not think that experiments ought to be performed on living animals which either give them suffering or cause their death without necessity. I should feel it, as I have already said before in other words, in myself an immoral act as a teacher, remembering that I lectured on anatomy for 14 years,—I should think I was guilty of an immoral and unjustifiable act if I ever showed upon a living animal, so as to cause its death or suffering, a thing which could be shown by diagrams or dissection. I have made hundreds of dissections for the University of Oxford, which are preserved now, in order that these dissections may not have to be repeated. They are preserved for study, because these things have to be seen. They took a great deal of trouble and expense to prepare. A number of persons, such as John Hunter and Professor Owen, have spent a vast amount of labour in

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making these dissections to show to other learners. It is not necessary in order to show the heart of a fish or a rabbit or any creature to destroy the creature to show it, because I have already got them put up in the museum. I should think it wantonness to kill an animal in order to show a thing when it is already shown in an animal which I have dissected and put up in the museum. The third resolution contains these words, " For this reason no painful experiment " ought to be performed by an unskilled person, with " insufficient instruments and assistants,"—of course not,—" or in places not suitable to the purpose,"—of course not. These resolutions seem to be things about which there cannot be two opinions. The only doubt is whether they are to be enforced by law; about the propriety of them there can be no question.

948. And whether they should be enforced by law would depend in your opinion, I think I have collected, upon whether it was established that a necessity for such interference existed in this country ?—Yes, I think so. If the Commission collects evidence which shows that the scientific men of England are careless and wanton, then I say that we deserve the humiliation of being looked after by the police; but I think it will not turn out so.

949. But you have already said, I think, that, without in the least degree supposing that any eminent scientific people are capable of anything of the sort, if it should turn out in evidence that any other persons practise anything of the sort, then it would be right that there should be some legal repression to prevent a recurrence of that evil ?—I have no objection to say that, because, as I said before, it will, I suppose, be found very rarely to occur, difficult to establish, except in the case of wanton and careless students, who I should have supposed would, under the existing law, be punishable under the Cruelty to Animals Act or some other Act; I mean in cases where they are not pursuing the experiment for scientific purposes; in such a case it is an act of wantonness, and if there are any schools where such things could go on, the schools themselves are not fit to exist as schools.

950. Nothing is further from my intention than to suggest to you that there is evidence before us at the present time that anything of that sort does exist; I merely put the question to you hypothetically, the hypothesis being so far sustained that, in your opinion, there are many things done on the continent which would fall within that category ?—Yes.

951. Has this subject been brought before the General Medical Council ?—No, it has not; and I am glad that you have asked me that question, because it enables me to say that it would have been brought before the Medical Council this year, at the session which has lately closed, had it not been that the Government had appointed your Lordship's Commission. We were going to discuss the question of the extent to which what is called practical physiology, which means in great measure this subject, was requisite for students; but my colleagues who were going to bring the matter forward felt that it would be more seemly if we delayed until after your Lordship's Commission had reported, and so the subject was for the time dropped; otherwise it would have been brought forward.

952. We have hitherto spoken chiefly of physiological and biological experiments. Are there not many experiments which are called pathological ?—Yes. These investigations may very properly be divided into four classes,—physiological, pathological, surgical, and toxicological; that is, into the actual structure and mode of action of living beings, their diseases, the way of treating them in surgery, and the way of examining those agents which are injurious to life and productive of disease. Now, with regard to the class of which your Lordship asked me, the pathological, there are a number of experiments which are not properly called vivisection, I suppose,—experiments on animals,—of which, one of the most notable or characteristic is the production of consumption. Experimentalists have endeavoured to ascertain the condition under which the disease called consumption speaking broadly, or tuberculosis as they technically call it, may be produced in animals. Now at first sight it seems to be a very useless operation to try and cause consumption in a rabbit or guinea pig, or any other animal. But supposing that by ascertaining how it is produced you can ascertain how to hinder its being produced, what then ? A more curious instance than that has occurred, because that happened to be not a very difficult thing. Brown-Séquard, one of the greatest experimentalist in this department of knowledge, has ascertained how to give epilepsy to guinea pigs; and that is a very singular thing; but further than that, he discovered how to make it hereditary in them. Now, supposing that having found out the way in which epilepsy was transmitted to the descendants of animals, he was to find out the way to hinder its being transmitted to human beings, what a result that would be! The public, who are not familiar with the complicated mode of investigation and reasoning which goes on in Medicine, can hardly appreciate the difficulties and magnitude of the questions involved. I think that the examples of epilepsy and of consumption which I have mentioned are an illustration of that. Even for us, who spend our whole lives in it, it is very difficult to follow the intricacy of some of these inquiries. The public are very much interested in one, viz., the origin of man, the Darwinian hypothesis. The transmission of national characteristics includes the origin and transmission of diseases; and so the medical inquirer gets entangled in almost every department of human knowledge. I do not think that those who are not specially versed in the subject are right, unless they are very lenient, in judging of the characters and works of great investigators, such as Brown-Séquard.

953. The inference may be drawn, may it not, that such investigations as those require persons of the highest competence ?—The very highest. It is a very rare quality, that of being fit to do these things; it is a very rare thing even for a medical man, or for a professed physiologist, to be able to pursue this subject to any advantage to mankind.

954. And therefore it would be an infliction of purposeless distress for any other class of persons to make these experiments ?—Entirely, so much so that I should think it entirely unjustifiable in myself to do anything of the kind, though for so many years I was engaged as teacher of physiology. My hand is out of practice in the subject, from the fact of my work lying in other directions. I should think it simply unjustifiable in me to perform a single experiment of the kind. I should send it to an expert, and I should say " I rather think you may find such and " such a point capable of elucidation in such a way; " will you see to it ? " but I should not dream of doing it myself, because I am incompetent.

955. Is there anything else which you desire to add ?—I have in my last remarks alluded to the great question,—the question of the propriety of inflicting pain upon living animals, for the purpose of acquiring all the knowledge that we can of the structure of ourselves or animals. I will only add that I cannot look upon any inquiry of this kind excepting as a part of the whole question of the structure of the world in which we are placed, and of our intellectual and moral and religious nature. I look upon investigations into the structure of animals as part of the whole question of our Nature. People who cannot agree upon the existence of a future state, or upon the value of Christianity, and upon what therefore is desirable for keeping society together by the higher kind of morality, in fact people who do not know what their aims are with regard to this life or the next, are not very likely to come to an agreement as to the precise relations of man to the sufferings of the other animals placed in the world with him. At all events I look upon this subject in the light of humanity at large.

956. (*Lord Winmarleigh.*) You said in answer to a question of Lord Cardwell's, at the commencement

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of your evidence on this subject, that your approval of experiments on animals depended upon the question whether it is essential to extend human knowledge in that direction. I have collected from your subsequent answers that you do believe that it is very desirable to continue to extend human knowledge in that direction?—I believe it to be my duty as a physician, and the duty of the profession of medicine throughout the world, to do everything that we can to mitigate human suffering, to prevent disease, and to make the human race, especially the nation to which we belong, as strong in body and mind as we can; but not to do it in wanton and immoral ways, and not to do it for fancies of any kind, nor for the purposes of scientific ambition.

957. But subject to those limitations, you do consider that experiments on living animals are desirable? —Yes, as part of investigation, though I do not think that for the purposes of advancing the knowledge of medicine experiments on animals are the most important means.

958. Your observation applied then simply to physiological experiments?—To physiological experiments. I declined to sign a memorial lately in which it was stated that the progress of medicine depended mainly upon experiments on animals; I do not think it does, and therefore I declined to sign the memorial.

959. But you do believe that the progress of medicine is assisted by them, and beneficially assisted by them?—Yes.

960. Would you state who are the parties in Oxford under your control who are authorised to make these experiments in your own school of medicine? Is there any person except yourself who is authorised to make these experiments?—There is no person authorised at all.

961. You never allow a pupil or any person of an inferior position to yourself to do it?—I must not put it in that way. You must bear in mind that I have nothing to do now with the anatomical department; I have left it entirely. I spoke of my colleague, Dr. Rolleston, and I said that personally I had no doubt that he did not allow it. But there is no one to authorise him either to do it or not to do it.

962. But when you were in the position which Dr. Rolleston is now in did you allow any person but yourself to perform these experiments?—I never did allow anyone except my highly-skilled assistant, nor do I think he would.

963. (*Mr. Hutton.*) Did you not say that you were anatomical professsor formerly?—I was the sole teacher of anatomy and physiology in Oxford all the time that I was the teacher.

964. (*Lord Winmarleigh.*) Were all the dissections that you made, chiefly or altogether made under anæsthetics?—These dissections were dissections of dead animals almost entirely.

965. But when it was an experiment on a living animal was it done chiefly under anæsthetics?—I think that all the preparations to which I have alluded that were got from living animals were got from animals that were killed for the purpose.

966. I collect from what you have stated that you would not have made an experiment, except under great necessity, which inflicted pain without anæsthetics?—Certainly not.

967. You have alluded to a discussion which has taken place in the medical profession, and which was stopped in consequence of the issuing of this Commission. Will that discussion go on now before the report of this Commission?—The Medical Council sits generally once in the year, and in consequence of this Commission sitting the discussion was not brought forward, and of course it will depend upon circumstances whether it is relevant or desirable to bring it forward when we meet again. The Medical Council will sooner or later have to consider to what extent the subject of practical physiology, as it is called, should be required of all students, and no doubt the Medical Council would be materially influenced by the report of this Commission.

968. (*Mr. Forster.*) I understood you to say that you certainly would not allow inexperienced persons to try these experiments on living animals?—The only possible justification for that would be that it was desirable to train them to do it; because of course in order to get experience people must begin. Now I do not think that it is at all necessary, and therefore it is not desirable, that the average medical students, who have no chance of being experts in this matter, and who never will be, should go through all this.

969. And I think I also understood you to say that you believed, at any rate you trusted, that in England the men of science who were fitted to try the experiments would do them with care and inflict as little suffering as possible?—Certainly.

970. Has your attention been called to a handbook for the physiological laboratory, lately published by Dr. Klein, Dr. Sanderson, Dr. Foster, and Dr. Brunton?—Yes.

971. Does not that book rather suggest to students— "workers," as they are called in the preface,—how they can in their own study best get to understand physiology?—I suppose the object of that book is in a great measure to explain to learners how they may themselves pursue these experimental investigations.

972. Would you just turn to page 108. Opening it by accident at that page I find a mention of that experiment upon the circulation of the blood, as illustrated by the frog, to which you referred; and there it is said how the mesentery can be laid bare; and it is suggested that that should be done under the operation of curare. Do you know what effect curare has?—Upon the whole I do. There are some niceties about the employment of it, but upon the whole the effect of it is to destroy voluntary motion.

973. Do you feel confident that is destroys sensibility?—I cannot answer that absolutely; I do not know.

974. Then just looking at that experiment, would you not think that it was very questionable to suggest to practical students that experiment merely under curare?—Of course the object of that is to produce stillness in the animal; that is the intention.

975. Will you turn to page 113. I observe that there is there a "demonstration of the lymphatic system of "the diaphragm by injection." It is stated that a middle sized rabbit has been kept from 16 to 20 hours without food, and then it was bled to death in a process of three hours and a half. Do you not think that experiments such as those require to be under some legal regulation?—I have already answered very fully, I think, on that subject; I think that experiments on animals which are not calculated to elicit new truths, and which are not actually necessary for the education of competent medical men, ought not to be performed. But whether that is to be secured by legal enactment is another question on which I have spoken, I think, quite fully; I have a doubt about it.

976. (*Mr. Erichsen.*) As President of the Medical Council you have the general supervision of medical education in Great Britain and Ireland?—The Medical Council has, but as President I have not.

977. And you have the supervision of the Medical Council?—No; I am President of that Council, and the Medical Council has the supervision of medical education.

978. And you have stated that a great part of the development of these physiological investigations of late years has been owing to the requirements by the directors of medical education of the establishment of physiological labatories for the teaching of practical physiology?—Physiologists have demanded these laboratories.

979. And has it come to your knowledge as a member of the Medical Council, that in these physiological labatories students (I mean young men in *statu pupillari*—I do not mean advanced men who are studying for their own purposes, but young students,) practise experiments, either under super-

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vision or without supervision, on living animals?—I think I may say that the Medical Council has not had this subject before it. It has only existed for a very few years; it has had a vast mass of work to do in the way of organising medical study generally; but this detailed subject has not been brought before it, as I stated before. It would have been brought before the Council this year for special consideration in framing some new regulations which we were engaged upon, had it not been for the sitting of this Commission. To that I would add, that I have no doubt that next year as it happens this subject must be considered, because the Council is about to revise the whole of the details of education and examination throughout the country, and it will be quite necessary (I may presume I think to say this without consulting the Council on the subject) for the Council to decide whether in examinations practical experimentation is to be required of candidates for licenses or not; and if the Medical Council does not require that candidates for licenses and diplomas should themselves perform such experiments, you may be quite sure that no pass candidates will acquire that knowledge. But then a further question would arise, which would not be within the province of the Medical Council,—how far these subjects should be made a branch of examination for honours; but Dr. Rolleston will, I hope, speak on that point.

980. Can you tell me from whom the recommendation of the establishment of these physiological laboratories emanated? Was it from the Medical Council or from individual teachers, or was it made by the different medical corporations and examining bodies?—I have no official knowledge on that point, but I believe that I am right in saying that the laboratories of practical physiology have arisen in consequence of the zeal and earnestness of the teachers in the department of physiological science, who have been endeavouring as years have gone on to provide for their students all that they believe to be necessary for the mastery of biological knowledge. When I was a student myself, the use of the microscope was not taught in St. George's Hospital in the physiological laboratories and hardly ever in the wards; and now in consequence of the increase of knowledge and the increase of appliances, all the apparatus is gradually being provided which seems to elucidate the physical sciences, and to assist in experimentation.

981. You made a most important distinction, and one that has been made to-day for the first time before this Commission, that between medical men and a body of scientific men who are growing up in this country who are not medical men, but who are yet devoted to the study of the sciences connected with the operations going on in the living body, who are physiologists and biologists; and it is your opinion that the great progress that has taken place in the development of physiological work is owing to these men working at their science as a science, independent of its direct application to the purposes of alleviating human suffering?—I think that speaking roughly of the history of human knowledge in this direction, the pursuit of biology was formerly generally confined to medical men, that is to say, to a body of men whose first object in life was the alleviation of suffering or the prevention of disease.

982. Might we say practising medical men?—Well, I should say the medical profession generally; they were generally practitioners; but in the progress of knowledge the extent has become so vast that a man now, as is well known to the whole members of the Commission, can hardly master the whole of any one of the physical sciences, therefore they have been subdivided amony different inquirers; and consequently there has arisen a race of professed physiologists, whose business has not to do with the alleviation of suffering, but has to do with research as to the mode of construction of living beings throughout the world, and all that will elucidate that.

983. Referring to such men as you have mentioned, Sir Benjamin Brodie, Dr. Hope, Sir Charles Bell, Sir Astley Cooper, Mr. Travers, Mr. Lawrence, and a number of men of equal or similar position in the profession, the experiments which they performed were performed with a definite object to elucidate some point that was directly connected either with the pathology of diseases, or with a surgical operation and its treatment, or in some way or another they had a direct bearing upon the alleviation of human disease and suffering?—I could hardly lay that down with the precision which you do, because I think that although that was the starting point of all their inquiries, it would really be unjust to those great names, looking upon them as some of our great intellectual lights of different epochs, to say that they had not a love of knowledge and research as such, because I think that Sir Benjamin Brodie essentially had; but his starting point and the centre of his whole life was the acquiring knowledge for the direct alleviation of suffering, and the benefit of mankind in that way. At the same time, however, he had a great scientific instinct as John Hunter had If you look at the single case of John Hunter you will see at a glance what happened with him. He being a surgeon thought that he could aid humanity by dissecting over the whole range of the animal world, and he did so; but he started with his investigations into life and generation and development, and so forth, for the purpose of discovering how he might alleviate human suffering. Then he pursued his subjects with a purely scientific aim. But it is one thing to have that object of alleviation of human suffering as the basis of your aim and character, and it is another thing to be entirely separate from it, and to be pursuing these investigations from a simply intellectual stand-point. I do not say that in any way as criticism, but merely as fact.

984. And these men you have mentioned had that as their basis?—It was their basis no doubt.

985. And their labours were recognised, and were encouraged by learned societies in this country. For instance, the Royal Society awarded the Copley medal to Sir Benjamin Brodie for experiments on living animals?—Yes.

986. The British Association for the Advancement of Science has, has it not, at various periods made advances of money for the purpose of defraying the cost of experiments on living animals to elucidate certain points?—Yes; and the Royal Society's transactions and various scientific publications for the last two centuries are full of experiments and investigations bearing upon this subject, dating at least as far back as Harvey; but with definite aims, which I would say upon a large scale are becoming, as time goes on, less necessary, because the more salient points and the greater questions are becoming understood, and the greater number of the experiments, therefore it is quite unneccessary to repeat.

987. I suppose we may take it that you would object to the repetition of experiments for the purpose of verification where those salient points have incontestably been decided?—Yes, I think so.

988. And for the purpose of demonstration in classes before students you have not found it necessary, as a reader of anatomy, to perform experiments upon or to dissect living animals?—At the time that I was lecturer on anatomy the subject had not attained the magnitude that it now has. Such a book as Klein's did not exist. Knowledge has extended very largely in this direction in the last 25 years, and there is therefore much less need to show these experiments now; but I did think it right when I was teacher to show, on more than one occasion, a series of discoveries, among the most remarkable that the world has ever seen, namely those of Martin Barry upon the development of the mammiferous ovum. It was an exceedingly delicate and difficult operation, which then was perfectly new to the world. The exhibition of this mammiferous ovum implied the death of an animal.

989. (*Mr. Forster.*) But it did not imply the vivi-

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section of the animal ?—No. And that was not done with suffering. This is, I think, a very good illustration of that. I did it partly for my own instruction. I thought it right that I, being a teacher in anatomy, should see this, which up to that time had never been understood in the world before. I thought it right that I should show it to a certain number of select students, some of whom are now living, none I think medical men, but eminent men in various walks of life; but I should have thought it wrong to make it a common practice even to sacrifice a rabbit for that purpose. That may be foolish in me, but then I have always had a horror of dissection, though I have done a great deal of it. I exceedingly dislike killing animals for it, and should not do it except from necessity.

990. (*Mr. Hutton.*) Dr. Humphry in his evidence said that the line of demarcation between experiments that were desirable for the purpose of demonstration and those that were desirable for a physiological class was quite an evanescent line. He gave us the impression that although ordinary practitioners in the present state of education need not go through a physiological laboratory, it was desirable that all students that were thoroughly educated should do so. Now have you an opinion to give on that subject ?—The best short answer perhaps that I can give to that is this,—that I think those who have seen the actual working of the mechanical structures of living beings during life have quite a different conception of them from those who have only seen them in the dead body. I do not think that anything made a greater impression upon me during my youth than accidentally while fishing in a trout stream in Devonshire opening a small trout which I had caught, and cleaning it as it is called, that is taking the viscera out and having in my hand the palpitating and acting heart for at least half an hour after it had been removed. That filled me with such a sense of wonderment, and so impressed me with the beauty of these internal structures, that probably it was the cause of my being in this room at this moment. Now a person who has only seen these structures after they are dead and their use gone does not know anything about them at all, in the sense that a person does who has seen these wonderful phenomena. Therefore I say that some of them ought to be seen during life.

991. (*Lord Winmarleigh.*) The circulation of the blood was discovered in exactly the way in which you have described, your observing the heart of the fish, was it not ?—It is not quite certain what argumentation led Harvey to that, whether it was the observation of the living structure or the contemplation of the dead structure.

992. (*Mr. Hutton.*) I understood you to object to experiments for the mere discovery of fresh knowledge, apart from their subsequent influence upon the healing art, or rather that you have great doubts about experiments for the mere purpose of extending knowledge ?—I did not mean to express myself quite so strongly as that. I drew a line of demarcation between knowledge which I obtain for a subsequent benefit to mankind, and knowledge which I obtain merely for knowledge sake. They are two things. I do not presume to say whether one is really inferior to the other; I did not wish to express an opinion on that point. I merely drew a distinction between them. I have not the slightest doubt that it is right in me to take a step which will enable me to save mankind from suffering and pain if I can, if I have a clear view that that is the way to do it. But whether it is right in me to put animals to suffering and death in order to add to my range of knowledge, if I could know beforehand that it could do no good to man, is another question. But it would not be at all right for me to throw any disparagement on the pursuit of knowledge for knowledge sake; it is the last thing that I would wish to do; but I draw a line between them, because I think that they are two distinct vocations and two distinct dispositions of mind.

993. But you would think that the one would require more careful supervision and control than the other. You would be more anxious to restrain experiments for the mere prosecution of science than those for the alleviation of human suffering ?—I should personally, because I am a practical physician. I am quite certain of the value of prevention and cure of disease. But it is impossible to draw an exact line between what knowledge is and what is not likely to be practically beneficial.

994. You were referring to those experiments of Brown-Séquard's. In a paper which I have in my hand he says: "My watching over guinea pigs has been on such a scale (at one time before the siege of Paris I had 584 in my laboratory) that I can say I have had many and many thousands under observation from 1843 till now, a period of more than 30 years." Would you not say that if experiments are attaining to anything like those dimensions in England, they would require very careful supervision to see that the interests of the victims were protected to some extent ?—I should be very loth to sanction anything which added to the suffering in the world, either in men or in animals; and I very much wish that the public sense of right would take a great deal more pains than it does to diminish unnecessary suffering in the world. I wish to say nothing here on the subject of the various field sports, and what pass as innocent amusements; but as a matter of fact the amount of needless suffering in the world caused in other ways is far greater than the amount of suffering caused by such a person as Brown-Séquard; and however unpopular that opinion may be, it is simply a statement of a fact, and therefore I think it right that I should say it.

995. You would hardly say, I suppose, that any one sportsman had given pain to that extent; that I suppose is clubbing the work of a great many sportsmen ?—I should be very sorry to say he had not.

996. Brown-Séquard's is a case of a most distinguished man, of the very highest scientific power; but if the same class of experiments were to be conducted by any man of inferior genius surely they would be liable to the greatest possible abuse ?—Certainly, and they would be perfectly useless.

997. But who is to draw the line, if there is to be no kind of legal control in the matter as to the power of a man to prosecute with advantage experiments of this kind ?—I should feel myself to be a very imperfect judge of that; but I do think that the general opinion of scientific men, and especially of the medical profession, rings so true upon this matter, that upon the whole, especially now that the attention of the world is being thoroughly drawn to it, the abuse by persons experimenting will be really very small; I should think so at least.

998. Do you speak after a thorough study of this handbook edited by Dr. Sanderson, or only with a general knowledge of it ?—I have not thoroughly studied that book, because I am not at work in that department.

999. It seems to me that a very large class of the experiments of this kind in this handbook are purely demonstrative; that they are intended to be repeated by students, and are actually repeated by students in most of the laboratories where this handbook is used ?—I quite agree to that. I have already said that on the continent this practice of experiments upon living animals is carried to a very great extent; and also that, as Mr. Erichsen has put it to me, the practice has grown up and is growing up in this country among teachers, partly in consequence of the additional knowledge which we have got, and the additional zeal of an increasing number of persons whose vocation it is to study living beings; and I illustrate that by saying that I have no doubt that there are many persons who look upon living beings merely as a form of matter in a particular kind of operation, and who simply consider it their business to investigate them, and learn anything they can about them. That is a new phase of modern thought and a comparatively new phase of investigation, one which leads us necessarily into very serious conceptions and questions of

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the nature of our whole being and moral responsibilities. It is to me part of a great moral and intellectual question bearing on the very foundation of human society.

1000. I understood you to say that you did not object to the Anatomy Acts; and they are only necessary for the protection of the feelings of relations, I suppose; they do not really prevent any kind of suffering, except to the feelings of the relatives?—Quite so.

1001. Surely then, if you do not object to the Anatomy Acts, you would be favourable to Acts which gave you the same kind of security on behalf of creatures which suffer in a way which dead bodies cannot do?—I think the cases are very different. The Anatomy Act was necessary in order to provide subjects for dissection for the ordinary education of medical students.

1002. But surely you would take the case for an Act in this matter as stronger than the case for the Anatomy Acts, so far as those Anatomy Acts were to protect the feelings of the living, and not simply to procure subjects for the anatomists?—I think that the whole answer I can give to that question is this: that if the case is made out that there is wilful and wanton and unnecessary cruelty on the part of the scientific teachers, then a case is made out for abating that. That I entirely agree to. But whether the way to abate that is by putting these eminent and cultivated persons under the control of the law in an ordinary way, or whether or not the Royal Society or other persons should have their attention drawn to it, with a view to their considering in what way these studies should be controlled, is another question; and I did not feel it to be desirable that I should express a definite opinion about it.

1003. (*Lord Winmarleigh.*) You leave that to us?—Yes, I feel it to be my duty to leave it to the Commission. I believe that it is a very good method which is adopted by the British Association, to place money grants in the hands of a committee, to be employed by particular persons for particular objects; and *mutatis mutandis* I could well imagine, at all events with any public grants, that some such method as that should be adopted; because money is voted at the present moment out of the public funds for purposes of this kind, or at all events allied purposes.

1004. (*Mr. Hutton.*) That does not suggest restriction, does it, but only fresh facilities?—Yes.

1005. With reference to Sir Benjamin Brodie's experiments, were not those experiments on dividing the vagus nerves both painful and fruitless; I mean those experiments with regard to digestion. There is an account of them given in a pamphlet before me, which gives the impression that they were both very painful experiments and quite fruitless?—Without referring to them in detail I could not give a scientific answer to that question, but I would say at once that supposing that were to turn out to be the case, that would be no objection whatever to such experiments in the hands of a really scientific man like Sir Benjamin Brodie, because it is clear in the nature of the case that an experiment which is to decide upon a point may sometimes decide that in that direction progress is barred, and nothing more can be done. It is rather a remarkable thing with regard to all great scientific men, that they very seldom have undertaken investigations which have not come to a successful issue. There is a peculiar instinct in the minds of men like John Hunter, or Sir Humphry Davy, or Sir Benjamin Brodie, or any great investigator in any department of science, which so guides them that they very seldom begin a research which does not come to a useful issue; they have an instinct which tells them just the point which is capable of further devolopment, and you will find that failures are very rare in their case; and that is one reason why inferior people are useless, because they have not that instinct and they do not know the precise point of science which is capable of further development, and so they waste their own time and other people's too.

1006. (*Chairman.*) And inflict purposeless suffering?—Suffering which produces no result.

1007. And suffering which a greater amount of knowledge on their part would have prevented?—Yes.

1008. (*Mr. Hutton.*) Do you not think very able men indeed, prosecuting experiments with a purely scientific end like Dr. Klein, would be apt to suggest a great many experiments which would be quite fruitless?—Fruitless very likely for the purpose of alleviating human misery, but not fruitless for the object of clearing up a definite point of knowledge.

The witness withdrew.

Sir W. Fergusson, Bart., F.R.S.

Sir William Fergusson, Bart., F.R.S., called in and examined.

1009. (*Chairman.*) You are Sergeant Surgeon to the Queen?—Yes.

1010. And surgeon to King's College Hospital?—Yes.

1011. And a Fellow of the Royal Society?—Yes.

1012. Has your attention been drawn to the subject which this Commission has been appointed to consider?—Yes, it has.

1013. Have you had much personal experience about it?—A great deal in my earlier life.

1014. Not so much latterly?—Not so much latterly.

1015. What is your opinion generally speaking upon the subject as to its neccessity or utility?—I have an idea that a great deal that has been done has been of service in regard to physiology and human knowledge, but that it has not been of that immense value to human nature that some claim for it.

1016. Are you of opinion that there has been recently a great development of the taste for this sort of investigation?—Yes; I am impressed with that opinion.

1017. And are you of opinion that that taste has been accompanied by a corresponding amount of utility?—No; I doubt if there has been a corresponding amount of utility.

1018. Now are experiments which involve suffering carried to a greater extent than they need be?—I think so.

1019. In what respects?—There is continued, and, in my estimation, useless repetition. When once a fact, which involves cruelty to lower animals, has been fairly recognised and accepted, it seems to me that there is no necessity for a continued repetition of experiments to display that fact.

1020. When an experiment has once decided a question, you think it should be left there?—Yes; unless there is some good reason for reviving the subject.

1021. Unless there is some reason to think that some new element has come under view which requires a new experiment to solve it?—Quite so.

1022. Now with regard to those new experiments, is it a matter for considerable judgment whether they ought to be tried or not?—Yes; I think it requires a very high style of judgment to do a rational experiment under such circumstances.

1023. And that therefore the number of persons who ought to perform such experiments is at any rate very limited?—Comparatively limited.

1024. Can you give us any instances in surgical history which would illustrate these positions?—Such instances as I can think of seem to me to have been after the fact more than prior to the fact. Some of the most striking experiments that have been performed on the lower animals with reference to surgery have really been already performed, not experimentally, but on the best judgment, on the human subject, and proved on the human subject; and therefore there is

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scarcely any necessity for the repetition of such operations on the lower animals to prove the fact. In recent times there has been more said and written to catch the public mind than there used to be on the subject; and I have observed that frequently certain operations in surgery have been referred to as having been developed in consequence of experiments performed on the lower animals. Now, John Hunter, who was one of our greatest physiologists, and allowed to be one of our greatest surgeons also, and may be said to this day to stand at the head of what is called scientific surgery in this country, is specially celebrated for an operation which he devised on the arteries. That operation for 60 or 80 years stood as one of the most brilliant in surgery; and in so far as I have been able to make out (and I have inquired into the subject), Hunter's first experiment, if it might so be called, was done on the human subject, and it was long after he had repeated his operation on the human subject and others had repeated it, that the fashion of tying arteries on the lower animals originated or was developed. That fashion was quite justifiable at the time; it is no longer now justifiable; but in regard to the surgical aspect of the case, the experiment might have been left entirely untouched, for Hunter had already experimented and developed the fact on the human subject.

1025. Then in short in this particular case the experiments that were tried on living animals did not establish the fact; they were only useful, if at all, for illustrating it *à posteriori*?—Quite so.

1026. Now have the great performers of these painful experiments on animals been generally great surgeons? — No, I am not aware of any great surgeons having been very great experimenters on the lower animals. In this country we think that the experiments which Sir Astley Cooper performed on the lower animals were interesting; but they were some of these very operations which I have referrred to, after the development of the operation on the human subject. Of course he is one of those great surgeons who did perform such operations satisfactorily for the time, but I am not aware that any very expert operator on the lower animals has made himself thereby an expert operator on the human subject; nor am I aware that a great operator on the human subject has ever prided himself on being a good operator on the lower animals.

1027. We have heard the name of a very celebrated surgeon, Mr. Syme; what was his practice?—My recollection of Mr. Syme's scientific investigations leads me to think that his operations on the lower animals were chiefly on dogs and rabbits, to ascertain certain views regarding the life and death and growth of bone; he used to perform a number of operations of that kind, but though the detail of them was interesting enough at the time, I am very doubtful now whether these experiments had any special beneficial influence on the practice of surgery.

1028. Mr. Syme was himself very unwilling to perform these experiments, was he not?—He lived to express an abhorrence of such operations, at all events if they were not useful. I think at the time he performed them he was willing enough, because he thought that he had a great object in view.

1029. But his ultimate authority was strongly on the other side?—Strongly on the other side, as expressed in a special report of his own in association with some gentlemen interested in veterinary surgery and physiology.

1030. Have you a copy of that?—Yes, I have got a copy of it. About the year 1867, Mr. Syme and other gentlemen had been asked to give their opinion regarding the subject of vivisection, and there is a report published in the 40th volume of the "Veterinarian," 1867, to this effect: "We have great " satisfaction in publishing the following important " protest: 'We the Court of Examiners for Scotland, " 'of the Royal College of Veterinary Surgeons, " 'desire to express our opinion that the performance " 'of operations on living animals is altogether un" 'necessary and useless for the purpose of causation. " 'James Syme, Chairman, James Dunsmure, M.D., " 'President of the College of Surgeons in Edinburgh, " 'J. Warburton Begbie, M.D.; John Lawson, Pre" 'sident of the Royal College of Veterinary Sur" 'geons; B. Cartledge, M.R.C.V.S., Member of Coun" 'cil of R.C.V.S.; William Cockburn, M.R.C.V.S.; " 'William Robertson, M.R.C.V.S.; Charles Secker, " 'M.R.C.V.S.; James Cowie, M.R.C.V.S. I fully " 'concur in the above, John Wilkinson, Principal " 'Veterinary Surgeon to the Forces.'" No man perhaps has ever had more experience on the human subject than Mr. Syme, and I believe, from knowledge that every man is acquainted with, that he investigated by experiments on living lower animals, partly with a view of developing features in reference to the human subject, but more in fact with regard to physiology than with regard to practical surgery, and I myself have a strong opinion that such an expression coming from Mr. Syme (and he must have passed the middle period of life at that time), was a mature and valuable opinion.

1031. Did I rightly understand from you just now that your own opinion in mature life was much less favourable to these experiments than it was when you were young?—Yes, because I had not the same grasp of the subject at that time. I was more perhaps influenced by what other people had done, and by the wish to come up to what they had done in regard to such matters, but the more matured judgment of recent years has led me to say to myself now that I would not perform some of the operations at this present time that I performed myself in earlier days.

1032. Do you think that these experiments on the lower animals have contributed very much to mitigating the pain and removing the suffering of the human race?—I do not think that they have. I think they have been of great value in many respects, but certainly I cannot think that they have led to the mitigation of pain in the human subject.

1033. But that in point of fact they have been much more pursued by persons who are not practical surgeons than by persons who are?—Decidedly. Even with reference to the subject of chloroform the best and chief experiments were made on the human subject; all the experiments on the lower animals have been done since the experiments were conclusively applied to the human subject.

1034. When chloroform was introduced by Sir James Simpson he tried many of the experiments on himself, did he not?—Yes, he did when he was trying the effects of chloroform. Of course anæsthesia was devised prior to that time.

1035. Are you able to tell us whether there is reason to believe that experiments upon animals, on the supposition that they may be necessary, are performed with all the care and all the regard to the sufferings of the animals that there ought to be?—I could not give a precise answer to that; but the impression on my mind is that these experiments are done very frequently in a most reckless manner.

1036. In a manner that, if it were known to the public at large, would call for interference on their part?—Yes, and would bring the reputation of certain scientific men far below what it should be.

1037. We have been told that, speaking generally, experiments of this kind are performed with the greatest possible consideration for the animal, and with the greatest indisposition to inflict at least protracted suffering. Do you believe that to be the case?—Gentlemen may fancy that, but I do not think that they fulfil that idea. Indeed I have reason to imagine that such sufferings incidental to such operations are protracted in a very shocking manner. I will give you an illustration of an animal being crucified for several days perhaps; introduced several times into a lecture room for the class to see how the experiment was going on.

1038. Do you believe that to be done not only on the continent of Europe but also in this country?—

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I believe it to be done in this country, from what I have heard.

1039. (*Mr. Erichsen.*) For what purpose?—Some physiological experiments which the teacher might be interested in at the time.

1040. (*Chairman.*) You think that if the public really knew what was actually going on in this country at this time they would expect an interference on the part of the Crown and Parliament?—I do think so, just as much as with reference to the disinterring of dead bodies years ago.

1041. Have you got in your mind any particular mode in which you think the Crown and Parliament could usefully interfere?—In a very vague way certainly. I think it could only be done by a judicious representation to men engaged in science that they must be careful how they resort to such experiments, and how they encourage the performance of them by those who are younger and less informed than themselves.

1042. You have just now referred to the subject of anatomy. Do you think that any interference with experiments upon living animals of the nature of that which was introduced in regard to anatomy could be usefully applied?—I believe there might be some sensible jurisdiction, but it would be a difficult thing to point out one to please all parties.

1043. But without attempting that impossibility, do you see your way to some interference with these experiments?—I think that an expression of some views from the Government and from the Houses of Parliament would have a very wholesome effect; that a man would then very likely run the risk of losing caste and character if he went beyond certain bounds.

1044. For that purpose it would be necessary, would it not, that there should be publicity?—Publicity is much better than privacy in regard to these matters.

1045. Would you see any objection to the application to this subject of the same general enactment as has been applied to anatomy?—It would be a much more difficult subject to legislate upon; the field is so extensive. There is a large amount of thought associated with the matter, giving rise to features that we cannot connect with the mere taking bodies from the grave and dissecting them. Men of a very superior stamp might have thoughts in their minds, and the desire to develope these thoughts by experiments that really would be of great value to the human subject, and it would be I think a great pity that such a man should be entirely trammelled. But if you had reason to think that a man of enthusiastic mind was indulging too freely in these thoughts in that way, it might be well to bring him to his senses, to show him that he was only working in ignorance as it were.

1046. Should I rightly represent your views if I said that you think there is some great need for a remedy; but that the application of that remedy is difficult?—Yes.

1047. It would require therefore great consideration and care?—Yes.

1048. Nevertheless, that if the public knew all that is to be known on the subject, they would be likely to demand some remedy?—Yes; I think they would.

1049. (*Lord Winmarleigh.*) You have stated that you consider that experiments involving cruelty to animals have been too frequent, and that they have not led to the mitigation of pain, generally speaking; but I presume you did not mean to say that they have not led to the successful treatment of complaints, or the mitigation of human suffering at all?—With reference to that I may perhaps speak more confidently regarding surgery than other departments in my own profession, and in surgery I am not aware of any of these experiments on the lower animals having led to the mitigation of pain or to improvement as regards surgical details.

1050. But we have had statements here by a very high medical authority that there were several branches of medical science which have been entirely ascertained by means of these experiments. We have had brought to our attention, for instance, the circulation of the blood, and the action of the heart?—Of course everyone admits those; but then that is a bygone thing.

1051. And on the liver and the kidneys and the nerves, we have been assured here that very great knowledge has been obtained, which could not possibly have been obtained except by vivisection?—Well, I suppose we must admit that; but I have no very strong impression on my mind as to the great value of some of these experiments. I do not think, referring to the most recent experiments that have been made, that you can form a very accurate opinion as to the actions of nature from looking through the walls of the abdomen at the liver. You get a window cut in the side of the abdomen, and get the gall bladder laid open, and you look at it, or a physiologist looks at it, and watches it a certain number of hours or days; but the animal is put into such an extraordinary condition by all that has been done, that I cannot say that I have very great confidence in the results of such an experiment.

1052. But do you think that the same knowledge would be obtained by operations on the human body?—No, certainly not; but a great distinction should be drawn between the two, I think. Very often certain operations are performed on the lower animals, and these experiments are used in a way that they never would be used in the human subject.

1053. (*Mr. Forster.*) What you meant by your answer I suppose was, that the abnormal conditions of the experiment in which the animal was put, made it very doubtful what was the advantage of the experiment?—Exactly. The opinion in my mind with regard to that might be illustrated from what we have all felt. At the beginning of the week you have felt as well as you have ever done in your life; at the end of the week you are knocked down by some kind of fever; and then every function in your body is disturbed, and there is nothing in accordance with normal or healthy nature. Now for my own part I say that an experiment performed on my body at the beginning of the week, and one performed at the end of the week, under such circumstances, would be totally different and would lead to different results. There would not be that kind of precision in the results that ought to be considered essential, unless indeed you are experimenting to ascertain the difference between health and disease.

1054. (*Lord Winmarleigh.*) With the opinion that you hold on the subject, would you think it safe to abolish altogether experiments on living animals with a view to obtaining thorough surgical or medical knowledge?—No, I certainly would not go that length of restraining rational men from doing that which they thought right; but I would enjoin great caution.

1055. Supposing that this Commission was to recommend certain restrictions upon these experiments, could you suggest to us any mode by which you could distinguish useful and necessary experiments from those which you think involve cruelty, and which are of no use to the human race?—In general terms I have done that already; but in regard to other ways, I think you must still leave a margin for a man's judgment, and if he chooses to display very bad judgment he must just suffer as other men do who display very bad judgment in all the ordinary transactions of life. But it would be well that there should be some kind of superintendence, such as is indicated by you; let the man know that if he goes to excess he will fall into a low caste.

1056. That is to say, you would leave it to the impression made on the public mind, and not to any legislative enactment. Does not your suggestion amount to that?—Yes; that is a fair view of it. I might give you an illustration of that. Perhaps it might be disputed, but my own idea is (and Mr.

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Erichsen, perhaps, would confirm me in that view), that so far as the treatment of human beings in hospitals is concerned (I say it with all respect to continental practitioners), there is, so to say, more humanity and kindness displayed in English hospitals than there is in hospitals abroad, in certain countries abroad at all events; and that I attribute largely to the circumstance that every man practising in an English hospital is largely amenable to the observation and opinions of those who take interest in these hospitals; whereas in a foreign hospital, so far as I can understand, the man is himself the sole arbiter. He may do what he chooses and there is nobody to interfere, and it will not injure his position in social life or practice; whereas if a man in this country were to get a character for roughness and rudeness, and want of courtesy to his patients, to a certainty that man would soon feel that he had lost caste in the general community, and losing caste he should not have that position that he otherwise should.

1057. The object of this Commission is first of all to ascertain what is the extent to which what is called vivisection is carried on in this country; the second object is to ascertain what is the way in which it should be treated in the future. Now I collect from what you have just stated to us, that you are opposed to legislation on the subject. Is that so? And would you prefer to leave it to the influences which you have just mentioned?—There might be, I think, an opinion expressed on the subject by the great authorities in the country, but I should be very reluctant to coerce scientific men to give up their investigations. They are considered of very much importance by many.

1058. You would not, for instance, recommend that any particular officer should be directed to attend on every occasion when such experiments were made?—No, I think it would be a most offensive thing to men of science in this country that that should be done.

1059. You would be satisfied then if this Commission were to express its opinion very strongly on the subject, and leave it for the consideration of the public?—That would come up to my views and feelings on the subject.

1060. (*Mr. Forster.*) The proposition has been made that the legislation should bring in the principle of licensing, that is to say, licensing men who are supposed to be qualified for making experiments, to try them. Has that suggestion come before you?—I have heard it.

1061. What do you think about it?—It is very questionable.

1062. When you say that high authorities should express an opinion on the subject, you mean the Legislature?—Yes, or the Government.

1063. But you think that it should be a mere opinion, without attaching any penalty to persons who went contrary to that opinion?—I do not see my way to a penalty, excepting that penalty that I have referred to again and again,—that a man must suffer in public estimation according to his acts and deeds.

1064. Is it not the case that a great many of these experiments are tried by men of science in their own apartments?—I believe so.

1065. Then public opinion would not reach those men, would it?—Yes, it would have its effect in a short time.

1066. You think it would be known that they did try the experiments?—Yes; of course there is the Society for the Suppression of Cruelty to Animals, they would interfere; just in the same way as when a person ill-uses one of his own family, if that goes on largely it is found out by the public at last, and the person suffers accordingly.

1067. Are you aware that the present Cruelty to Animals Act is not supposed to apply to these experiments because of the definition of "animal?"—Yes, I am aware of that feature.

1068. Do you see any objection to enlarging that definition, so as to include wild animals?—It would be a great advantage I think to do so.

1069. So as to throw, as it were, the *onus probandi* on the man to show that he had a proper object in the experiment?—Quite so, and to make him aware that the life of the wild animal is as precious to itself as the life of the domestic animal.

1070. The fact that that might in some degree hit the sportsman would not be an argument against it, you think?—No, that is the poor device of some people to stop all these inquiries; I do not think it is an argument at all.

1071. You have studied anæsthetics a great deal, I suppose?—Yes.

1072. Do you know anything of this Wourali? Do you consider it an anæsthetic, or not?—Forty years ago I used to use it frequently as a poison.

1073. Do you think it is an anæsthetic?—I am not aware that we have the smallest proof to that effect.

1074. My reason for asking that is that in this handbook of physiology there are a great many experiments described, generally speaking, at least often they are suggested to be made, under the influence of this Wourali. You would not consider that that could be depended on as an anæsthetic?—Certainly not. My recollection is to the effect that it generally killed outright, and very speedily.

1075. I suppose the fact that it is in itself a poison prevents the question being solved whether it is an anæsthetic or not, in the same way as in the case of chloroform, because no human subject ought to take it?—It has never been used or put before the profession in such a way that it could be used on the human subject as an anæsthetic, so far as I know. The way in which it was tried 40 years ago was to put a little of it under the skin of a rabbit and see the rabbit die in a minute or two.

1076. And you never saw it applied to the human subject?—No; I have never known it used for the human subject.

1077. We are told that the reason why it is recommended in experiments is not that it makes the animal insensible to pain, but that it makes the animal still and quiet. Do you know whether that is the case?—I should doubt very much its making the animal insensible. I do not know it of my own knowledge, but I have very strong ideas with reference to these experiments performed under anæsthesia as being far less valuable. I do not go in with that view, which is very prevalent, that these experiments may now be permitted because we have got anæsthesia to prevent the pain. The experiment is not of the smallest value during its performance. You cannot make a perfect experiment on the animal until it is in its normal condition.

1078. That does not imply that you do not think that the animal is made as insensible to pain as the human subject would be?—I fancy so. I do not think that there is much difference.

1079. What you mean is that if a man tries his experiment of course he hopes it will be a successful one; whereas you think that the anæsthetic may so derange the animal as to prevent its being successful?—It would be difficult for them to see what they want to see under anæsthesia, because the animal is no longer itself. An experiment on the human subject, for example, to whom you have given an anæsthetic, chloroform say, goes this length,—that the person is rendered insensible, and you may do any kind of painful thing to that individual for the time. That proves what I say. But further than that the anæsthetic has no other value, because when a person, having undergone an ordinary surgical operation recovers from it, then he suffers just the same in every respect as if he had not had chloroform at all during the performance of the operation.

1080. In the after suffering you mean?—In the after suffering; and there I think there is a great weakness on the part of those who try to make it appear that vivisection of the lower animals may now be more readily done than it could be before,

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because an experiment at the time of an animal being insensible is really of little or no value.

1081. Several of the witnesses before us have said that they think that anæsthetics ought to be employed wherever they could, and that the animal should be killed before it woke up to sensation. What have you to say to that?—I myself cannot understand these experiments as a surgeon; I do not see what value they can be of at all.

1082. You think that the fact that the animal was under at least the temporary influence of a powerful poison would throw great doubt upon the whole nature of the experiment?—Upon the value of the experiment I think it would. I have no doubt whatever that in the case of a rabbit or a dog you could make it so insensible that you might cut out all its bowels, and the animal would never express pain by any mental or physical indication. Then of course you know what would happen after that.

1083. As far as regards an experiment for merely enabling an operator to do an operation, that is to say, for a surgeon, you would say that such an experiment would be of very little use, or that it would be very doubtful whether it would be of use?—I think this is an instance which I would put at once to any man who is trying an experiment of that kind intended to be applied to the human subject, because we have had more experiments performed on the human subject than have ever been performed on the lower animals during anæsthesia.

1084. But that objection of yours to that kind of experiments would hardly apply, would it, to experiments that were physiological, such as the discovery of the circulation of the blood, because these are experiments to see the actual process within the human body?—That I think is right enough; but I do not see that it is applicable; moreover that is a wrought out subject.

1085. But am I right in rather gathering this from your evidence, that although you would not say that no future experiments of this kind might be useful, or even under certain circumstances necessary, yet you attach less value to them than many other members of your profession, for two reasons; first, because the actual experiment itself puts the animal into an abnormal condition; secondly, that anæsthetics when used, would in addition put the animal into an abnormal condition?—I have no strong ideas about the experiment itself apart from anæsthesia, though in some instances I believe dogs laid hold of suddenly and violently and pinned down on a board, get into such a violent and excited state that you have not the animal in its normal condition.

1086. (*Mr. Erichsen.*) In reference to a remark that you have just made, I will ask you a question, because it is novel to me. Is it the case that dogs are ever "pinned" to boards, or that animals are crucified, as you said in another part of your evidence?—I have not myself seen it, but I have heard of it frequently, and I have had brought under my notice in the last nine months the case of two dogs being strapped to boards (I meant pinned to them in that sense) at Norwich, and a surgeon went up and cut the straps so as to let the creatures have some kind of relief. Let me also draw your attention to this,—you know that frogs are not strapped; they are pinned and tied; pinned more frequently than in any other way.

1087. With regard to surgical experiments, you have had immense experience, as we all know, not only in surgery itself, but also in the education of young surgeons, and in watching the process of many surgeons through life. Is it your experience or not that surgeons perform operations upon living animals with the view of accustoming themselves to operations, so as to acquire dexterity in operations on the human subject?—It is not my experience. I have heard it said that it might be done.

1088. But it is not your experience that it is so done in this country?—Certainly not.

1089. I gather from you that it is not the habit of surgeons to practise vivisection for the purpose of acquiring dexterity upon human beings?—Certainly not in this country.

1090. And that it would be useless to do so, because the condition of the tissues is very different?—Yes; the surroundings are so very different that I should place no confidence in any man who acquired additional experience in that way.

1091. A great many of the most eminent surgeons in this country, Sir Charles Bell, Sir Astley Cooper, Sir Benjamin Brodie, Mr. Travers, Mr. Lawrence, and others, have all performed experiments of various kinds upon animals; these have been to elucidate a certain definite point, and in the hope of adding something or other to the general stock of human knowledge?—They have also been performed by gentlemen, such as these, to aid in the development of special views of their own, and, so far as I have been able to judge, very legitimately.

1092. You stated, I think, that operations in surgery were not usually first of all performed upon animals, and then upon the human subject?—Yes; I have a very strong impression to that effect. I have thought over it again and again, and have not been able to come to a conclusion in my own mind that there is any single operation in surgery which has been initiated by the performance of something like it on the lower animals.

1093. The operations that have been performed on the lower animals, with a view of elucidating similar procedures upon the human subject, have all been performed after those operations had been done on the human subject?—Invariably. Whatever has been done after is a matter of curiosity.

1094. We are told that some of these experiments have thrown great light upon practical surgery, as, for instance, Mr. Jones' experiments upon the kind of ligature to use upon the arteries; that before his time John Hunter used broad ligatures, and that bad results followed the use of those ligatures, and that the experiments performed by Mr. Jones threw great light upon the proper sort of ligature to use, and were consequently of use to surgeons. What is your view as to that?—Jones' experiments were of considerable value to surgery at that particular time; but they have all been done again and again.

1095. And it would not be proper to repeat them?—Certainly not.

1096. That is a past matter; but at the time they were of great service to surgery?—Up to this day, I may say with regard to that point, practical surgeons seem to have as much difference of opinion as to the best kind of ligature as they had in Jones's day.

1097. Experiments have been made of late years on that very point by Mr. Lister of Edinburgh?—Yes, and by many others.

1098. (*Mr. Forster.*) Experiments on animals or on the human subject do you mean?—On animals and on the human subject; experiments are going on on the human subject daily just now; but these are rational experiments, quite legitimate.

1099. (*Mr. Erichsen.*) Experiments on the human subject, that is to say, after the operation; one surgeon uses one kind of ligature and another another?—Yes; it is the object of the experiments to ascertain what is best. But in saying that I do not mean to say that it is a reckless operation as to human life.

1100. I am not at this moment speaking of the indirect influence that experimentation on the lower animals has on the progress of surgery, but we have been told that certain observations, such as those which are made in what may be termed pathological experimentation, the production of inflammation for instance in the web of a frog's foot, and so forth, have had a very direct bearing (and no doubt they have) upon the progress of medical science. Have such experiments, in your opinion had any direct bearing upon the progress of surgical practice?—Well; I do not think it. I am as familiar as most people with these

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experiments, and I cannot say that I have been much impressed with the value of them.

1101. Has your own practice been very much influenced in any way whatever by what has been observed in this way in the lower animals; or has it been the result of your own clinical and pathological observation on the human subject?—Chiefly the latter; and since I have been indoctrinated with the usual views that are held in this country, I cannot say that I have had my mind opened up by experiments to any increase of knowledge on this subject.

1102. I suppose we may say this, that medicine is based upon physiological, but also upon clinical observation and pathology?—Yes.

1103. And in your opinion is clinical observation and pathological observation of more service to practical surgery than experimental physiology?—Yes; there is a precision in the one, whilst the other is largely theoretical.

1104. (*Mr. Hutton.*) Have you seen anything of the experiments for the transfusion of blood to which Sir James Paget referred in his evidence?—I think I may have seen once or twice the transfusion of blood from one human being to another, and I have known it in my experience again and again.

1105. Can you say whether the experiments on animals on that subject were in your opinion essential to the determining of the proper conditions for that transfusion?—I do not think they would be of the smallest practical value.

1106. You were a witness, were you not, of that experiment at Norwich which has been alluded to?—No, I am happy to say I was not; I was a witness in the trial.

1107. But in your opinion the attempt to determine the effect of absinthe and alcohol on the stomach by transfusing it into the veins of an animal was a perfectly useless experiment? — I think it was grounded upon incorrect views altogether. There is no strict analogy between the two, particularly when you want to put that analogy in association with a similar thing in the human body.

1108. And did I rightly understand you to say that Mr. Jones' experiments on the ligatures had not determined the question at all of the kind of ligature best suited for the arteries?—That is so; the controversy has been going on ever since, and variety of opinion still exists.

1109. Experiments on the human body are much more efficient for the purpose than experiments on the arteries of animals?—The ordinary operations, I would hardly call them experiments, are much more efficient; all such experiments have been founded on reason, and without any risk to the human being.

1110. Do you know anything of this handbook of Dr. Sanderson's?—I have heard a good deal about it, but I have purposely rather avoided it.

1111. You cannot give any opinion on the experiments described in it?—I have heard something about them; I fancy a large number of them are such as ought not to be sanctioned.

1112. (*Sir John Karslake.*) Would you let me know what are the grounds upon which you state the opinion that there is a great deal of reckless practice of vivisection going on at the present time?—I hear young men who are pupils, or have recently been pupils, speaking of what they have seen in the theatres, lecture rooms, and laboratories of those who profess to teach physiology.

1113. Could you point out to the Commission particular theatres or lecture rooms or laboratories in which you have heard that these practices prevail? —I could not be precise.

1114. Could you give us some clue by which we could get evidence for the purpose of ascertaining to what extent they prevail?—I can only do it in very general terms. I should recommend inquiries to be made at any of the physiological institutions in London and elsewhere.

1115. It is from what you have heard of what goes on at those theatres and laboratories that you have formed the opinion that there is a great deal of reckless practice of vivisection prevalent at the present time?—Yes; and I might have seen it in the papers too. In some of the Edinburgh papers recently there has been a letter from "A Citizen of Edinburgh," in which he questions the propriety of the system by which, when they are building a new university in Edinburgh, it should appear conspicuously that very careful provisions are being made for the lower animals that are to be kept in the institution for the purpose of performing experiments upon them. I think that is proof enough to show the estimation in which in certain quarters that style of practice is held.

1116. My question was rather pointed to the prevalence of the practice. and the sources from which we could ascertain whether it does prevail, and if so, to what extent?—I think the best authority that I can refer you to is the best and largest of the teaching institutions in London.

1117. (*Chairman.*) Your opinion is that the value of these experiments has been altogether exaggerated? —I do hold that opinion.

1118. But that if in the hands of the principal scientific men in this country, public opinion would be a sufficient check upon them?—Yes, that is my impression.

1119. And that the notion of being generally reputed to be very indifferent to suffering would be such as to prevent any of them continuing so or becoming so, even if they would otherwise have been disposed to become so?—Yes; I think public opinion would have a large influence upon the practices of such men.

1120. For that purpose it would be necessary, would it not, that something should be known about the proceedings that were going on in the different theatres and lecture rooms?—It would certainly be beneficial to your investigation that you should know that.

1121. But for the influence of public opinion upon scientific men, it would be necessary that public opinion should have something to form itself upon, would it not?—Yes.

1122. That is to say, unless the public knows what is going on, it can neither form nor express any opinion upon it?—Quite so.

1123. And your opinion, as I understood you, is that at this very moment there is a great deal going on in this town which, if the public did know, would excite them very much?—That is my impression from what I hear.

1124. And public opinion is in the present state of the law and practice of the country quite unable to bring itself to bear upon it?—Yes.

1125. Something therefore must be done in order to enable that public opinion to produce the effect that you speak of, must it not?—Possibly you gentlemen might bring before you some of the most intelligent of, say the young members of the profession, or the senior students in the profession, who have recently been attending lectures and demonstrations on physiology, and from them perhaps you would get the information.

1126. But I want to draw your attention to a different point. You have said that something requires to be done to check what you consider an amount of cruelty now going on?—Yes.

1127. But when pressed upon the subject as to the particular mode of doing that, you expressed a hope that public opinion would have a sufficient influence to bring about that result?—Yes.

1128. Then what I have pointed out to you is that under the present state of our law and of our practice these things appear to be going on, and yet public opinion appears to have no opportunity of operating because it does not know what is going on?—Yes,

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1129. It will therefore, according to that view, be a consideration for this Commission, whether they should not recommend some change in the law and practice, by which everybody who does these things shall hereafter be subject to some influence of public opinion; is not that so?—Yes.

1130. That would point to some change in the law?—At all events, to an expression of opinion from some of the highest sources in the kingdom.

1131. But an expression of opinion which should not be brought to bear upon the practice of a person who is permitted to continue the practice without the knowledge of the public would be inoperative, would it not?—We have already in some degree referred to that. First of all I would suggest that if you gentlemen were to have information sufficient to convince you that such practices are going on to an extent that you do not think should be permitted, it would be for you to recommend to the Government to either say or do something on the subject. Then I think I would say as to the next part of your question that in the present state of the law we have the Society for the Suppression of Cruelty to Animals keeping a constant watch upon such matters, and a person amenable to the law at this present time for cruelty to animals would be still more likely, I think, to be put in the right course, or to have his practices suppressed altogether, if it were done under stronger sanction than there is at this present time. There is the common law at this present time, but we should have some additional expression indicating that Government could not tolerate this sort of thing in this country.

1132. At the present moment there is, according to your belief, much going on somehow or other which ought not to go on?—That is my impression.

1133. And that probably the law and the practice, as it stands at present, is not sufficient to prevent it?—It does not reach it at the present time.

1134. And that which you would recommend would be, I suppose, that some change in our law and practice should be made which would enable us to reach it?—Quite so.

1135. And then, so far as the most eminent scientific men are concerned, you think that after that had been done, public opinion would have sufficient influence with them?—The fear of an unfavourable public opinion, I think, would have great influence with them.

1136. Then with regard to other persons than those eminent scientific people, it might, perhaps, be necessary for some severer measures to be taken with regard to them, might it not?—Yes; if there was excessive cruelty or an excessive want of common humanity.

1137. Supposing a comparatively ignorant person, from whose experiments no real good could reasonably be expected, were nevertheless to practice such experiments; do not you think that something stronger than public opinion might be brought to bear upon him?—I think there would be some difficulty in applying that to people in the position of quacks and bone-setters, and so forth; because you might arouse in the mind of the public the feeling that the man was a persecuted man, that the whole profession had gone against him, and you might make rather a martyr of him than otherwise, and probably an elevated martyr.

1138. In fact, you would suggest that if the Commission did recommend any further proceedings, they should take care not to exceed the limits of public opinion, not to go beyond what public opinion might be expected to support?—Yes. I think when you are referring to that subject, I may say that it has struck me that it would be well, with a view to what may be the result of this investigation which you are engaged in, if the attention of the governors of medical schools were called more forcibly to the subject than at this present time; that there might be certain governors in each school who should take an interest in the matter, and see that there was no unnecessary cruelty in a part of the institution where it is admitted generally that there must be a certain amount of cruelty for special purposes.

1139. But that is limited to the great hospitals and scientific schools, is it not?—Yes; it would be limited to them; but I do not think there is much of this that goes on irrespectively of these schools. I do not think that there is much amateur physiology going on in this country.

1140. But if there was, it would be still more necessary, would it not, to provide a remedy in that case?—Yes; if that was ascertained, it would be a very important thing.

1141. (*Mr. Forster.*) I think you said that you think what does go on that ought not to go on is mainly at the large institutions?—So far as I can make out. I believe that here and there there are outsiders, so to say (I speak of them with respect), who are not actually in these institutions; they are zealous and talented men, and take opportunities of their own where they can; and I think it would be a pity to restrain even these men, so long as it was seen that they were under a wholesome influence.

1142. Has it ever occurred to you whether, as regards these large institutions, it might not be well to put them under the obligation to make a public report of the experiments that they were performing?—That has occurred to me. It might be a very good rule to make.

1143. Take for instance what is going on now; we hear different accounts, and it is very difficult to know accurately what is going on, and therefore it is very difficult for public opinion in any reasonable manner to operate upon them; but supposing they were under an obligation to declare what they had done, and why they had done it, then public opinion could form, could it not, a reasonable judgment?—I think that in legislation on the subject it would be a very important thing to have some view of that kind developed. It had a very wholesome effect on the conduct of dissecting rooms, for instance. Now there cannot be a subject in a dissecting room without there being a proper account of it, where it has come from, how long it has been there, and so forth.

1144. At the present moment they are compelled to keep an account of dead human bodies?—Yes; and if there is a large piece of a body lying there that cannot be accounted for, the individual that has that piece, the professor of anatomy, is immediately amenable to the law, and the law is very stringent; he would have his license taken from him, and his school, so far as the teaching of anatomy was concerned, would be put an end to. There is a very wholesome regulation of the practice with reference to that matter.

1145. And in the same manner, I suppose, it would be possible to say that a report must be made day by day of any experiments on a living animal, and why it was done?—At certain dates. Day by day would be perhaps too much officiousness; but I have thought in my own mind that it would be quite possible that you should have an inspector of these experiments, just as they have an inspector of anatomy; and that just as the common law enables the authorities to send detectives in certain directions to ascertain who may be in certain houses, so they should be able to send men who might say: "I should like to see the "number of dogs and rabbits or cats about this "place." I think that would have a very wholesome effect.

1146. (*Chairman.*) There was a great excitement about anatomy, we are told, (and many of us are old enough to remember it,) when the law was in its old state?—Yes.

1147. That excitement was entirely put down by the changes which have since been made, and there is no public complaint any more on the subject. Do you think that something analogous to that might be

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adopted if there should be reason to believe that things are done in this matter which would not be satisfactory to the public if they knew of them?—Yes, I think it would be a very wholesome thing to do, if this Commission were satisfied that these practices are carried to an exorbitant extent.

1148. In other words, if they were satisfied that there was sufficient occasion?—Yes.

The witness withdrew.

Adjourned to Wednesday next, at 12 o'clock.

Wednesday, 14th July 1875.

PRESENT:

THE RIGHT HON. VISCOUNT CARDWELL, IN THE CHAIR.

The Right Hon. W. E. FORSTER.
The Right Hon. Sir JOHN KARSLAKE.
JOHN ERIC ERICHSEN, Esq.
RICHARD HOLT HUTTON, Esq.

Mr. N. BAKER, Secretary.

ALFRED SWAINE TAYLOR, M.D., F.R.S., called in and examined.

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1148. (*Chairman.*) You are a Fellow of the Royal College of Physicians and of the Royal Society?—I am.

1149. What is your exact position at Guy's Hospital?—Lecturer on medical jurisprudence and toxicology, an office which I have occupied for 44 years.

1150. You are also the author of the works on Poisons and on Medical jurisprudence so constantly resorted to when questions of that nature occupy the attention of the courts of law?—Yes.

1151. During the time you have mentioned what has been your experience with regard to experiments on living animals?—It has been of a limited kind. I have performed a few occasionally.

1152. Do you mean a few experiments with the knife; or that the whole number of your experiments, including those with poisons, has been a limited one?—In some cases the knife has been used to produce a slight wound for the introduction of poison; that has been the only use of the knife.

1153. And that is not of a painful character?—Not of a painful kind; a very slight wound is sufficient for that purpose.

1154. Have you experimented largely in toxicology?—Yes, I have in reference to poisons.

1155. I think that you were much employed about the trial of William Palmer?—Yes, and indeed I have been employed for the Government for more than 20 years on all great cases, including those of Palmer, Smethurst, and many others that have been tried,—in Palmer's case especially.

1156. Would you relate to us, if you please, your experience in Palmer's case?—In Palmer's case it was necessary to perform some experiments on living animals, as so many conflicting results were reported to have been obtained by experiments performed by witnesses for the defence. Dr. Rees and I on that occasion found it necessary to experiment on, I think, six rabbits.

1157. And in the result, the evidence which you gave led to Palmer's conviction?—It did. It showed that the power of discovering the poison depended on the amount given to the animal, and the time that it lived after taking it; whereas it was said, I may casually remark, that if an animal or a man died from poison, it must always be found in the body. Our object was to see whether we could kill an animal without necessarily finding it. We satisfied ourselves that one of these rabbits at any rate died from a poison which had been introduced by absorption, and that no trace of it could be detected in any way, the quantity which killed it was so small.

1158. Is it your opinion that experiments are resorted to in such cases to a greater extent than is necessary?—I think so; and in that case they were particularly.

1159. Have you tried experiments to determine the influence of the poison of the Cobra di Capello?—Yes, those were the last experiments I performed. They involved some very curious points of physiology. I had a quantity of the dry poison of the Cobra di Capello given to me.

1160. Was that for a question of jurisprudence?—Rather physiology; because serpent poison would hardly come in except in the general way of showing death by absorption. It was therefore rather a physiological than a medico-legal point. I may state that the poison was collected from a Cobra in 1861, and I received it in 1873, 12 years afterwards; it had been kept in a dry state in a tube; and I used a small quantity of it to test the question,—whether the poison remained for that long period unchanged. I found that it did; that it destroyed a rabbit in a quarter of an hour when introduced by a wound. Then there was another important question. It had been long said that the serpent poison would only operate through a wound; that had been recently placed in a little doubt by some experiments; and Dr. Pavy and I determined to test it. We made a dog swallow two grains of the Cobra poison after he had been kept fasting for about 25 or 30 hours (that was necessary to promote absorption); but the animal manifested no symptoms of injury at all; he perfectly recovered, indeed there was no effect; so we let him go, and gave him a good meal; and that clearly satisfied us that this dreadful poison operates chiefly by absorption through a wound, and not through the stomach.

1161. Had it any bearing on any particular case of jurisprudence?—No, not at all; it was a mere point with regard to physiology in reference to the action of serpent poison, and it was done in order to try and settle what had been a contested question.

1162. Can you give us, from your long experience, some instances in which your experiments have had an important bearing upon recent cases of jurisprudence?—Yes. The case to which I am about to allude occurred in Suffolk about 12 or 14 years ago. A woman was charged with poisoning her step child, a little girl; she had applied to the head an ointment of white precipitate mixed with arsenic, to cure the ringworm; the child died from the effects of the poison, and it was charged that the woman had treated her cruelly, that she had deliberately caused her death; and this seemed to be borne out by the fact, that although she said she had applied the poison to the head, it was discovered in the stomach. The experiments performed by Dr. Pavy and myself were these;—to see whether, when poison was introduced into the blood by a wound, or absorbed in any way and circulated, any portion of the poison might find its way into the stomach. I may say that the quantity which I found in this child's stomach was exceedingly small; it was in a dissolved state mixed with the general fluids of the mucous membrane, and there was no appearance of food. Upon performing experiments on rabbits and dogs

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Dr. Pavy and I found that arsenic injected into the blood or through a wound, if the animal lived a few hours, was in part eliminated by the mucous fluids of the stomach. In short, it goes out by the urine, and by all the fluids of the body, and the stomach itself becomes an eliminating medium. The quantity I found was so small that I was able to say that I believed it to be simply the result of elimination from absorbed poison. This inference was borne out by other circumstances. There was no time at which symptoms of arsenic as a poison acting through the stomach had appeared; the symptoms were wholly different; none appeared for two days; and this satisfied my mind that it had not been given in the usual way by the stomach. The child died from the effects of the poison clearly. The jury were satisfied with the evidence given upon my statement, that the discovery in the stomach did not prove necessarily the administration by the mouth, the quantity found being so small that it was consistent with elimination by the fluids of the body. It was rather a novelty to demonstrate by experiment that from the outside of the body, the poison could be carried into the interior of the stomach. That was the object of these experiments, and I think the results had the effect of preventing a verdict of wilful murder from being returned against the woman. The coroner's jury returned a verdict of "Died from poison, but the result of accident;" so it went no further than the coroner's inquest. Up to this time I believe it had not been clearly proved by science that a poison could find its way from the skin to the stomach.

1163. You justify the performing in that case of experiments upon dogs or rabbits by the result which showed the action of the poison of arsenic in this particular case, and saved the woman from being the subject of a criminal trial?—Yes.

1164. Now do you think that there are many cases in which experiments of this nature are performed with benefit to mankind?—Yes, I think there are.

1165. Do you think that there are more performed than there is much necessity for?—I do; and I would say that it has become rather a custom in notorious cases of poisoning, cases which attract great public attention, such as Palmer's and Smethurst's, for many persons to experiment very unnecessarily upon animals for the purpose of working up the prosecution or defence. I have mentioned in a note to your Lordship that in the Palmer case "one medical man" boasted of having operated upon and destroyed 60 animals.

1166. (*Mr. Forster.*) Did he appear as a witness?—He appeared as a witness for the defence; a large number of animals were thus destroyed on the part of the defence.

1167. (*Sir John Karslake.*) Would you add to that what the reasons which he gave for his experiments on these animals were?—I can hardly explain; for the results at which he arrived were only what we knew before, that is that animals might be destroyed very rapidly by strychnia; but the greater number of the experiments, according to my recollection, seem to have been performed by him in order to determine the power of discovering this poison in all cases after death. That was the point. You will remember that in Palmer's case the evidence for the prosecution was greatly interfered with by the mode in which the viscera were handled after being taken from the body. The point dwelt upon for the defence was that poison was not found in the contents of the stomach. But it was not properly made known to the public that Palmer himself was present at the inspection of the body; that the stomach was cut open and the contents, with all the fœcal matter of the intestines and other viscera were thrown into one jar; and then as the poison was believed to have been given in pills, not in powder, it placed the greatest difficulties in the way of its detection; because without examining all the animal matter we should have found nothing—we found nothing in the stomach to account for death.

1168. (*Chairman.*) You think that a limited number of experiments was necessary, but that a very unnecessary number were resorted to?—I think so on all sides. There were a great number of witnesses who appeared on the occasion for the defence, and they seemed to rest their case greatly upon the number of experiments, and indeed they put it that what Dr. Rees and myself did, namely, in four or six experiments only, were not to be relied upon. The late Mr. Serjeant Shee dwelt upon this as a strong argument, that we had worked so little that we really had no *locus standi* there to put ourselves forward to give an opinion—we were put down actually by the number of experiments performed on the other side. So far you see that experiments upon animals may be carried to an unnecessary extent for the purpose of a defence, for after all, a hundred experiments in which the poison was found, would not overthrow a few in which it could not be found, the results varying according to the circumstances.

1169. Your opinion is that, when once a thing has been proved by a sufficient experiment, further resort to experiments in that case is purposeless cruelty?—I think it is.

1170. Do you think that, speaking of toxicological experiments, when performed even by men of great names in science, they are always necessary and really conducive to scientific results?—They are not always so. They may have an intention of working out some problem in their own minds; but as for being of general use in science, they certainly are not so; a number of experiments in fact are uselessly performed.

1171. Would you give us an illustration?—I would say that was the case chiefly among the French authorities, and, without perhaps naming individuals, I may say that a very eminent toxicologist was in the habit of experimenting upon dogs on a very large scale indeed—and after giving the poisons (nearly every poison in the list that we know of), to the animals by the mouth he cut into the neck to tie the œsophagus (the gullet), to prevent the animal vomiting; and of course that must have caused great pain and suffering, and it also prevented the efforts of nature to get rid of the poison, and as the same time it defeated the object which a toxicologist ought to have in view, because it placed the animal in an unnatural condition; for one could not fairly judge from the symptoms what the effects would be from that particular poison. For that reason in my work on Toxicology I have not been able to make any use of the hundreds of these experiments which this French physician performed; I have only been able to make use of those observations that were made on human beings, where the poison was allowed to act in the usual way without interference. The application of a ligature to the gullet was attended with great pain and suffering, and defeated the very object for which he undertook it. He wanted to prevent vomiting, and in doing that he altered, of course, all the circumstances which we should study in regard to a human being. Others have worked since that time on dogs, but I believe not in quite so cruel a fashion. Another eminent French toxicologist now living, has operated, I know chiefly, on small birds, pigeons, and sparrows, without resorting to experiments of this kind.

1172. Are those kinds of experiments really conducive to scientific conclusions or not?—I can hardly say that they help us much, the results depend so much on the weight of the animal and the circulation of the blood, and so on; they allow us simply to say this, "Such a substance is a poison or not." The toxicologist has tried an experiment with the poison of serpents or the Cobra poison, and it has just allowed him to say "This destroys the life of a small animal," and that does not help us in medical jurisprudence. In fact, I have never quoted, in any evidence that I have given, the results of experiments upon such animals as frogs or birds.

1173. Now taking strychnine, does it follow that the action of strychnine upon all of the lower animals would be the same, or nearly the same, as on the human being?—Upon all those furnished with a spinal marrow it would.

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1174. But upon others?—Upon the invertebrate it would not, that is upon such animals as the bots found in the stomach of horses, or the larvæ of insects. With regard to them, they are not provided with the requisite conditions; they have no spinal marrow, no medulla oblongata, and therefore the poison has not that organ to act upon. I have buried these animals in strychnia sufficient to destroy a human being, and they have crawled out of the poison. Of course we derive nothing but the curious information that it requires a certain organ to be affected by a certain poison. Strychnia is what is called a spinal poison, and animals destitute of a spinal marrow do not suffer from it in the same way as others.

1175. Then it requires a great deal of scientific knowledge to form a just opinion whether a particular experiment in toxicology will be of any use, or not?—Yes.

1176. Use for purposes I mean of application to the human subject?—Yes.

1177. Either in regard to medicine or to jurisprudence?—Yes.

1178. And you think that many experiments are resorted to which are evidently useless for those purposes from the beginning, and which inflict a great deal of unnecessary torture upon the animals that are subjected to them?—Yes, I do. I would apply that observation more to French experimenters than to English, judging from my own knowledge.

1179. When a scientific conclusion has already been firmly arrived at, do you think it justifiable to expose animals to torture merely for the purpose of demonstration?—I do not, and I have invariably acted upon this principle. For a long period of years I have had to go over every poison that destroys human life (I have only just finished the summer course now), and with regard to strychnia, the facts are now so well ascertained as to its operation, and there have been so many observations, not only on animals, but on the human subject, that there really is no point to be gained by poisoning an animal with strychnine; we gain nothing by it, and in my opinion it is a cruel experiment.

1180. Purposeless cruelty, you think?—Purposeless cruelty.

1181. Have anæsthetics made a great change, do you think, in the question referred to us?—Well, I am not aware myself of their having been used, I have only heard it, and do not know it from any direct personal knowledge; but there are many cases in which the use of them might interfere with the object of an experiment in toxicology, because we require something like an active nervous system to show us what the effects of a poison are, whether the brain is affected, or the spinal marrow, or the sympathetic nerve, or other parts, and this we could hardly know if we destroyed sensibility by another poison before beginning to apply the poison in question.

1182. Now, is it necessary for such a purpose as that that the experiment should be long protracted, I mean for the purpose of discovering the nature of the operation of that particular poison?—No.

1183. Then would not the consequence follow that the animal should be put out of its pain as quickly as possible?—You mean after the performance of the experiment; yes.

1184. Now, do you think that the most eminent men in this country would be as desirous as any other men to assist in remedying the evil of which you have spoken, if it exists in this country, namely, that of trying experiments which are in their nature severe and also useless, or trying repeated experiments merely for the purpose of illustrating points that have been already established?—I think that scientific men would have no objection to join in that view at all. I think the object of scientific men in England generally, as far as I have known, has been merely to obtain some actual benefit from the destruction of life, and that they have rather avoided destroying life where it has led to no useful conclusion. I believe that they would join in any Act which the Legislature might enforce in trying to put down the useless and purposeless extinction of animal life for any object.

1185. Has it at all occurred to you what direction any such measures might take if they should be resorted to?—I have thought of that, but there appear to me to be very great difficulties. First of all it would be necessary to draw a clear line about animals, as to what animals are to be exempted from experiment, except under precautions. Another point would be this: if it were alleged that an experiment had been performed cruelly and unnecessarily, I doubt whether any magistrate or any ordinary authority would be able to form a judgment in such a case, unless at least he were assisted by some one acquainted with the subject, and with the object of the performance of such an experiment. Therefore, I think that legislation, to be effectual, should in some way or other define the animals or the class of animals, and that a person or persons might be appointed to aid the opinion of a magistrate in deciding a case when the question was whether the experiment was necessary or not necessary; a magistrate himself could hardly undertake this duty.

1186. Some persons of great authority have drawn our attention to the analogy of the law with regard to anatomy; has that ever occurred to you?—I have thought of it; in fact from an observation made by your Lordship, it has struck me that this leaves certainly a path open, that if licenses were granted as under the Anatomical Act, it would be very easy to secure a power of control over the performance of experiments, so that no person should be permitted to perform them except with a license; and no person, of course, would be licensed except one of public position who had some direct object in the performing of those experiments. A great deal must be left to the discretion and feeling of scientific men as to their performance. I believe that the general desire of scientific men in this country (I am speaking from my conversations with those who have had to do with poison) is really not to destroy life if it can by any possibility be avoided.

1187. You think it is a sentiment of humanity?—Yes; and the only exceptional instances have been in some great trial, where perhaps a man has been desirous of bringing himself forward, or trying a new thing merely for the sake of a little notoriety in some instances. This has been the only exception which I have known in which they have departed from the usual course.

1188. Were you at all aware what passed at Norwich, when a great deal of attention was drawn to the subject?—I read in the Medical Journals what did pass about some injections that were made into a dog. To me they appeared to be of a most cruel kind, and to answer no purpose justifying the nature of the experiment. Of course there will be a difference of opinion always in such cases, and I think you will find generally that physiologists will maintain a right to experiment to any degree. Medical jurists, perhaps, will not go to that extreme; they will only resort to it in special cases in which some new question has to be answered, or some new point has to be ascertained, connected with the administration of the law. I think that as new poisons are continually being found, and improvements being made in toxicology, it would be impossible to shut out altogether the opportunity of experiments, and I might tell your Lordship, perhaps, that one of the most important points on which I should chiefly rely, in the event of chemistry failing to detect a poison in a dead body, would be the application to an animal of some part of the substance which was said to have caused death, in order to determine its nature. There is, for instance, the Calabar bean, and some other poisons which are coming into medicinal use, and may therefore fall into the hands of those disposed to perpetrate crime. The Calabar bean is a powerful poison, paralysing the heart and destroying life. We have no chemical means whatever of discovering it; but it is found on giving an extract of it to vertebrate animals that it operates by

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suppressing the action of the heart, just as it does in the human being.

1189. (*Mr. Forster.*) As regards anæsthetics, many of the surgical and medical witnesses that we have had have approved of the suggestion that they should be used whenever possible in physiological experiments; but I suppose they would completely frustrate your experiments?—They would in toxicology. In regard to operations, I have no doubt about the propriety of their use.

1190. But in regard to toxicology, you could not try the influence of one poison while at the same time administering another poison, which an anæsthetic is?—No; we could not rely upon the results.

1191. You have studied poisons; now we have had a great deal of difficulty in finding out what is the real nature and effect of the curare poison, whether it is a poison which deprives the animal taking it of sensation or not?—I have not experimented much with that myself, but I believe that it operates on the nervous system, and that, given in what is called by hypodermic injection, it tends to act as a sedative on the nervous system.

1192. There are two theories about it, one that it does that, and the other that it merely stops the muscular action, and that therefore the animal may appear quiet and yet be suffering great agony?—I should doubt whether the muscles are not affected really through the nerves. I should be disposed to believe that the nerves are the first to be affected; but it depends on whether you use the curare extract or the alkaloid called curarina. I believe there is not a grain of the alkaloid in this country. I have only the curare extract, and that is a kind of preparation made by the American Indians, we do not know exactly how, and we are not quite sure that it is always made in exactly the same way.

1193. It is what they poison their arrows with, is it not?—Yes. It seems to act rapidly on the muscular system; the animal drops dead after running some distance; but I believe its real operation is on the nervous system.

1194. I suppose there is no case on record of any man having been poisoned by a poisoned arrow and recovered?—No, I have never heard of any.

1195. You state that for the defence in murder cases sometimes there are a great number of experiments tried?—Yes, in cases which excite great public interest that is so.

1196. I think you stated that you are of opinion also that they are needless in their extent?—I think they are.

1197. But would it not be very difficult to limit by law the power of a man tried for murder to accumulate evidence on his behalf?—Certainly it would; but I think that an accused person does not improve his position with regard to scientific evidence by the destruction of so many animals. Now, to take the case of one witness who gave evidence for the defence in Palmer's case, the destruction of 60 animals, looking at his conclusions, was really quite unnecessary; it was merely an effort to overwhelm the evidence for the prosecution by the number of experiments.

1198. I suppose that the counsel for the defence used a number of experiments showing a certain result as an argument?—He used the totality; he said, "Sixty experiments against four." But one experiment may be better than 20, according to the mode in which it is performed.

1199. Do you think that that argument could be of any real service?—No, I do not think it influenced the jury; but it did so happen in that remarkable case that the foreman of the jury was a man thoroughly acquainted with the action of strychnine on animals.

1200. Do you remember that curious case of Tawell?—Yes, I was consulted in that.

1201. Were there any experiments in that case?—Not many; that was by prussic acid, and all the facts connected with that are pretty well known.

1202. What animal do you imagine would experience the effects of a poison most like a human being?—A dog. I might mention to you that the doses of poisons for a middle sized dog are very similar to those which will act upon a human being; so that a middle sized dog comes the nearest to a human being; the symptoms of suffering and the whole course of the proceeding are really very much the same.

1203. Do you think that these experiments are useful, not merely for legal purposes, but also for discovering antidotes to poisons?—I think they are.

1204. Do you know of any cases in which an antidote has been discovered by the help of experiments—I have known cases in which it has been said that the hypodermic injections of certain antagonistic poisons have had a good effect; but I have not myself experimented in that way by the injection of a poison beneath the skin. Now, in the case of strychnia, curare has been employed among other things. It has been said that that administered to animals or injected beneath the skin has had the effect of counteracting the excessive stimulus which strychina produces. But unless these poisons are used in proper proportions the animal is likely to die. I do not put much faith in them, and I do not know that we have yet learnt anything, so far as treatment is concerned, from our experiments with them on animals. With reference to prussic acid, an experiment which did not involve any wounding of the animal, the placing of a dog or cat under a tap of cold water has roused it; and that has been found a good remedy in human beings, cold affusion; this did not involve suffering.

1205. Sir James Paget illustrated what he conceived to be the necessity of experiments in some cases, by the endeavours to find out a cure for snake bites, by which it is stated that 20,000 are killed every year in India. Do you imagine that experiments are likely to do much good for that purpose?—No, I do not. I have read them all with great care. Ammonia has been recommended by Dr. Halford in Australia; but this has proved utterly inefficient when the experiments have been fairly performed; and, in truth, if you consider for a moment the mode of death from poison, you will see how difficult it is for any antidote by injection to operate. The poison rapidly gets into the blood, when in the blood it alters this fluid; and unless the remedy proposed enters into the blood quite as quickly, and very soon after the poison has entered, no good can be done. There may be some slowly operating poisons; but with regard to serpent poison, when it once enters the blood, the effect is most extraordinary, the rapidity of death is very great indeed.

1206. Not many murders are committed in these times by slowly operating poisons I suppose?—No, not many. It has unfortunately transpired that some members of the medical profession, or men connected with the medical profession, have thus made use of their knowledge in an improper way where chronic poisoning has been the cause of death. The public in general know nothing about what we call chronic poisoning, the administering of small quantities at intervals. The only cases that I happen to remember were the case of Palmer who poisoned his wife, and Smethurst, and Pritchard, and two or three more who showed in the use of poisons a kind of medical knowledge which we have not to contend with in the public in general.

1207. The poisoner, to succeed in such cases must have constant access and also knowledge?—Yes; and there the public are so far safe. The general effect in all cases of criminal poisoning to my mind is this: the poisoner does too much, he gives too large a dose, and by that, the crime is detected.

1208. (*Sir John Karslake.*) Do you consider that there are many cases in which it is inexpedient to give to the world, generally through publications, the history of the operation of a particular poison?—I think it is so with regard to those which we are unable to detect by chemistry.

1209. If those experiments which have been made and which have established the effect of a particular poison, are not generally circulated among the members of the medical profession, does not that lead

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to further experiments being made by those who have not the means of that knowledge for the purpose of establishing facts which are already established, if they were only known?—It would be so.

1210. Is not that very objectionable?—It would be objectionable; but at the same time we must steer between two difficulties, on the one hand of not making these matters too public, but at the same time giving enough to inform professional men. I have endeavoured to do that in a work on Poisons lately published, that is to say, to give to professional men knowledge without giving knowledge to the public.

1211. You give the hint without giving the full details?—I doubt whether it would even amount to a hint.

1212. I have understood from one of the gentlemen who have been before us here that any discoveries that are made are generally published to the profession almost as soon as they are made, and that is a strong reason against repeating experiments when the facts have been thoroughly well established?—No doubt in the medical journals we find all that is new brought forward; but I do not know that this would limit or affect much the number of experiments.

1213. I think you have said to Mr. Forster that you would not propose to limit by legislation the power of medical and scientific men to ascertain facts for the purpose of establishing a defence in a serious case like a case of murder?—I would not; and indeed, for the improvement of physiological science, there may be a great many things justifiable; the only thing is not to multiply these cases and perform experiments unnecessarily, but to have a certain definite object in view.

1214. That which would be a very great argument to adduce before a jury might be considered insufficient by scientific men?—Yes.

1215. It might be a strong argument to a jury that in 50 cases in which an experiment had been tried no particular result had occurred to oppose to evidence that in one particular case that result had occurred? Yes; they would however be apt to be influenced by the number of the experiments.

1216. And by the argument that in the one case the experiment was not a satisfactory one, or that the experiment was not accurate?—Yes.

1217. Have the cases been numerous in which you have attempted to ascertain by experiments the difference in the size of the corpuscles between the blood of animals and the blood of human beings; the quantity of blood you have to take from the living animals for that purpose is very small, is it not?—Yes, the slightest wound will give enough.

1218. With regard to strychnine I think in some of the evidence already before us it has been said that it is important that medical practitioners, when they are students, should see the operation of strychnine upon animals to see how it causes death. Is that your opinion?—I really do not consider it absolutely necessary.

1219. I do not put it as high as "absolutely necessary," but that it was considered expedient; is that your view?—No, I do not consider it to be so. If I had thought so I should have performed such experiments in my own class; but from having seen the results so frequently myself I think I have the power of describing to a class the symptoms so accurately that they would know them. There is something very dreadful in the operation of strychnine upon an animal; no doubt it suffers agonising pain.

1220. But the knowledge of the operation of strychnine in this country is comparatively recent, is it not?—Chiefly since the trial of Palmer.

1221. Or since that of Mrs. Sergison Smith, about the year 1847 in Hampshire?—Yes; but as long ago as 1831, when we knew very little about it, there was a case of life insurance, where a man had by means of strychnine destroyed a lady whose life he had insured. There a very eminent medical man was deceived, and set the case down to tetanus, natural disease.

1222. In your judgment at the present day are the symptoms of poisoning by strychnine so well established that it is unnecessary to show students the operation itself, in order to enable them to see what the effect of strychnine is?—I think it quite unnecessary.

1223. (*Mr. Erichsen.*) I would pursue the question that has been put by Sir John Karslake a little further, and ask you whether you think it necessary or not to poison animals for the purpose of demonstration to classes of students, in order to exhibit the post mortem appearances presented say by the stomach and intestines?—I do not think it absolutely necessary. Of course it would be a kind of addition to their knowledge to have something beyond verbal description; but there is no ordinary poison, the effects of which we are not now well acquainted with, in regard to the appearances of the body. I have relied a great deal upon the minute descriptions which I have given, and in producing wax models of the stomachs of human beings, but I have not considered it necessary to perform the experiments.

1224. Coloured wax models and drawings are sufficient, you think?—Yes.

1225. And it is not necessary to kill animals for that purpose?—No.

1226. (*Mr. Forster.*) A great many of the medical men who are called in to give evidence are medical men of not much note in their profession. If an inquest, for example, is held in a country place you must take the best man you can find?—Yes.

1227. Would such a man, do you think, be able to form a judgment about questions like strychnia poison?—If he has had a proper education according to the present rules of our colleges he would be. On one or two occasions I have experimented before a class, showing the effects of the mineral acids upon animals, but I have not carried that out to any great extent. I believe from a general acquaintance with students, that they have received as much information from accurate description and the illustrations by models and drawings, as could possibly be gained by experiments on animals.

1228. (*Mr. Erichsen.*) And do you consider it necessary to poison an animal in order to analyse the contents of the stomach, and so to show the students the method of analysing the contents of the stomach of a poisoned man?—No; I have a number of stomachs of persons who have died of poison, and I use them in preference to any experiments on animals. Articles of food and other matters have even been sent to me from Sierra Leone for analysis where poison has been used. These are kept, and they are used for demonstration before the classes without resorting to the destruction of animals.

1229. And in the other department, that of medico-legal teaching, independent of the toxicological department, do you find it necessary to destroy animals?—I do not call to mind any case.

1230. For instance, in illustrating the effects of wounds and injuries, and so forth?—The only instance I have known has been not for showing the nature of wounds, but for the production of fresh blood from an animal.

1231. But a mere puncture will suffice for that?—Yes, very little indeed will suffice.

1232. You are connected with one of the largest schools of medicine in the country?—Yes.

1233. Is it the practice in that school to allow students to perform toxicological experiments?—Yes; there is a practical class and a laboratory on purpose. I have not that department now; that is in the hands of another gentleman. We divide our work now, and the students are exercised in the examination of poisons.

1234. Upon the living animal do you mean?—No, chemically. Sometimes, I believe, my successor has given arsenic to rabbits to show the mode of extraction from the living animal. I have always preferred to take human viscera, and, having had so many cases sent to me to examine, I have had a sufficient number of them for experiment and demonstration.

1235. In the large school with which you are connected are experiments upon living animals done by

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students for toxicological purposes?—No; the only experiments I know of at Guy's Hospital are those chiefly conducted by Dr. Pavy; they are not performed by the students, but by the professors themselves, and for a special purpose.

1236. So that it would be wrong to say that there was anything like reckless or improper experimentation at the school of medicine with which you are connected?—I think it would. I make this remark generally for England, from what I know about toxicologists, and, seeing all the works published, I think that the experiments performed on animals are chiefly by the French and Germans.

1237. (*Mr. Hutton.*) You think then that there is no reckless experimenting in toxicology in England at all?—I have never heard of anything of the kind, with the exception of what I have already stated, namely, that in some great trials there has been a desire to overwhelm evidence given contrary to certain experiments by increasing the number of them.

1238. You rather expressed a doubt about urari, but you thought it was an anæsthetic?—I believe it operates as a sedative.

1239. Claude Bernard says that it acts upon the motor nerves, and not upon the sensory nerves?—I am aware that this has been said. I believe the differences are to be explained by the fact that what is sold as curare, the extract, is liable to variation.

1240. You are aware that this urari is very commonly used now in the physiological laboratories in England?—Yes.

1241. Which would be used, the thing which you think would destroy pain or the thing which would only paralyse the motor nerves?—I have not heard much of anæsthetics being used at all myself.

1242. (*Mr. Forster.*) Supposing that you were going to make an experiment upon a living animal which could be done successfully under an anæsthetic, would you use this curare as such anæsthetic?—No, I should not; I think curare is far too dangerous a preparation to use.

1243. And too doubtful as to whether it really prevents pain?—Yes.

1244. (*Mr. Hutton.*) I find that in the case of frogs, and often in the case of mammals, in this handbook of physiology before us, curare is frequently the poison used?—I have tried in every way within the last month or two to get the curarina, the alkaloid, and I have not found it possible to do so. I am told it is only made in small quantity for experiments performed by some German chemist. The alkaloid, no doubt, would destroy sensation—the other thing, the curare, might or might not; its effects are variable.

1245. (*Chairman.*) That which you are sure would, is so rare that it can scarcely be obtained?—Yes.

1246. And as to that which is ordinarily used there is no security that it does?—No; and by giving it in any case you place the life of the animal in great jeopardy.

1247. (*Mr. Forster.*) And that alkaloid, curarina, would almost certainly kill?—Yes; the medical and scientific evidence about this substance is too conflicting to allow of its general use—chloroform or ether in vapour would be comparatively safe in preference to it.

1248. (*Mr. Hutton.*) With respect to the remark that it would be very dangerous to limit the power of private people to make these experiments in cases where justice is in question, do not you think that if a certain number of licensed laboratories were distributed throughout the country, the necessary toxicological experiments might be made in those laboratories without any difficulty?—Yes, without leaving it in the hands of individuals.

1249. Without leaving it to the discretion of private individuals?—Yes.

1250. (*Sir John Karslake.*) But would that apply to a case where you are obliged to try an experiment with a part of the vomit of a person who is supposed to have been poisoned?—That would be a case where I think it would be necessary to operate at once. It would be necessary, if we used it, to employ the extract at once without waiting for a laboratory for the purpose.

1251. (*Mr. Hutton.*) And without sending it up to London?—Yes.

1252. Do you not think there are very few exceptional cases where it might be necessary to break the law if the law did not permit it?—Yes.

1253. (*Chairman.*) Or would it be well to frame the law in such a manner as that provision might be made beforehand for cases of exceptional necessity?—Yes, exactly so.

1254. (*Mr. Hutton.*) You were saying that if any law were made there should be a great distinction as to the animals. Would you say that frogs, for instance, suffer less than mammals?—It is difficult to draw an inference on that point. I should rather measure the amount of sensibility and pain by the perfection of the anatomy of the nervous system; and I cannot believe that frogs suffer in the same degree as the higher class of animals.

1255. They have a vertebrate system, have they not?—They have a vertebrate system, but it is more diffused, not a system concentrated in one great organ like the encephalon, which can receive and transmit impressions. In the frog there is a distribution of nervous power over the body; a frog will jump about after his head is cut off; and frogs have a wonderful tenacity of life in their different parts; the slightest thing will excite them and set them in a state of tetanus, even sometimes merely touching them; but I do not think that this is exactly an indication of their suffering pain, but the effect of the motor nerves being more easily excited.

1256. There is an experiment without any anæsthetic given in this handbook in which the epithelium is scraped off a frog's cornea, and then nitrate of silver is brushed on to the cornea in order to inflame the cornea. Now, do you suppose that that would cause severe suffering or not?—No, I do not think it would.

1257. The frog gives all the signs of suffering—there is no question about that, I think,—in its way of using its fore legs?—The cornea itself, I may remark, is a horny substance, with not much sensation in it.

1258. There is another experiment in this handbook apparently for a demonstrative purpose. A frog is put into warm water, and the temperature is raised to about 35 or 40 degrees of centigrade; that is about 100 degrees of Fahrenheit, I suppose; and then the frog is killed; and, apparently, it is to demonstrate something on the reflex system of the frog. Is not that nearly equivalent to boiling a human being alive, having regard to the different temperature of the frog's blood and the human blood?—It is, certainly. Raising the temperature to 100 degrees in water for a cold blooded animal would be very like putting oneself into a temperature of 212 degrees.

1259. And that you would regard as a cruel experiment?—That is a cruel experiment. I cannot see what purpose it would answer.

1260. I think you said, with regard to even the very few experiments which you have ever made before a class, in the way of demonstration, that you have rather discontinued them and found them unnecessary?—Yes.

1261. So that now, even with regard to mineral acids, you do not use experiments?—No. My object there was to demonstrate to the students the destruction of parts produced by taking corrosive poisons; and that led me on a few occasions when I began lecturing to perform experiments; but I did not find that the students gained more from them than from the splendid drawings and illustrations which we have in the museum; and I have taken pains during my lectures there in every important case to have drawings and models made in order to avoid the necessity of such experiments. The models in the case of human beings are beautifully executed; and sometimes we have them in the case of animals. We have models of the stomachs of dogs, for instance, so that it is not necessary to resort to such experiments.

1262. (*Mr. Erichsen.*) The pathological appearance

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as presented by the influence of the poison is shown in these models and drawings?—Yes. We have them in the horse, the dog, and a great variety of human beings, a whole portfolio of drawings; so that I have found it quite unnecessary in order to go into that point to repeat experiments.

1263. (*Mr. Hutton.*) But taking a scientific toxicologist, a man who is inquiring for the sake of science, on what scale should you imagine that he would have to experiment on animal life; how many animals would he put to death in the course of the year, pursuing his studies as a scientific investigator?—I have a great difficulty in answering that question, because it would depend upon what he was investigating. It might be the amount of poison that would destroy the life of the animal; or it might be the removal of it by absorption, and the diffusion of it through the body.

1264. But, putting aside now your experiments for the purpose of justice, on what scale would your own experiments have been performed?—Rarely more than one animal.

1265. Rarely more than one for a particular experiment, that it to say?—Yes.

1266. In the most active time of your life, what number a year would you have used for experiments?—Taking the whole 40 years I do not think I have experimented upon more than 12 or 14.

1267. So that in fact you do not think there is anything like a hecatomb of victims sacrificed for the investigation of these questions?—Certainly not in this country.

1268. Do you know anything of foreign investigations?—I have studied abroad, in Paris.

1269. Is the scale of investigation there very much larger and more reckless?—Yes. I think there the disposition is to work a great deal more by experiments on animals than in this country. I know it chiefly by repute, because my studies have been more in regard to human beings; but I have heard that at Alfort and some other places in France experiments are carried out on a very cruel scale.

1270. Are you at all well acquainted with this handbook of physiology edited by Dr. Sanderson?—I know it as a book; it is a book of very good repute.

1271. Would you mind giving me your opinion as to whether that kind of experiment is at all necessary which is mentioned at page 108, on the mesentery of a frog. Do you regard that as a very painful experiment, and as a very instructive one, and one at all desirable for the purposes of a handbook?—I think it a very painful experiment, subject to the observation that the frog does not suffer as much pain as ourselves, but still for the animal I have no doubt it is a very painful experiment, and I do not see what good purpose it would answer.

The witness withdrew.

Adjourned to to-morrow at 12 o'clock.

Thursday, 15th July 1875.

PRESENT:

The RIGHT HON. VISCOUNT CARDWELL, IN THE CHAIR.

The Right Hon. W. E. FORSTER, M.P.
Sir J. B. KARSLAKE, M.P.
JOHN ERIC ERICHSEN, Esq.
RICHARD HOLT HUTTON, Esq.

N. BAKER, Esq., Secretary.

MR. GEORGE ROLLESTON, M.D., called in and examined.

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1272. (*Chairman.*) You are the Linacre Professor of Anatomy and Physiology at Oxford?—Yes.

1273. You have taken part already, I think, sometimes in the discussion of the question which is the subject of our inquiry?—I have. In the year 1863, in an address to the Biological section of the British Association at Newcastle, I brought the subject into my address. I have here a copy of the report furnished by myself to the association's volume for that year. It runs thus, pages 109–110 of Transactions of Sections:—"Whilst upon this subject "Professor Rolleston made some remarks upon vivi- "section. A defence might be set up for it upon "the following grounds, and under the following "limitations:—Firstly, in the operations passing "under that name, the first thing done in many cases "was to extinguish life and sensibility in a manner (as "by pithing) as much more speedy than the ordinary "methods for the destruction of animals, as the scalpel "of the anatomist was a surer and speedier agent "than the clumsy tools of the slaughter-house, or "the uncertain ones of the sportsmen. In such "cases the term vivisection was a misnomer. "Secondly, chloroform was in these days almost in- "variably employable and employed; the cases in "which it could not be put into use, on account of "its introducing some chemical or other source of "fallacy, were very few. On the other hand, it was "quite open to the opponents of vivisection to say "that recognizing the susceptibility to pain which "the lower creatures had, and in addition to this, "certain rudiments in them of a moral nature as "giving them still further claims upon our con- "sideration, and feeling that we could not without "grievous injury to our own better nature make "a practice of sacrificing their lives, we were necessi- "tated to regard vivisection as something unjustifiable "and indefensible. To this it might be replied that "such a line of argument, if consistently followed "out, would lead, as indeed it had led, to vege- "tarianism, against which the instincts and even "less ambiguously the practice of the great mass of "mankind would be found to rebel, at all events at "present. And it might further be said that the "results of experiments on the lower animals had "enabled us to understand something of the nature "of, and to combat with something of success, the "attacks of two terrible human maladies, epilepsy "and diabetes. The question was a complex one, "very different considerations having to be weighed, "one against the other, one scale containing human, "the other brute suffering. Wantonness and mal- "ignity were of course excluded from our considera- "tion, whilst on the other hand the means at our "disposal for the extinction of sensibility and of life "diminished the amount of brute suffering to a "very small actual residue. Nothing, however, could "be alleged in favour of vivisection if practised "for the sake of obtaining merely greater operative "dexterity, and the whole discussion was expressly "limited to the consideration of it as practised in "England by the following words:—'In a country "'like this, where human life is highly prized, "'brute misery will never be wantonly produced.' "'The merciful man is merciful unto his beast.' It "is possible that where human life is held cheap the "man who loveth not his brother may be wanton in "his treatment of the brute. This is not the case "here; and in a British Association I need allude "no further to the matter." Also, when I presided

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over the Biological section of the British Association when it met in 1871 at Liverpool, I was entrusted with drafting a report about this subject, and those resolutions have been spoken of since as the resolutions of the Liverpool Congress. I have here an authentic copy of what was done. Those resolutions were drafted after the meeting at Liverpool, and were accepted at the meeting of the British Association at Edinburgh in the ensuing year. I understand that they are already before this Commission.

1274. You are a member of the committee for forming a conjoint scheme for medical examination?—Yes.

1275. What is that committee?—I hold their regulations in my hand, and will put them in. It having been seen to be an evil that there should be competition between examining boards for licences for practice, the Government of the day suggested that the various boards should combine, and form what is called an uniportal system for admission of candidates to the medical profession. The College of Physicians, the College of Surgeons, and the Universities, and so on, all met. We then drew up various regulations; and you know there has been a Bill before the Houses of Parliament,—the Bill under the charge of Sir John Lubbock,—which will do away with the last technical objections to the establishing of this uniportal system; and in that part which enforces the attendance upon courses of physiology we add this note, L., which stands under paragraph 12:—"By the practical course "referred to in clause 12, it is meant that the learners "themselves shall individually be engaged in the "necessary experiments, manipulations, &c.; but it "is not hereby intended that the learners shall perform vivisections."

1276. Now, were the resolutions of the Liverpool Committee in all respects satisfactory to you?—I suppose I may say, as I drafted them, that they are not; for this reason, that they have omitted any provision for the extinction of the life of an animal which has been operated on under anæsthetics, so as to prevent its wakening up into pain after the expiration of the operation of the anæsthetic. I drafted the resolutions, and I really did not know that a thing of that kind was done, otherwise I should have added it.

1277. You thought that it was sufficiently understood, but you are now of opinion that it had better be more clearly expressed?—That is just what I wish to say.

1278. What do you consider to be the object and uses of experiments upon living animals?—I should like to say before that, if you would allow me, that I think that the Liverpool meeting of the British Association did recognize very distinctly two or three very important matters, namely, that there is a great difference between the employment of vivisection for research, and the employment of it for educational purposes; and I think too, with regard to the insistance which those resolutions embody, that anæsthetics should always be employed whensoever, if ever, vivisection is employed for teaching purposes, that their insistance upon the invariable employment of anæsthetics for teaching purposes is a great step.

1279. What I understand you to say is that experiments involving pain should never be resorted to except for a necessary purpose, and if resorted to should always be performed with the greatest use of anæsthetics that the nature of the case admits of, and, speaking generally, with the greatest attention possible to humanity?—Most certainly; but I wish also to say another thing, that before more than an exceedingly limited number of individuals painful experiments should not be performed at all.

1280. What do you consider to be the necessary purposes?—I think that the necessary purposes for the employment of vivisection, not using the word etymologically, but using it in the sense in which it is employed in this Bill of Lord Henniker's, are these: Firstly, such a matter as this: if a person was found to die suddenly, or to be taken violently ill with whatever it might be, convulsions or what not, after eating say a particular pudding, it would be quite justifiable to test what the remainder of that pudding would do with a dog, or something of that kind. That is, I take it, the first necessity for it, its employment in medical jurisprudence,—the testing for poisons, which, as your Lordship will be aware, was done in Palmer's case; though there it was not a testing what was eaten, but just testing with strychnine. Secondly, I think that it may be used for testing the operation of drugs and medicines of various kinds; though I must say, speaking scientifically, that there is great difficulty about arguing from any one animal to ourselves, and also, considering what varieties of idiosyncracy there are in us, even in arguing from one man to another. No doubt you are aware, for instance, that some men are acted on by opium in one way, others are acted on by opium in another way; or, to put it still more plainly, a rabbit will eat as much belladonna as would poison a large number of men, and yet it will not act upon the rabbit in the least. Then I think the further uses of vivisection (all these being in research) are in such matters as surgical operations. Of course, such a thing as tying an artery, to show that it is possible to tie an artery, say above the knee, without starving the limb below, when it might be necessary to do such an operation for what is called aneurism, the widening out of the artery just at the back of the knee or lower down was a justifiable use of vivisection. Then certainly the functions of ourselves in health and in other kinds of disease have received considerable elucidation from vivisectional experiments. Of course, it is quite erroneous to say that all knowledge of medicine, or all knowledge of the functions, is dependent upon experiments on animals, because there is available for obtaining such knowledge the examination of dead bodies of men and of other creatures, and the comparing what is seen in the dead body with what has been observed in that body while it was yet a living machine. But it is right and fair to say, with regard to vivisection, that it has thrown light on some of the most terrible diseases of which we are the subjects,—diabetes, for example. You can produce diabetes artificially, as you can see detailed in a book which I see upon the table here, and that gives you some insight into its cause, that is to say, into the very nature of the thing. Again, epilepsy is a thing which can be produced artificially. I am speaking of vivisection as a means whereby you get all this knowledge in the way of pure research, and without any bearing upon the educational application of it you certainly have uses for it.

1281. By the word "vivisection" you throughout understand what is more accurately expressed in the terms of our Commission, "Experiments on living animals for scientific purposes?"—Yes; just what in this Bill vivisection includes, "The cutting or wound-"ing or treating with galvanism or other appliances "any living vertebrate animal for purposes of "physiological research or demonstration, also the "artificial production in any living vertebrate animal "of painful disease." It is the last sentence of Lord Henniker's Bill.

1282. But the terms of our Commission being "Experiments on living animals for scientific purposes," would include everything?—I think so.

1283. And when you use the word you do so in that wider sense?—Yes; not in its etymological sense. Then, coming next to vivisection for purposes of teaching. I cannot but think that most persons would grant that as to students of medicine, or any students, except those who intend to devote themselves to purposes of research, they should not be required to have themselves performed vivisection. In that paper, which I have handed in, you will see that the conjoint Board represents the Universities of Oxford, Cambridge, London, and Durham, the College of Physicians, and the College of Surgeons of London. Those regulations were agreed to by representatives of all those corporations; and I must say, in addition, that *à fortiori* it would be wrong to make the performance of experiments on living animals a subject in examination. They should not be required

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to bring certificates of having performed such experiments, and much less should they be set to do them when in an examination.

1284. In point of fact you think that students, meaning people still *in statu pupillari*, ought not to perform such experiments at all ?—I should think that with the exception of students who were going to devote themselves to physiological research as a profession, it is not a fitting educational subject for students.

1285. Would you draw the distinction that those who intended to be practical surgeons should not do it, and that those who intended to be physiological inquirers should do it ?—That they *might* do it.

1286. Do you think that this habit of making experiments upon living animals is liable to abuse ?—I certainly think it is, and from several points of view; but I should be glad first to put in a paper, which has only come into my hands within the last day or two (if your Lordship will allow me). Now, I have here a letter from Trinity College, Dublin, written to me by Professor Samuel Haughton, a man of very great scientific attainments, a Fellow of the Royal Society, and so on, in which there is a copy of a resolution carried for the School of Physic in Ireland, in which I find a note to the effect that "Vivisections are strictly prohibited" for teaching purposes. I should like to say a word on one other point. Speaking now, not as a man of science, if I may presume so to call myself, but just as a person familiar with teaching, a large part of whose life has been taken up with teaching young men, namely, on this point: To what extent is it necessary that facts should be actually reproduced before students? I think that is a point on which you would perhaps wish to hear what I have to say. There are two, or rather three, views which you may take about it. One is the view which is taken by Mr. Todhunter. He is one of the great teachers of mathematics in Cambridge. He has recently written a book called "The Conflict of Studies;" in that you will see what I think is one of the extremes, and not the right view. Mr. Todhunter says this at page 17 of that work: "It may be "said that the fact makes a stronger impression in "the boy through the medium of his sight, that he "believes it the more confidently. I say that this "ought not to be the case. If he does not believe "the statements of his tutor, probably a clergyman "of mature knowledge, recognized ability, and "blameless character, his suspicion is irrational "and manifests a want of the power of appreciating evidence, a want fatal to his success in "that branch of science which he is supposed to "be cultivating." Now, that is one view that may be taken of it. There is an entirely opposite view, and that is this: I have heard it expressed in this way, that it is actually immoral to take any statement whatever upon faith that can be verified by the eyes; which seems to me to be even further from the truth than the opposite pole which I have just put before you. That leads to profusion and extravagance in experimentation of every sort and kind, whether of a chemical or physical or biological character. I would just say this, that my own view is this, that a few cardinal experiments in any subject which is subjectable to the eyes are justifiable to enable a person, who watches them attentively, to understand the records of any similar experiments, howsoever numerous. Let me illustrate it now by this case, from a somewhat alien subject. I have been engaged myself in the study of pre-historic man, and I found it difficult to understand the description of the stratification of deposits in caves. I made it my business to go and examine one cave thoroughly, and I have now no difficulty whatever in understanding descriptions of any number. That is one way of putting it. I give that illustration just to make plain what I mean. This, I think, is a medium view, that a few cardinal experiments may be useful and legitimate for large classes, always provided that the anæsthesia is thoroughly maintained during them, and also provided that the experiment is one which cannot be sufficiently demonstrated, except upon the living subject.

1287. Is not this practice of making experiments upon living animals very liable to abuse ?—I think it is very liable to abuse. It is liable to abuse in common with many other studies, and it has also certain special liabilities which are inherent in its particular nature and essence. Firstly, it is amenable to abuse when employed for the purposes of research; and I must just say that with regard to all absorbing studies that is the besetting sin of them, and of original research, that they lift a man so entirely above the ordinary sphere of daily duty that they betray him into selfishness and unscrupulous neglect of duty. Vivisection is specially likely to tempt a man into certain carelessnesses, the passive impressions produced by the sight of suffering growing, as is the law of our nature, weaker, while the habit of and the pleasure in experimenting grows stronger by repetition. Professor Schiff says in a work of his, Leçons sur la Physiologie de la Digestion, tom. i., p. 291, 1868, that when dogs come into his laboratory, he finds it necessary to cut two of their nerves, the nerves of vocalization, the inferior recurrent laryngeal nerves, as they are called. "*Je suis obligé* "*de faire subir cette dernière opération à beaucoup* "*de nos chiens fraîchement arrivés au laboratoire,* "*pour les empêcher de se livrer à des concerts* "*nocturnes trop bruyants et de discréditer ainsi les* "*études physiologiques aupres des habitants du* "*quartier.*" It is true that it is only in young animals that this operation entails great dyspnœa and distress; still it is a grave operation in any animal, and ought not to be spoken of in terms of such levity as those quoted. Further, in a letter of Professor Schiff's in the "Times" of January 7, 1874, he distinctly says that the reason that the inhabitants of the district were not so disturbed as that French quotation says they might have been, was that there were no dogs in pain in his laboratory. Now it is perfectly clear that the statement in the book published in 1868 ought to have been shown to be reconcileable with the statement in the letter of January 1874. The matter is of sufficient importance to justify one in demanding this. I mean to say that vivisection in its application to research may be somewhat more demoralizing than other kinds of devotion to research; every kind of original research being a gratification of self, and liable to develope selfishness, which of course is the root of all unscrupulousness. Then, again, it is amenable to abuse when employed for teaching purposes. A lecturer, when lecturing, is to a considerable extent upon his trial, and he is under a temptation to set aside any considerations which may tend to impede him in just showing what he is worth. Still in teaching, a lecturer on biology is, perhaps, no more amenable to temptation than any other lecturer. I would here say that really one of the good things that lectures do in these days is, that they subject a lecturer to public opinion, and put him on his trial. I know well that into my lecture room any person may walk at any moment, and if he finds me lecturing badly, so much the worse for me. I think that in these days the lectures do more good almost to the lecturer himself than to the persons lectured to, considering the opportunities that there are nowadays of getting knowledge by books. I was saying that a biological professor is not very much more amenable to that temptation than any other person. I would just refer to Mr. Skey's work on surgery; he was a metropolitan surgeon, and has only just died; I received what little surgical education I received from him as his dresser. If you look into his work you will see this sentence: "A man "who has the reputation of a splendid operator is "ever a just object of suspicion." I have no data myself. I am happy to say that I have never seen a surgeon abuse his opportunities, or keep a patient under operation longer than needful for the sake of showing off his dexterity, or anything of that kind; but if any member of the Commission chooses to look over Mr. Skey's introduction to his book on surgery, he

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will see a good deal to this effect, which, however, is all summed up in that sentence I have quoted. Now a person who is operating upon the lower animals, who have no friends to remonstrate for them, is very much more likely to give way to such a temptation than a person operating upon human beings, who would have friends or other persons who would be made "wondrous kind" by a fellow feeling, thinking that the same thing might happen to them. But if I am to say all that I think on this matter, I do really and seriously believe that vivisection has special and distinctive liabilities and amenabilities to abuse; for it does act on our emotiono-motor nature in a particular way. Just let me illustrate this from the literary side. Canon Kingsley wrote a book called "Hypatia," in which he describes the attempt made to regalvanize into life the gladiatorial shows, and he describes the slaughter of men in the amphitheatre. When this took place under the eyes of an assembled multitude, he says, "then burst forth the sleeping devil in the hearts." You may express this by saying that this is the lower or animal nature, the unworked-out beast which exists in man. The way that Kingsley puts it is by speaking of the devil that is in the heart of every man; but, if you prefer, you may say that it is the lower nature which we possess in common with carnivora, and creatures of that sort; it is just this, that the sight of a living, bleeding, and quivering organism most undoubtedly does act in a particular way upon what Dr. Carpenter calls the emotiono-motor nature in us. Now I should wish it distinctly to be said that I know that the better class of natures are superior to this, but the very fact of their being a better class of natures means or implies that they are thus superior. A man is worse or a man is better, according as he gives way to or as he resists any particular temptation intrinsic in his own nature, or howsoever superinduced. I know that that is so, that many men are superior to it; but I beg to say that if we are talking about legislation, we are not to legislate for the good, but for the mass, who I submit are not always good. I have another point. I believe it is a fact to be accepted, that when we are massed together, we being the social and gregarious men that we are, various impulses are very much more potent upon us than when we are not so massed. It is a trite observation that people are acted upon differently by what they see in the theatre, and by the same thing as it occurs, or if it occurs, in private. When men are massed together the emotiono-motor nature is more responsive, it becomes more sensitive to impressions than it does at other times. That, of course, bears very greatly upon the question of interference with vivisections as employed before masses. I know that I am likely to be exceedingly abused for what I have just now said; but I think that nobody can demur to it who has both the knowledge and the honesty to speak of human nature as it is. Still, I should wish to fortify myself by the authority of a person who was something of a statesman as well as a scholar, a good mixture, in whatever proportions the two constituents be mixed, namely, Lipsius. Now Lipsius is referred to by Gibbon in his description, chap. xxx., of the attempt of Honorius to bring back the gladiatorial shows; it is a passage which is very well worth reading, and in fact I owe this quotation to Gibbon, and have verified it. Lipsius gave way to his antiquarian sympathies by saying something to justify the gladiatorial shows; and what he says illustrates what I now say, as he refers to our emotiono-motor nature in words which are very short and plain. This occurs in his De Saturnalibus ii., cap. xxv. Speaking of Saturnalia, at which gladiatorial shows formed part of the amusement, he says this:—"Suppose a man " is going to be executed, who does not leave his " business and run into the market place so that he " may have a look at the drawn sword of the executioner, and may look at the bleeding purple neck " of the executed wretch? *Videat et unius miseri* " *purpuream cervicem. Ita est. Queri libet de* " *perversitate naturæ*,"—These are the facts, "Blame " nature for it if you like." Without going back as far as Lipsius, one knows the trivially reported story that a butcher in France is not allowed to sit on a jury. And I may mention that soldiers will tell you that the sight of blood upon the gauntlet, that white glove which Dragoon regiments wear, to use their plain language, "wakes all the devil up in them."

1288. Then I may collect from what you have been good enough to say, that in your opinion the habit of performing experiments upon living animals is a very dangerous habit, and requires to be very specially guarded?—Quite so. I should not say that their liability to abuse should lead us to prohibit the use of alcohol or opium or chloral, or any of those things which we abuse as drugs or as food, and the cases are somewhat analogous.

1289. But that the use is one thing and the abuse is another; and there is a great tendency to abuse, and therefore the whole subject requires special vigilance?—That is quite what I think.

1290. Now do you think that there is a growth of moral sentiment in the direction of greater carefulness in the infliction of pain?—I think that is quite clear if you look at it in either of two ways. First, we may look at the history of individual minds, and I will give a concrete instance from traditions relating to the famous Haller, who did at one time practise vivisections, in his own words, *supra fidem certè numerosas*. Now, in studying Haller's Bibliotheca Anatomica, 1774–1777, for a different purpose from this, I came on a passage that struck me as extremely odd; he describes, tom. i., p. 447, a person as performing vivisectional experiments in these words:—"*Suecus* " *celebris per plurimos et vastissimos quos suscepit* " *labores, cum juvenis ex sæculi consuetudine animalia* " *viva numerosa incideret et potissime annis* 1649 *et* " 1650 *in vasis lacteis inquirendis laboraret*." "It " was the fashion in his age; he dissected a great " number of animals alive." I thought it was a curious thing for Haller to speak of its being "*ex sæculi consuetudine*;" but I had seen a statement somewhere to the effect that Haller was exceedingly careful about inflicting pain; and I interpreted the words in the sense which the much plainer utterance of Du Bois-Reymond regarding Haller's great successor, Johannes Müller, suggests: "Er allerdings nicht, wie " Einige heute thun, für das nothwendige Attribut " eines Physiologen hielt, dass seine Hände täglich " von Hundeblut rauchen."—Berlin Abhand. Akad. Wiss., 1860, p. 92. But that is not the whole story. The story is this: that as he grew older he grew more sensitive to the infliction of pain; and as is stated by Krug, (a German writer on moral philosophy, who was the author of many works, and the successor of Kant at Konigsberg,) Haller fell in his later age into a permanent anguish of conscience, which is shown in his epistles, reproaching himself most bitterly for his vivisections. These are Krug's words (Allgemeines Handwörterbuch der Philosophischen Wissenschaften, 2nd ed., 1833, vol. ii., p. 323):—"Auch der grosse " Haller hatte sich dergleichen Grausamkeiten erlaubt. " Er machte sich aber in seinen letzten Lebensjahren " der bittersten Vorwürfe darüber, und fiel in eine " Art von fortdauernder Gewissenangst, wie man aus " seinen Briefen sieht." Looking then at the date which the book I have quoted, viz., the Bibliotheca Anatomica, bears, namely, 1774, I find it was just three years before Haller's death. But I should wish to state that Haller was by no means in his dotage at that time, quite the reverse,—he was not 70 when he died,—three years later. That is the striking point; and I think I may say this (but I shall not give the name), that it is within my own personal experience, that a person who has a considerable name before the world, and has performed a large number of vivisections in his time, has expressed himself to me as exceedingly sorry that he ever did them, did them I should say to the extent which he did. The whole race has without any doubt whatever developed in sensitiveness in this direction. It is perfectly clear that there is scarcely a country in the world where such a punishment as crucifixion would be endured now; yet the Romans crucified thousands of men at once after the Servile war. And again, such a

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thing as the gladiatorial exhibitions would not be tolerated now. And this development has gone on considerably faster within the last couple of hundred years. Any person who will take the third volume of the "Philosophical Transactions," p. 522, Oct. 21, 1667, when people were repeating Harvey's experiments about the circulation of the blood, will see what was being done in France at the time. I may refer here to a number of the "Spectator" of February the 7th, 1874, in which it is said, and I believe with some truth, that human patients treated in Vienna or Paris are treated as they would not be in London or New York. Exactly the same thing is stated with regard to transfusion of blood in the "Philosophical Transactions" at the third volume, page 522, October 21st, 1667.

1291. Would you be kind enough to tell us what you think is the practical object at which we should aim?—To put it in a very general way, I should think the practical object was to put into a stable and permanent form the present wholesome condition of public opinion upon the subject. And I think the present wholesome condition of public opinion is this: that for class demonstrations, limitations undoubtedly should be imposed, and that those limitations should firstly render painful experimentations illegal before classes. I am loth to say anything about interference with original research; firstly (perhaps this might also be lastly), because I think it is impossible to so interfere with it. I am not entirely clear as to whether knowledge can or cannot be bought too dear; I will not pronounce myself upon that; but this I am quite certain that *de minimis non curat lex*, that what individuals do is not necessarily a thing upon which legislation should determine itself. But legislation is called for when numbers are concerned.

1292. But supposing that anything which can fairly be called atrocious is practised by an individual, do not you think that society would have both a right and a duty to restrain it?—Most undoubtedly. But are there not some acts which would just bring a person under the law for atrocious cruelty? I have not the knowledge of the law about that. What I am clear about is the positive side, that legislation is expedient and advisable, and called for with reference to class demonstrations. With reference to the other point, that is really a matter not so much for me as for people who can judge of what is possible and practicable; for instance, a wise governor of police who knows to what extent you can go into private houses, and what not.

1293. I think, if you will allow me, we will endeavour to obtain from you (and I think we are arriving at a just appreciation of your sentiments, what are the objects that society should aim at in the first instance; then afterwards we should be glad of any suggestions which may occur to you as to the mode in which we should recommend society to endeavour to arrive at them. Now what I understand you to say is that you think that experiments may be very much diminished in number without any injury at all to science?—Certainly.

1294. That if performed upon living animals they ought to be performed with anæsthetics in almost every case?—Certainly.

1295. That whatever is the most humane thing that can be done ought to be done?—Yes.

1296. That many persons, like, for instance, Haller, as they get more experienced become more sensitive to the mischiefs which in their earlier years they did not so clearly perceive?—I do not quite think it depends on experience in the particular person so much as on that development of the moral sense which one sees in individuals. A child will pull off a fly's wing and we think little of it; but if any one of us was to do it we should consider it very strange. It is not experience of a scientific kind but the evolution of the moral sense as a man gets more knowledge of the world at large, and so on.

1297. I may put it in this way—a man in his wiser times will condemn, as Haller did, that which in his younger times he practised?—Yes, I should be glad to say that.

1298. If, therefore, society is wiser than the individuals, we may be encouraged, by such examples as Haller's, in bringing the forces of society to bear upon the individuals?—If you can get them to bear on the individuals, certainly.

1299. Therefore, the object in view I think we have arrived at; and now the most important question for us is whether you can make any practical suggestion to us as to any particular mode of proceeding, or any limitations upon any mode that we might otherwise be inclined to pursue. Now, you would not allow in a lecture room the exhibition of pain before the pupils?—No.

1300. You would consider that demoralising to the pupils?—Certainly.

1301. In regard to individual research you are afraid that it may not be easy by any legislation to coerce an individual?—Of course it is very much more difficult to deal with a man within four walls and by himself than it is in a public place; that is all I mean.

1302. (*Mr. Forster.*) You mentioned two diseases in which advantage has been gained by experiments, diabetes and epilepsy. In what way has that advantage been shown in epilepsy?—Well, it is not so much in the ponderable advantage of therapeutical dealing with it as in that insight into its essence which may one day or another, we may hope, lead to some therapeutical improvements. As far as I know, that is the condition it was in when I left off practice; that is now some ten years ago. Ætiology had gained rather than therapeutics.

1303. Coming to practical suggestions, we have had before us the suggestions of making experiments on living animals illegal, even for the purposes of research, unless in licensed places and by licensed persons. What do you think of that suggestion?—As I said in answer to Lord Cardwell, that is rather a question for a person who understands interference of a domiciliary kind than for me. To what extent would you find it practicable to supervise a man in a study with doors and what not between him and the outside? What I do think is that a law should aim at what is practicable and verifiable in its results; but really I am speaking now on a subject which is not quite my own. I am quite clear of this, that any legislation in favour of restriction of the practice in one application would assuredly bear fruit in another; and if it was made illegal to abuse it in public, and before pupils, that would be a fixed point gained in the gradual evolution of moral tone upwards: that is my feeling.

1303*a*. But I rather gather that you think it would be very difficult to get a law that would be effectual, which would interfere with the research of a scientific inquirer in his own study?—I must say that my own impression of the English nature is that it is a law-abiding nature, and that a pronunciamento on the side of carefulness, even as regarded private practice of vivisection, would have its effect.

1304. (*Sir John Karslake.*) I gather from you that in your opinion some restriction put upon the practice of vivisection in schools would have its effect upon the practice of vivisection in private houses and for private research?—I think so; on the principle that Mr. Froude, I believe, lays so much weight upon, about embodying the public opinion in the permanent form of legislation. That is a great matter. It, so to say, crystalises it into a solid basis which serves as a stepping stone in the evolution of moral sensibility upwards.

1305. May I take it that you would not recommend that all private research for *bonâ fide* scientific purposes should be prevented?—I should not wish that it should be prevented; but I will not say that I think it would be inexpedient to legislate for it as well as for the other. I do not feel so confident as to the good that would be done by legislating for it in private, Mr. G. Rolleston, M.D.

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as I do about the legislating for it as regards public classes.

1306. Now may I ask you this: in your opinion is there no case in which experiments of this sort should be tried before classes?—Painful experiment should not be tried before classes.

1307. None whatever?—None whatever of the painful ones. I would just say that there is a great difference I am sure between an experiment shown to three or four people and one shown to thirty. It may seem absurd to say so, but I am confident that it is so; and I would repeat with Lipsius—"*Ita est.* "*Queri libet de perversitate naturæ,*"—"You may "complain of the perversity of nature, but that is what "nature is."

1308. Then as I understand it, in addition to allowing experiments of this sort for the purpose of private research with a view to scientific conclusions, you would not object to them where the exhibition of the experiment was made between three or four persons who desired to see the experiment made, and had a real object in wishing to see it made?—Yes, that is, I think, the position I should take.

1309. But the objection you would have would be to allowing a large number of students *in statu pupillari* to see the experiments made?—If it was a painful one.

1310. (*Mr. Forster.*) Does that mean that although you would think it unadvisable to allow, or rather that you would prevent experiments before a class of the size of which classes generally are, you would not object to the teacher doing them before his pupils in smaller groups?—Yes, I should certainly; because the addition of three, four, five, six, or seven classes in that way would of course be equivalent to the whole thing, and that would simply be an evasion of the rule. What I mean is just this: that it is possible that under certain circumstances it may be for the public good, and may be legitimate that two or three advanced students, not students working for an examination, but students of the other kind, the men indicated by the word "students," should assist the professor in original research; and now I would say that it might be advisable that legislation should be employed for such exhibitions as those, but that in their case the legislation should be less stringent. It is clear that it is an impossibility and a contradiction in terms to think that you can have a lecture room full of students who are going into original research; out of 100 men or even 100 men who are students it is a question whether you will find one who has a gift for original research.

1311. (*Sir John Karslake.*) You would not object to persons either alone, or with the assistance of one or two others, practising these experiments with a *bonâ fide* view to scientific research?—I would not.

1312. Then you would object to legislation directed against that state of things?—I am not clear that I should object to it; I do not feel myself prepared to object to legislation for that. What I have made my mind up about is the positive thing that it is right that legislation should affect classes. And now that you bring me to face this question (not more directly than I have faced it before, but by going over the ground again and again, one gets precision in one's views). I think that legislation might be brought to bear even upon that other application of vivisection.

1313. (*Mr. Forster.*) Should I be right in gathering from what you have stated that while you would by legislation prevent painful experiments on living animals in the presence of classes as usually constituted, you would also advise legislation with regard to experiments for research, either by one man or by a man assisted by advanced students, but that in that case you would not advise absolute prohibition, but only some regulation?—Quite so; that is the view of it that I have come to and tried to express.

1314. (*Sir John Karslake.*) Going back for a moment to the exhibition of these experiments in schools, you say that you would not allow painful experiments there?—No.

1315. Would you allow experiments which might be considered necessary, and which could be performed when the animal was under anæsthetics?—If it is certain that the animal is under an anæsthetic, and if it is also understood, as I have said at the commencement of my giving evidence, that the animal is put out of life before it returns to sensibility, the animal may then practically be considered as dead, albeit for physiological purposes it is a living machine.

1316. In order to see what would be done by way of legislation, you have to define in what cases these experiments might be made before a class?—I should doubt whether legislation should do that. I think that profusion and extravagance should be guarded against by legislation even as regards experiments performed with the precautions specified.

1317. But could you give us any form of words which would meet your view with regard to that, so as practically to have the effect of preventing excess while permitting experiments up to a certain point and under certain conditions?—I have not considered the subject quite practically. Perhaps it would be necessary to have something like a standing provision, that there should be returns as regards experiments performed in any particular laboratory before students.

1318. Do you think the case would be met by providing that in every school of this description there should be a register made of the experiments which are from time to time made in the school, and the reasons for which those experiments are performed, or the objects for which those experiments are performed?—Yes, that is what I meant.

1319. Would that in your judgment be sufficient with the aid of public opinion to prevent abuse?—Let me say that I have a school of human anatomy in my hands. If I go down as I intend to do to Oxford tomorrow I may meet one of my servants when I go into the museum who will say to me "the inspector of anatomy is here." If so he has come without any consultation with me at all, and has just come to see whether any abuse has been perpetrated. I am entirely amenable to inspection, and have no right to feel in the least annoyed by it. I think that in addition to a register, inspection of some kind is a thing which is desirable.

1320. That there might be the power to allow an inspector at any time he thought right to enter these schools to see what was going on?—Yes, and that in addition to that a register should be kept. I send a register of all bodies dissected, though they are dead ones; and in addition I am amenable to the visit of an inspector.

1321. That gives us a definite notion of what you would propose with regard to these schools?—I have not considered that point as fully as I ought to have considered it, and consequently I am not prepared to say in a matter of detail of this kind that that is the wisest plan for securing it. It is a plan which I have heard of before, and I do not see any objection to it at the moment; but, of course, if I had more time to think over it I might or I might add to or alter it.

1322. I do not ask you to pledge yourself to that being the best plan; but it would be a plan which would go in the direction which you would wish?—Yes.

1323. That plan would not apply in any way, as far as I can judge, to the case of those who in their own houses make experiments with a view to scientific information, private research as you have called it?—No.

1324. Does it suggest itself to you that there could be any legislative control in those cases?—I really am of opinion that most experiments worth doing would be done in a public laboratory, and that very few cases of experimentation in private houses would take place at all; and consequently I think that no great inconvenience would arise from compelling all experiments to take place in a respectable laboratory, a laboratory amenable to the visits of an inspector.

1325. No inconvenience would arise from that you think?—Very little. Of course it is possible that

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you might have people, such as Mr. Boyle was 200 years ago, who might wish to start a private laboratory, and so on; but then *de minimis non curat lex.*

1326. Then it may be that legislation would not be expedient in those cases, because they are among the class which you describe by the term "*de minimis?*"—Still the inconvenience that persons would experience, who would start laboratories if there was no provision of the kind supposed, would be so small that it would not be a thing which should weigh in legislating as to having experiments performed in places amenable to inspection.

1327. Let me put a case to you which has been put before this Commission. A person living in a remote part of the country suddenly dies with symptom of poison, after having partaken of some particular food. A medical man, living at a distance from any laboratory, such as you suppose might exist in the country, tries the effect of that food, which was supposed to have caused the death of the human being, upon an animal, in order to ascertain whether it would cause death by poisoning in that animal. Would you say that legislation should interfere with that experiment?—I should certainly have thought that as men run to and fro by steam, and knowledge is increased by the electric telegraph, the suspected thing might be sent up instantly to a laboratory. Certainly in these cases we have had abundant evidence that things can be sent up to experts in medical jurisprudence; and *ipso facto*, if it is in a lone and desolate country, is a person there likely to have the means and appliances for testing a poison available except such as giving it to a dog?

1328. That is the very thing I am suggesting, that you would give it to a dog or a rabbit for the purpose of ascertaining whether it causes that animal to die, apparently with symptoms of poisoning?—Very few poisons, except such as prussic acid, are volatile or decomposable in such a way as not to be testable afterwards; and even that is preservable for testing purposes. But I should have thought that a case of that kind is not one that should guide legislation, for it can but happen in the very nature of the case very rarely.

1329. But supposing you lay down some rule, such as I understand you to suggest, that no person shall, even for the purposes of private research or experiment, perform an experiment on a lower animal, except in a public laboratory, or an authorised laboratory, then you know the person who performed the experiment, which I am suggesting for a *bonâ fide* object, would come within the law, and be liable to the penalties of the law?—I quite see that; but then I must say that that which is the third recommendation of the Liverpool Congress is one which I never felt very strongly about. I do not feel it to be of the same cardinal consequence as the other thing. I see that there are difficulties that beset that. Then in these questions you must weigh one set of disadvantages against another, I apprehend.

1330. I only wanted to know what was the balance in your mind; would you still say that there should be no private research at all, except in authorised or registered laboratories?—I should be sorry to say that, I think.

1331. (*Mr. Forster.*) Do you not think that it would be possible by law to say that there was to be no such research, except in authorised laboratories, and to make a proviso that the penalty should not apply in the case of an experiment made with regard to any person who had been poisoned?—Quite so. You are good enough to supply me at once with the answer; but I again say, in justice to myself, that it is not my business to give advice about police regulations, and what you have suggested seems to me to cover the case.

1332. (*Mr. Erichsen.*) Have you been able to form any opinion as to the extent to which experiments on animals are carried on in private?—No; but I do know that experimentation on animals is carried on in private by scientific men.

1333. Could you give us any information as to the physiological laboratory at Oxford? Are you connected with it?—Yes; I am the professor of anatomy and physiology, both.

1334. In that laboratory are students,—I mean students *in statu pupillari*,—allowed to make experiments, either independently or under the supervision of the teacher?—No, no student *in statu pupillari* has anything to do with experimentation on living animals in the University Museum in Oxford.

1335. And it would be erroneous, I suppose, to say that there was anything like a reckless system of experimentation carried on at Oxford?—Most assuredly; I should be responsible for whatever of the kind was done in the university department of physiology.

1336. Perhaps you would kindly explain to us the way in which the experiments are conducted in that laboratory?—I do not show students who are going to be examined, who have that sword of Damocles hanging over their heads, experiments upon living animals at all. As a matter of fact, I might refer to evidence of mine in reference to the necessity for increasing the professoriate at Oxford, and that there should be not only, as there is at present, one professor to deal with two subjects, but two professors to deal with the two subjects of anatomy and physiology. Some few experiments on living animals have been carried on in my laboratory, but they have not been for students *in statu pupillari;* and then one and all were for research, or every now and then there may have been one for showing some cardinal point of physiology. They have been carried on under anæsthetics.

1337. The "students" in point of fact, then, are medical men who are studying the higher branches of the profession?—Not quite medical men. At Oxford a large number of my students are not going to be doctors at all. Sometimes half may have been persons who were going to take, say science masterships in schools, or something of that sort, or persons who simply saw their way to their B.A. degree through a science curriculum. We have that anomaly, and a very useful one it is, that persons can get a degree in arts by a training in science; and those come to me, a number of them, and others go to chemistry and physics.

1338. What you stated with regard to the emotional tone of mind induced by large assemblages of pupils witnessing experiments would go further, I take it, than experiments on living animals; it would apply also very much to the witnessing of operations in operating theatres?—Yes, I think it does.

1339. (*Mr. Hutton.*) You were saying that you thought there was a distinct growth of moral feeling in favour of greater care to avoid the infliction of pain. Now you apply that remark rather, I suppose, to the growth of centuries than to the last few years?—It is rather the reverse, I should think, within perhaps the last few years. There are eddies in a river which is flowing from east to west, but it is flowing from east to west all the same.

1340. But do you not think that the introduction of so many physiological lecturers who have studied on the Continent has produced a school of quite a different character in the physiology of the present day in England?—I should like just to say this upon that point: Dr. Child, who has been a practising doctor, and is now an exceedingly useful inspector of health (he has one of the large divisions, viz., Oxfordshire), wrote a book about some 10 years ago, in which he dealt with this subject of physiological experimentation. I had some talk with him about it, and he said to me distinctly that if he had now to write that essay over again, he should recommend legislative interference, which he then thought was not necessary. He has written a book of physiological essays and so on, and is a person of an active mind, and also he has a quantity of business to transact which brings him

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into relation with other men on questions of this kind. I presume that answers your question.

1341. (*Mr. Forster.*) And did he come to that opinion because he thought there was so much greater inclination to try experiments that the necessity for legal regulation was greater ?—That is just the case. I may add that I asked him whether I might quote him, and I asked him whether he would be prepared to give evidence before the Commission to that effect, and he said he should be very glad to do so.

1342. (*Mr. Hutton.*) You gave us the idea that your own experiments have been on a very moderate scale indeed ?—Very.

1343. Can you give us any idea of the actual number of your experiments on living animals in a year ?—Some years there are scarcely any at all. I do not profess to be an experimental physiologist, and I do not think that the educational value of it would be great for my purposes, for I am not more a teacher of young doctors of the future than of ordinary English gentlemen. I have an human anatomy class; but certainly I do not profess to teach experimental physiology, that is to say, if "Physiology" is to mean what it is made by some professors of it on the Continent, namely, nothing but vivisection. If you take that book of Schiff's up, you will find that almost every lecture has some animal sacrificed for it; there are 700, that is more than two a day experimented on, according to report, every year there.

1344. I am going to ask you a question on a rather delicate matter, but do you not consider that this handbook before us introduces quite a new enlargement of the practice of vivisection into the teaching of physiology ?—No doubt; but I should say about that, that I am very well acquainted with one of the authors of it, namely, Dr. Michael Foster; he is a person of, it seems to me, an exceptionally refined mind, and what is safe for a man of an exceptionally refined mind is not safe for the ordinary mass of mankind, and it is with the ordinary mass of mankind that legislation deals.

1345. I have often heard that about Dr. Foster, and I believe it to be perfectly true; but amongst his experiments there seem to be some of the most questionable, and, as far as I can see, some of the most needless that are found in that book. Now take the experiment on the reflex action of a frog at page 409 :—"As an instance tending directly to the contrary supposition, the following experiment may be performed :—In a shallow glass or porcelain dish, place enough water to reach up to the head of a frog. Line the sides and bottom of the vessel inside with felt or blotting paper. Place an unmutilated frog in the water, and then gradually raise the temperature. Cover the top of the vessel with a piece of guage or netting, to prevent the escape of the frog. As the temperature rises the frog becomes uneasy, and after 20° C. or 30° C. is reached, makes violent attempts to escape," and so on. I suppose that that frog is practically boiled alive, is not he ? 35° centigrade is, I suppose, practically much the same as about 95° or 100° Fahrenheit, is it not ?—Yes.

1346. Having regard to the low temperature of the frog's blood, that is very much the same as if we were put into boiling water, is it not ?—Well, I am not quite clear about that. Albumen is albumen and coagulates at 140° of Fahrenheit you know, which is considerably above it, and the boiling point 212° F. is necessary for producing a blister in us. A frog would be killed, as cold-blooded animals usually are, by continuance in a medium such as water at 100° F., but it would not be boiled or blistered. I am not quite clear that that is an experiment, such as you would call "most questionable," but I should say it was needless to repeat it. While you are on this point, I do not know whether I ought to say it, or not, but I spoke to Dr. Foster about one experiment which is frequently referred to, one mentioned at page 403, of testing recurrent sensibility. Now Dr. Foster himself told me that not only had he never shown it to any of his pupils, but that he had never seen it himself; and I must say that I should be sorry to see that experiment myself.

1347. But surely it is put here in a handbook in a mode which would encourage the trying of that experiment ?—Obviously. I am speaking in vindication of the character of a friend of mine, but not at all in vindication of the book.

1348. (*Mr. Forster.*) You have read that handbook ?—Yes.

1349. It would give me the impression, and I should like to know whether you think it would be a correct one, that experiments on living animals are suggested there in just the same way as any other experiments might be suggested ?—Yes. I should like to say with reference to the amount of anæsthetics employed, that I have tried myself a number of experiments upon the action of anæsthetics, and I must say that it is not so easy a thing to know when you have an animal thoroughly anæsthetised; and what is more, some animals recover with much greater rapidity than others of the same species from the same doses of anæsthetics.

1350. Many of the experiments there are suggested to be performed under curare; would you consider that an anæsthetic ?—The very Handbook itself shows it is not if you look at the experiments about it, p. 395–396; but I would say this, that I am informed that there quantity comes in. I have done experiments myself with a small quantity of curare, enough to make the animal quite quiet; and then it would recover from it, and there was nothing painful done after it. I beg to say this, that I am strongly inclined to think that a very large dose of curare would be probably an anæsthetic. But I must say that the whole question of anæsthetising animals has an element of uncertainty about it. Some animals are, some are not, within the limits of the same species, amenable to anæsthetics just as we are ourselves.

1351. (*Mr. Hutton.*) Then I understand that your opinion about the handbook is that it is a dangerous book to society, and that it has warranted to some extent the feeling of anxiety in the public which its publication has created ?—I am sorry to have to say that I do think that is so. I do happen to know not only Dr. Michael Foster but I know also Dr. Sanderson, though I do not know him as well; but I think that their book represents them as being something other than I happen to know one of them at all events, Dr. Michael Foster, is.

1352. But now, in that book the experiments on frogs are very frequent, and very many of them I find are performed under curare only ?—I daresay that may be so; but I think if you look at the beginning of Dr. Foster's part of the book you will find that he does pith frogs.

1353. He does in some cases, but not in all ?—The particular department he has dealt with, the electro motor department of physiology, is not one that I am an adept in. I have read what he says, but it is not one of my subjects. It ought to be, perhaps, but it is not.

The witness withdrew.

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MR. JOHN SIMON, F.R.S., called in and examined.

1354. (*Chairman.*) You are the medical officer of the Privy Council and the Local Government Board ?—I am.

1355. You are a surgeon, and a Fellow of the Royal Society ?—Yes.

1356. Has your attention been particularly called to the subject into which we are appointed to inquire ?—It has.

1357. Chiefly, I think, pathologically ?—Chiefly so, of late years.

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1358. Perhaps you can give us a few illustrations of the subjects which in your official capacity your inquiries have been particularly directed ?—In my official capacity I have to think chiefly of the prevention of disease. For the prevention of disease the first necessity is to have knowledge of causes, the more exact the better; and in working out exact knowledge of causes of disease it is occasionally requisite, in parts of the subject, to administer experimentally to animals the influences which we are studying in that relation.

1359. I believe the particular subjects which have occupied you are cholera, tubercle, pyæmia, sheep-pox, and disinfectants ?—They have been among our chief subjects.

1360. I think there is an annual grant made, of which the distribution falls under your charge ?—There is an annual grant of 2,000*l.* to the Privy Council Office for scientific investigations in aid of medicine.

1361. Speaking broadly, how is that divided ?—At present the 2,000*l.* is first of all divided into two halves. The one half would have nothing whatever to do with the present question; it is spent entirely on work of the chemical laboratory. Only the other half is in that class of studies where sometimes experiments on animals would come in.

1362. Namely, anatomical ?—Rather, pathological.

1363. Is a great part of that done with the dead subject ?—A great part is on the dead subject.

1364. What sort of proportion do you think is on the living subject ?—I have no way of stating proportions exactly, but I would give you some illustrations of experiments which have been performed on living animals. A few years ago it was necessary for us to follow, as far as we could at that time, the subject of the infectability of animals by choleraic discharges; and a considerable number of experiments were then performed by the administration of extremely minute doses of cholera-discharge to the subjects of experiment. The subjects of experiment were mostly mice.

1365. Then there was no pain of the knife in cases of that kind, I suppose ?—No pain of the knife.

1366. Any distress that was caused there was distress from other causes ?—Bowel complaint.

1367. Are there any rules or minutes of the Privy Council which govern the application of this money ?—At the end of 1873, when I was making a provisional report of some of our work, forestalling that which was last year laid before Parliament as No. 3 of the new series of my Reports, Mr. Forster made a special note on the use of animals in some of the investigations. His observations were the following:—“Upon being informed that there were experiments on living animals connected with the Auxiliary Scientific Investigations, I had to consider how far I could make myself in any way responsible for these experiments. After careful consideration, I came to the conclusion that however much I might dislike vivisection I should not be justified in preventing it if practised, not only for the purpose, but with the reasonable ground for expectation that it may result in discoveries which will diminish disease in men or other animals, provided that every precaution against pain be taken of which the experiment admits. As such discoveries are the object of these investigations, and, indeed, of the vote, I felt I could not require these experiments, when really necessary, to be discontinued; but I thought it right to require that they should be conducted with every endeavour to avoid pain, and especially with the use of chloroform or other anæsthetic. I found that these experiments were thus conducted, as indeed I expected would be the case, knowing as I do the desire of Mr. Simon to avoid giving pain to any animal; and it is therefore merely as a justification for myself that I desire to have on record the opinion upon which, had I remained in office, I should myself have acted, viz., that no experiments on living animals should be conducted at the cost of the State without the employment of some anæsthetic in case of painful operation, and without a report from time to time by the gentlemen conducting the experiments, explaining their object, and showing their necessity for the purpose of discovery.” That is dated the 17th of February 1874, and signed “W. E. F.” It, of course, subsequently came under the notice of Mr. Forster’s successor in office, Lord Sandon; and for his and the Duke of Richmond’s information I afterwards submitted an explanatory minute which I am now authorised to lay before the Commisson. It is dated February 26th of this year, and is as follows:—“In modern endeavours to increase the power of preventing different diseases of man and domestic animals, usually a first aim is to obtain exact scientific knowledge of the causes and the mode of attack of any disease which is in question; and in this sort of study it frequently happens that more or less experiment has to be made as to the results which the administration of a particular influence will produce on an animal. In further aid of preventive medicine, and often in aid of therapeutics, experiments on animals are also from time to time wanted, to test, as against known causes or processes of disease, the value of alleged prophylactics, antidotes, and remedies.—Studies of this experimental test are made sometimes more immediately in the interests of man, as, for instance in the case of Asiatic cholera, and sometimes more immediately in that of the domestic animals, as in the cases of sheep-pox and cattle-plague, but perhaps oftenest in the common interest of both; for, in regard of certain large quantities of disease, it may be assumed that any better knowledge which is got will probably sooner or later yield equal advantage to human and to veterinary medicine. If in such experiments it were ever necessary to perform on any animal any severely painful operation, chloroform or other anæsthetic ought of course to be used; but, in fact, experiments of the class referred to have generally consisted only in inoculating animals (by mere puncture) with some specific material, or in giving to them in their food the material of which the effect has had to be watched, and perhaps afterwards taking from them blood for examination, operations which in themselves are little or nothing more than the vaccination or bleeding of the human subject. No doubt, however, but that pain or uneasiness will afterwards arise in cases where disease is the result of the experiment; but in experiments which are not of curative intention, the experiment would often require that the animal should be killed for examination as soon as disease had become manifest, and of course no animal would needlessly be let live in a state of suffering.—That experimental studies of disease are of the utmost importance to the progress of medicine, that indeed such progress must at present in large proportion depend on them, is, in my opinion, quite certain. For myself I may say, that, obliged as medical adviser of the Government to make myself as proficient as I can in all that relates to the preventing and resisting of disease, I have felt it indispensable that I should have recourse *inter alia* to such studies; and in some of the Scientific Investigations which are made under authority of the Privy Council in aid of pathology and medicine, and which I have the honour of superintending for their Lordships, such studies have necessarily formed a part.—I can confidently say that in any experimental use which has thus been made of animal life in the medical interests of mankind, conscientious regard has always been had to the limit within which such use can alone, in my opinion, be justified. In view of the universal practice of mankind in other relations to the lower animals, and under sanction as I believe of sound reason, it has been assumed that, for the sufficient advantage of man, suffering may be inflicted on brute animals; but the limitations have been well remembered, that no such suffering should be inflicted except in endeavours for considerable human advantage, nor except with all proper care to make the suffering

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" as little as can be.—At the end of 1873, on occasion " of a provisional statement which I then made (and " which has since been embodied in a report laid " before Parliament) on the scientific investigations " which were in progress under the Privy Council, " Mr. Forster asked me to explain to him how I " stood in regard of the performance of painful " operations on animals; and on the explanation which " I then gave to him in accordance with that which " I have here written, he wrote remarks of which copy " is above. I communicated Mr. Forster's minute as " an instruction to the gentlemen who act with me " in pathological investigations." Adverting to the change of ministers which has taken place, I may mention that of course Mr. Forster's minute is still understood to be in force. In the present connexion it may be convenient that I should now put in a return recently moved for by Mr. Dillwyn showing our last four years investigations. It is No. 223 of the present session, and is dated 27th of May 1875. [*Paper handed in.*]

1368. That opinion which you have expressed, and which I may divide into two parts, namely, first that such experiments are necessary, and secondly that they ought to be conducted with all possible regard to humanity, I understood you to say that the general opinion of the eminent medical men of this country would concur in ?—Certainly I believe so, but I say this with special reference to such of them as have thoroughly considered the subject. Some eminent men may have never considered parts of it.

1369. Do you consider that the practice in this country is extensive ?—It must be confined, I should suppose, to very few persons.

1370. And those the most competent ?—Yes.

1371. And ought to be so confined ?—Yes.

1372. Do you think it is increasing ?—Parts of it are probably increasing, other parts probably diminishing. I should think that probably the more painful sort of experiments which I dare say the Commission has had particularly in view, and which may be described as severe surgical operations on the animals, are both greatly fewer in proportion to the number of physiologists, and greatly fewer in proportion to the range of physiological research, than they were in the times of Magendie.

1373. Then upon the whole, of operations which are in themselves calculated to produce severe pain, you do not think the number is materially increasing? —I should think not. It is very difficult to judge that, and I cannot profess to speak with certainty.

1374. And then the introduction of anæsthetics must have materially affected the question which we are considering ?—Very materially.

1375. May almost all severe surgical operations of this character be performed when the animal is entirely unconscious ?—I should think nearly all, certainly a very large majority.

1376. And as to the rest, is it possible to perform the severe parts under anæsthesia, although the result afterwards may not admit of the continuation of the anæsthesia ?—Yes; local anæsthesia can also, in some cases, be produced where general anæsthesia could not be.

1377. Is it your opinion then that very few operations could now be justified on the ground that the infliction of severe pain was a necessary element of the experiment ?—The Commission would have better evidence on that point from the actual prosecutors of such experiments, or from some chief modern physiologist, such as Dr. Sanderson, than it would have from me. My impression is, that it would rarely be necessary for a physiological purpose that the animal should continue sensible to pain during a severely painful operation.

1378. Now such being the state of the case, and such the general sentiment of the eminent and competent men of the medical profession, do you think that there are any other persons who are neither eminent nor competent, and who may nevertheless be disposed to resort to these practices ?—I cannot say that I have any experience of such. There may perhaps be imagined an occasional boy or girl who tortures a dog or cat under the notion of doing physiology; but I can hardly conceive anything of the kind. I have no reason to believe that it exists to any appreciable extent.

1379. Then your belief would rather be that, if it were represented to us that experiments of a severe character were repeated, when there was no real necessity for the repetition, that is a mistake ?—I should think so.

1380. And if it is said that experiments, which might be performed under anæsthetics, are performed without care being taken to render the animal unconscious, you would imagine that that is a mistake too ?—Generally speaking, yes. There may be individual acts of carelessness: extremely few, I should suppose, in regard of any domestic animal; but more likely to occur in regard of frogs, as they are of far larger use for physiological purposes, and perhaps are also often ranked as less sensitive. I think one advantage of the sitting of this Commission may be that those of us who have in any way to use animals for purposes of experiment will ask ourselves even more strictly, whether we are doing all that we ought to do, all that we can do, whether we could possibly go further than we go, in preventing pain where we perform experiments. But I believe that as a rule much care is already taken to prevent pain to animals in severely painful procedures, and that at least in regard of domestic animals any such procedure would hardly ever, if ever, be undertaken except with use of anæsthetics.

1381. Naturally, as far as you are yourself personally acquainted with them, the experiments are in the hands of those who act in the spirit of the memorandum that you have been so good as to read to us, and you think that the most eminent and the most competent people in the country all act in the same spirit; but, of course, it is possible that there may be persons who think they are doing some good in practising these sorts of experiments, and who are not of the same either competent instruction and right minded sentiment as the eminent persons of whom you have spoken; but you are not aware of such; you do not think that there are any ?—Not as an appreciable class.

1382. Therefore your opinion is that the necessity for measures being taken by the Crown or by Parliament is not nearly so great as has been held out to the public of late ?—I think certainly not. It has seemed to me that the public has been led to form extremely exaggerated views of the extent to which the sort of thing is done in this country, and has not been made aware that, where it is done, it is done under reasonable precautions.

1383. If there were to arise sufficient evidence of such abuse, do you think that the competent persons, of whom we have been speaking, would be as ready, as any other eminent persons in the country, to support the Crown and Parliament in reasonable measures for the repression of those abuses ?—Certainly.

1384. But you have not yourself any belief that the necessity has arisen ?—No.

1385. Then I suppose you have not provided yourself with any proposals for remedies, as you do not imagine the necessity to exist ?—I have not. I have looked at the Act of 1849 for preventing cruelty to domestic animals, and at the two Bills which have recently been before Parliament. The Act for the prevention of cruelty to domestic animals was founded on a previous statute of, I think, 1836, and the preamble of that Act takes as its basis, that "many and great cruelties" are practised in driving and conveying cattle, and in slaughtering them, and keeping them without food and nourishment, "to the great and needless increase " of the suffering of dumb animals, and to the de- " moralization of the people." I have not a conception that there is anything answering to those terms which purports to be for the sake of physiology; but were it so, I would observe that under the Act it

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is made punishable to "torture" any domestic animal, and that if a physiologist should do an act which could properly be described as "torture" of a domestic animal he would apparently be within that provision of the law.

1386. (*Sir John Karslake.*) To cruelly torture any domestic animal, if you look at the whole sentence?—And the "*cruelly*" enables me even better to explain my meaning. I presume that it is open to any one to take proceedings under that Act of 1849 against any one found causing pain to any domestic animal for purposes declaredly physiological; and it would then be a question whether the causation of pain could be defended. It would not, I apprehend, be enough to declare that the intention of the procedure had been physiological. I presume that before the magistrate the person charged might fairly be called upon to show, (1) that he had a reasonable prospect of scientific use to mankind from the study in which he was engaged, and (2) that he was personally of competent skill and judgment in the study, and (3) that he had not wilfully or negligently omitted to use proper care for the prevention of the suffering to which his operation or experiment tended. I presume that such showing as this would exonerate him from any charge of having "*cruelly* tortured" under the Act; but that with failure to show to some such effect, the physiologist, like other men, would be punishable. Supposing that to be a right construction of the Act, I do not see what more should be wanted; but if that is not a right construction of the Act, I should not myself see the least objection to a statutory declaration in some such sense.

1387. (*Mr. Forster.*) Would the Act apply to many of the animals upon which experiments are performed, such animals not being domestic animals?—It would not apply to frogs.

1388. (*Sir John Karslake.*) Nor to white mice?—Nor to white mice. I do not doubt but that, as regards the question of moral obligation, frogs and mice ought to be safe against cruelty; but law does not pretend to be co-extensive with morals; and as regards the question of law, I do not see how the Legislature can intend to take care of other than domestic animals. Probably it will find even that a larger task than it can successfully manage; but, since the Act of 1849 affirms that as its principle, there could not I apprehend be any objection if it were thought necessary to explain the Act in relation to physiology in some such sense as I have suggested. Further than that I do not see how the law could go without going into unpractical refinements on the subject, and having sooner or later to draw a new arbitrary line where the grounds of distinction might be far less definite. Without referring to the fact that the limits of the animal kingdom downwards are somewhat uncertain, I may point out that there is no reason in the nature of things why a man should be allowed to torture, say, a slug or a snail rather than a rabbit. Lord Henniker, I think, draws his line at vertebrate animals; but I do not see why it should be permissible to "cruelly torture" a slug or worm rather than a rat or mouse, and I think it would be practically impossible to afford legal protection to any but domestic animals.

1389. (*Chairman.*) But supposing that by the skill of the draftsman, and the power of Parliament, that difficulty were overcome, it would appear, I infer from your last answer, as reasonable in itself to protect the rat or even a slug, if the slug were susceptible of the same degree of pain, as to protect a sheep, or an ox, or a horse, would it not?—My meaning is to distinguish between what may be reasonable in sense of morals, and reasonable for matter of statute.

1390. You have read to us the interesting memorandum drawn up by yourself for the Privy Council which contains excellent sentiments with regard to the treatment of some animals; and I understood you, in your last answer, to say that there was no very clear ground why one animal should be subjected to suffering and not another. If then we find that it is possible to overcome technical difficulties which stand in the way, we should have you on our side, should we not, in overcoming them?—Any law which I should think right, as restricting the physiologist in relation to a dog, would, I should think, be theoretically right in the same relation to any other animal.

1391. But when you got very low in the scale of creation you would perhaps as a physiologist doubt whether the same amount of suffering was inflicted by the same proceeding?—I apprehend that Parliament could not proceed on speculations of that kind; but a high authority says that the "beetle which we tread " upon in mortal sufferance feels a pang as great as " when a giant dies."

1392. The object of that particular sentiment is rather to make giants less careful about their corporal sufferings than to say anything whatever about beetles, if I remember the application of it?—But it cuts both ways.

1393. The words are more usually applied in the way in which you apply them, but I think they are not so by the great authority to whom you refer. But reverting to the question, the particular mode in which you have thought that the law, perhaps, might be amended is by leaving cruelty to be dealt with by the judges or the magistrates before whom the case was brought?—Yes; that the Act of 1849 might, if necessary, have statutory interpretation in regard of inflictions of pain for purposes of physiological study.

1394. Supposing any sensible magistrates to be dealing with the case of a highly sensitive animal, they would apply probably one degree of judgment to that animal, whereas if the animal was a jelly fish, or any of the things that border upon the external scale of what we commonly call the animal creation, they would apply a very different degree, would they not?—No doubt; but I am not sure whether they would always be right in their scale.

1395. So that if your mode of dealing with the subject were adopted, namely, an amendment of the statute against cruelty, the statute would become elastic, if it applied to all animals, and the elasticity would be dealt with by the tribunal?—It would be so; but perhaps penal laws are hardly meant to be elastic.

1396. (*Mr. Forster.*) With regard to the experiments which you conducted, they are chiefly, almost entirely, pathological experiments, are they not?—Entirely; I am not aware that there has ever been any severe operation done.

1397. In my minute which I left, I gave an opinion that there ought to be from time to time a report with regard to these experiments. Has there been such a report?—There is the blue book before you, which I have just handed in.

1398. How far does that blue book give a report of the experiments?—So far as experiments are done, they are mentioned in the separate papers there. If you find no experiments mentioned, you may take for granted that no experiments have been done.

1399. On page 49 I see a mention of communicating sheep-pox by inoculation to a number of sheep?—Yes. Similarly in the paper on tubercle, mention is made of experiments.

1400. I suppose I am to understand that you now think it incumbent upon you, in accordance with my minute, which has been adopted by my successor, to make no experiments respecting which you do not give a report?—Experiments are only done in contribution to some particular research; the research is reported, and the experiments, or the substance of them, in connection with it.

1401. Then I am to understand that no experiments are conducted without a report relating to them?—Quite so.

1402. Have you interpreted the minute which I left to mean that any description of the number of the animals operated upon should be inserted?—Certainly not; its words do not tend to any such meaning.

1403. Then what did you understand that it did mean?—I understood it to mean that I should do

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what I have done in my Report No. 3, now lying on the table.

1404. What did you consider it necessary to insert? —Such reports on the researches conducted as would, where experiments have been performed, render their scientific purpose manifest. And I may add that any scientific report, dealing with experimental matter, almost necessarily includes a statement of the number of experiments on which its conclusions are founded.

1405. (*Sir John Karslake.*) And the kind of animals upon which the operations were performed? —Yes.

1406. (*Mr. Forster.*) What advantage do you think has been obtained for sanitary purposes by any of these experiments?—First, as regards the cholera experiments, all the advice that I give to the public about cholera refers to the infectiveness of the discharges; and the infectiveness of the discharges can hardly be said to have been directly tried on any but lower animals.

1407. In what way do you consider that you have obtained information which will be really useful for sanitary purposes?—By improved knowledge of the causes of disease, and thus gradually showing people how best to avoid the diseases which have been under experiment.

1408. (*Chairman.*) That is, you tell the public that if the discharges which proceed from cholera patients are permitted to get to other people, those other people will in all probability have the cholera? —Yes.

1409. And that you have found out by actually testing it upon animals?—That is my belief; but I prefer to rest on the general principle that, in the unanimous belief of followers of medicine, a more exact knowledge of the causes of disease is our hope, our essential hope, of being able to prevent them.

1410. (*Mr. Forster.*) Then your experiments have been rather to increase the range of mere physiological knowledge than experiments in order to guard against a special disease, or to remedy a special evil?—I put the justification on general grounds. In a particular instance I will justify the experiments, if you please. In cholera we believe that experiment has furnished an exact rational basis for the prevention of the disease.

1411. Then I think I must ask you again, in what way have these experiments enabled you to give advice to the Government or to the public to avoid the cholera or to check its spread?—In that case thoroughly.

1412. Will you tell us how they have done so?— By showing us, for instance, with what extreme watchfulness we must keep even the slightest taint of choleraic excrement out of drinking water.

1413. (*Chairman.*) You administered very small doses of choleraic discharges to living animals, and by that administration of very small doses you discovered that the disease was communicated to the animals; is that so?—Yes.

1414. And from thence you have drawn the inference that it is of the utmost importance to the health of the public to prevent even very small doses of that discharge from being communicated to the human frame?—Quite so; and all our practical advice to the public as to how they shall deal with individual cases of cholera when they occur, and the precautions they shall take against the spreading of the disease, refers to that experimental basis.

1415. (*Mr. Forster.*) Independent of this information gained by experiments on living animals, there was some reason to come to that conclusion from observation of attacks of cholera, and the way in which they had appeared to spread from one patient to another, was there not?—I can hardly say that. Till these experiments were performed in Germany, and had been repeated here, I may confess for myself that I did not consider it to be proved that cholera spread in that way. It had been suspected; it was a theory of cholera which began in 1849, in the teaching of Dr. John Snow; and the theory has been converted by experiments into such degree of certainty, as can properly be held to justify the advice of a Government Department.

1416. Then do you consider that without these experiments upon animals either by you or by somebody else you would not have been able to assert as you now think you can assert, with certainty, that an infinitesimal portion of choleraic discharge may, by getting into the water which a man drinks, give him the cholera?—I think the knowledge could not have existed without experiment.

1417. Now, with regard to experiments that have been made under your direction upon animals for the prevention of disease in the lower animals themselves, what cases can you give us?—I have very great difficulty, insuperable difficulty, in explaining the position clearly in popular form, and that is why I preferred to rest upon the general scientific principle. We are going through successive stages of endeavour, and are at present in but an early stage; we are trying to amend and develop our medical knowledge in accordance with rules which have been discovered to work well for all other branches of knowledge, trying to make our knowledge quite precise. In regard of sheep-pox, we have been getting, as you will see in that blue book, exact knowledge of the contagium of the disease. Hitherto, I fully admit, we cannot prevent sheep-pox otherwise than by the quarantine arrangements which we could have practised before those experiments were performed; but as I said, we are going through a progressive work which has many stages, and are now getting more precise knowledge of the contagium. By these experiments on sheep it has been made quite clear that the contagium of sheep-pox is something of which the habits can be studied, as the habits of a fern or a moss can be studied; and we look forward to opportunities of thus studying the contagium outside the body which it infects. This is not a thing to be done in a day, nor perhaps in 10 years, but must extend over a long period of time. Dr. Klein's present paper represents one very important stage of a vast special study. He gives the identification of the contagium as something which he has studied to the end in the infected body, and which can now in a future stage be studied outside the body.

1418. But up to this time have any of your experiments led to a result which would enable a veterinary surgeon to treat sheep-pox with more hope of cure than he did before?—No.

1419. But you hope that they will do so?—Yes, or rather with (especially) more hope of prevention.

1420. Now, as regards consumption, do you consider that there has been any practical advantage in your experiments with regard to consumption?—We have gained very great advance in knowledge. I again rest on the general position, warranted by all scientific analogy, that with such advance in knowledge we shall necessarily get results in practice.

1421. I am well aware that in medicine as in other sciences, perhaps more than in any other science, the increase of knowlege is most desirable, because from it may come practical advantage; but it is no proof that you ought not to endeavour to obtain that knowledge that up to this time you have not arrived at practical result; but it would be very important to know if practical result has been obtained. Has any practical result been obtained with regard to consumption in the same way as with regard to cholera you have shown that there has been a practical result?—We have gained the knowledge of the communicability of the disease from subject to subject.

1422. (*Mr. Hutton.*) Do you mean by contagion? —By certain ways of contagion; and that will of course have a very great influence in the advice to be given in reference to the disease; but to speak of that as an immediately practical result is, perhaps, not putting it quite correctly.

1423. (*Chairman.*) I understand that you now find that keeping two sheep together you can get tubercular disease in one that was sound before; is that so?—What we know of it is that it is inoculable from subject to subject.

1424. (*Mr. Forster.*) Would what you have arrived

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at induce you to form any opinion one way or the other as to the danger of a healthy person being in close communication with a consumptive person?—It would undoubtedly do so; and I will ask you to observe the particular emphasis which experiments of that sort give to a passage, which I will read from page 7 of the blue book already referred to:—"In "connection with these experiments it deserves "notice that within the last few weeks Dr. Hermann "Weber has brought before the Clinical Society some "facts of a strikingly suggestive kind as to the "possibility of tubercular infection being freely com- "municated to women through their conjugal rela- "tions with men who have pulmonary phthisis in a "quiescent or very chronic state." Dr. Weber had brought before the society several cases of men who, within his own knowledge, marrying in succession two, three, even four healthy women, with no family predisposition to phthisis, had had each wife in turn die of the disease; and the man in each of these cases had apparently from the first been tubercular. The experimental inquiry has opened an immense field, a field of which it is impossible to see the eventual outcome, in the direction of preventive medicine.

1425. You are engaged now in investigations with regard to scarlet fever, are you not?—Yes.

1426. Are you making experiments upon animals in those investigations?—No.

1427. Why?—We have no reason to believe that scarlet fever is communicable to them.

1428. Does the fact that you are not able to make such experiments impede you in the investigation? —It might be a great advantage to us in the study of the disease if we could communicate it to the lower animals, so as to watch its earlier processes.

1429. And your object in making these investigations, I understand, is not merely to acquire knowledge but with the hope of acquiring such knowledge as would enable you, as their medical officer, to advise the Government how to prevent the spread of such a disease as scarlet fever?—Certainly; my opinion being, that scientific advice concerning prevention must in all cases refer to exact knowledge of causes.

1430. As regards these experiments it is your habit, is it not, to put the animal out of the pain of a lingering disease as soon as you possibly can?—Yes.

1431. (*Sir John Karslake.*) Is there much pain attendant upon the disease which you call sheep-pox? —I think it would be fair to assume that there may be.

1432. Are animals, to which you have communicated the disease, curable, or do you generally kill them?—The inoculated disease is usually mild.

1433. Then does the animal recover?—The animal would usually recover.

1434. So that its life would not be sacrificed?—Its life would not necessarily be sacrificed.

1435. But there would be temporary inconvenience and distress caused to the animal?—Yes.

1436. That would be the extent of it?—That would be generally the extent of it.

1437. I understood you to say that in your judgment severe surgical operations by way of experiment are rare at the present time. I think you were referring to Majendie and his times, and saying that as compared with those times they were rare now?—My own work of late years has not been at all in those directions, so that I speak with reserve. I think that you probably would be better informed on that subject by such a man as Dr. Sanderson. I believe that severe surgical operations for purposes of experiment are comparatively rare, and that, if done, they would be done under chloroform. I think that they probably form a very small proportion of the experiments which are done.

1438. Now, may I ask you, as an officer of the Government, have you had occasion to perform any severe surgical operations with a view to establishing any particular fact?—Never.

1439. That has not fallen within your province at all?—Not at all.

1440. (*Mr. Erichsen.*) There is another side of your professional career which has scarcely been adverted to, and it is this, that you are surgeon to St. Thomas's Hospital, and have been so for a great number of years?—Yes.

1441. And your attention has been greatly attracted to the science and progress of modern surgery during that period?—Yes.

1442. Would you tell me whether you think that experimentation on living animals has tended in any way to the progress of surgery during, say, the last half century?—I should say immeasurably. The Commission, of course, knows that medicine is essentially an experimental science, and we save ourselves from a great deal of needless experimentation on man by some experimentations on the lower animals.

1443. I would ask whether the elucidation of such subjects as the process of inflammation, and the repair of a wound in the lower animals, has tended to the advance of surgical science and practice in man, in your opinion?—So importantly, in my opinion, has such been its tendency, that in such questions as those referred to—our knowledge of inflammation and of fever, I myself habitually refer more to what has been learnt by experiment than to what has been learnt in any other way. We cannot get such intricate studies worked out with sufficient exactness in the human subject, though we give all conceivable attention to clinical work, and we are still constantly obliged to fill up the gaps, and to get supplementary means of clinical interpretation, from experiments on the lower animals.

1444. I suppose one may say that surgery is based, not only on clinical observation and pathological research, but also on physiological or pathological experimentation?—Yes; certainly.

1445. And that it has a triple basis in that way?—Certainly.

1446. There is another subject on which there has been some difference of opinion expressed before the Commission, and I should be glad to have your opinion on that point; it is with regard to experiments made on the ligatures of the arteries in animals, the experiments, for instance, that were made by Mr. Jones in the early part of this century, whether those experiments did or did not at the time that they were made tend greatly to advance surgical practice in connexion with the ligature of arteries?—I can only say that when I was a medical student, and our then professor of surgery, my master, Mr. Green, was lecturing on the surgery of arteries, Mr. Jones's experiments formed a basis from which he proceeded.

1447. And they formed the basis on which the practice of surgery has been founded ever since?—Yes.

1448. May I ask you if, antecedently to those experiments, even so great a master of his art as John Hunter erred in the method of the application of the ligature?—Undoubtedly.

1449. And lost patients in consequence of what we now know to be the erroneous method of applying them?—Undoubtedly.

1450. At St. Thomas's Hospital, with which you have been so long connected, there is a large medical school?—Yes.

1451. Is it there the custom to your knowledge of students, I mean of the young men, to be allowed to perform or to practice without permission, experiments on the living animal?—Not to my knowledge.

1452. Or to your belief?—Not to my belief.

1453. You have been connected with that hospital for something like 30 years, have you not?—I went there as a boy in a jacket 42 years ago, and have not left it since, except for an interval of connexion with the medical school at King's College.

1454. And it would certainly have come to your knowledge somehow or other during that period if such were the case?—I cannot conceive otherwise; and at this opportunity I would beg leave to make a collateral observation. I observe a great deal of distinction drawn between experiments which are

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justifiable for one's own study, and experiments which are made for purposes of teaching; and this distinction is intimately connected with the questions which you are asking me. An essential difference between the scientific and the clerical professions is, that the scientific professions have to rest on actual sensible experience; education in them must be a matter of sense; a man must see, hear, smell, touch, handle for himself. When we are teaching our pupils, the universal rule which we have to give them, is that in all matters of sense they must as far as possible use their own senses. Now when that is applied to the matter of physiology, how is the lecturer to proceed with his pupils? Is he to say to them, "Such "and such is the action of the heart; go and verify "what I tell you, each of you separately for himself; "you 300, each of you, go and make this observation "on a frog?" or is he, once for all, to show it to the 300? Is one frog to do for the class, or is each member of the class to take a frog for himself? A physiological teacher (and this goes back to your question) could not fitly discourage his students in seeking under reasonable limits to see things for themselves; but experiments, in any considerable sense of the word, would certainly not be done, except under the direction, guidance, and sanction of the physiological teacher; and a single experiment shown by the teacher to his class may in some cases satisfy the requirements of study as fully as if the 300 students had experimented separately.

1455. Do you think it would be proper to exhibit to the class experiments, such as you have mentioned, showing the action of the heart if it could be done without pain, as it can be done?—Yes.

1456. By pithing the animal that is to say, or by giving it anæsthetics?—Yes.

1457. That the student would learn from such an experiment a vast deal that no description could possibly convey to him?—Yes.

1458. Or another experiment, such as showing the vermicular motion of the intestines, which can be shown in an animal that is dead, that has been pithed?—Yes; but I am anxious not all to underrate the real fact that the life of the animal is sacrificed for physiology; and I think that in discussing these things it is well to use the larger term, and frankly to accept as our responsibility that physiology does involve a limited sacrifice of animal life. Society at large uses animal life for its convenience; it does this to an extent so enormous that imagination cannot reach the dimensions of the use; and in this general use of animal life, the part which physiology plays is a fraction incalculably small.

1459. And many of the most eminent surgeons of the past half century have been in the habit, in order to elucidate certain points, of experimenting upon animals; such men as Sir Benjamin Brodie, Sir Astley Cooper, Mr. Travers, and numerous others?—Yes; and if occasionally there has been an instance of a surgeon who has done no such experiments, he, probably to a larger extent than others, has been an experimenter on his patients.

1460. Is such a man in your opinion greatly indebted for what he knows of science and the practice of surgery to the experiments of others?—Very greatly.

1461. Although he may not practice experiments himself, yet had these experiments not been done by others his knowledge would be of a very much lower standard than it is at present?—Very much so.

1462. In connexion with this subject you would say that physiologists, and occasionally experimenters, do not seek always to establish a new method of treatment or a new operation, but that they lay down a basis, or that they discover facts on which such new methods may afterwards be founded?—Clearly.

1463. (*Mr. Hutton.*) You entirely differ from an eminent witness we had on the theory of poisons yesterday, Dr. Taylor, who gave us the strongest possible evidence that he never found it necessary to exhibit the action of a poison on the living animal, and who stated that by the use of painted wax and various other models, he found it possible to give a complete idea of what was necessary for his students without exhibiting experiments at all on the living animals. You differ from that entirely, I infer?—He would be a perfectly competent witness on that branch of the subject. If he found that method of teaching sufficient for his students I have nothing to say against it.

1464. And as regards the heart, is it not possible to have an artificial heart which pulsates in precisely the same way as the human heart, and exhibits the same phenomena; indeed I have read an account of that in this very handbook before us?—You can imitate the action of the heart to some extent, but not, I think, in a way which would render it undesirable that the student should see an actual heart in action.

1465. As regards pyæmia have those experiments yielded much scientific result?—Yes. I shall very shortly be publishing some results on that subject.

1466. Experiments in pyæmia on the living animal I suppose are of a very painful character; I mean of very long duration?—The cases where the effects are most painful are cases of quickest operation, and the duration of the pain is shortest.

1467. The experiments are of very long duration, are they not?—In what sense "very long?"

1468. You have to keep the animal during the disease for a very long period, have you not?—In cases which are most typical the period would be reckoned in hours; but where the disease produced is of long duration you may take for granted that it is of low intensity, and consequently is less and less the cause of severe suffering to the animal.

1469. But you think that those experiments have yielded clear scientific results; do you mean therapeutic results, or only scientific results?—Scientific results in the most intimate relation to practice.

1470. Typhoid fever is a very bad kind of suffering, is it not, and yet it lasts five or six weeks?—It cannot be said that typhoid fever involves a severe pain of five or six weeks duration; but we have not artificially produced typhoid fever.

1471. That is a kind of blood poisoning, is it not?—That is one variety.

1472. May I ask whom you have employed chiefly for the purpose of these scientific inquiries; as I understand you, you have conducted no investigations yourself?—With the exception of some experiments on the inoculation of tubercle, which I did in 1866 or 1867, I have not for a long while done any experiment with my own hand.

1473. Whom have you chiefly employed in these experiments?—Dr. Sanderson has been at the head of that branch of the investigations which sometimes includes experiments on animals. But it should be understood that experiments only form part of this branch of the work.

1474. Anyone else?—Under him Dr. Klein, and Dr. Creighton, and Dr. Baxter. Now the mention of these investigators reminds me to refer to certain disinfection experiments which one of them (Dr. Baxter) has been carrying on in relation to particular morbid poisons. We were without exact material for advising the public on the relative value of different disinfectants, as means of neutralising the contagia of diseases. He has been doing exact experiments on the contagia of several diseases—on that of vaccinia, that of glanders, that of pyæmia, and that of common putrid infection. He has been making experiments on those contagia with various disinfectants, and getting very precise knowledge which will be the basis of advice which our department will be giving, probably for years to come, on this subject; and I need not tell you how important it is that we should have quite exact knowledge, such as he has been getting.

1475. With regard to the effect on the gentlemen whom you have employed in this large practice of experiments, do you think it has produced no hardening effect at all with regard to their sympathies with the lower animals?—I think not.

1476. You do not think that the habit of regarding

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animals as a mere battery of vital forces on which particular results are to be studied, necessarily to a certain extent produces the effect of diminishing the sympathy with their sufferings?—I think not. I do not anywhere know a kinder person than Dr. Sanderson.

1477. Or than Dr. Klein, for instance?—I have no reason to think otherwise of him.

1478. You were saying, I think, that, as compared with the time of Majendie, this painful class of experiments has diminished in number?—I think so, but have no materials for exact comparison.

1479. You were then comparing, were you not, the state of experiments in France at that period with the state of experiments in England at this period?—I hardly meant to compare the two countries.

1480. Would you say that, as compared with the same period as regards England, this class of experiment has not very much increased. Comparing, for instance, the period of Sir Charles Bell's experiments with this period, would you not say that this class of experiments has very much increased since then?—I should doubt whether any man now does severely painful operations on as large a scale as Herbert Mayo did in those days.

1481. You do not think that this handbook of Dr. Sanderson's gives us a much larger number of experiments of that class than were before suggested as necessary for physiological students?—That book is an encyclopædia of all approved methods of procedure for persons who may wish to do particular observations and experiments on the animal body, living and dead. Instructions as to all are brought together there; but it must not be supposed that each physiologist who consults the book does each of the observations and experiments. Immeasurably the largest use of the book in this country is, I believe, with regard to methods of anatomical preparation from the dead body; and the parts of it which describe severely painful operations which may be practised for particular purposes on living animals would, I believe, be used by but an extremely small number of persons, each of whom again would probably have but an extremely small sphere of experimentation.

1482. A considerable number of these experiments in the handbook are what you call pathological experiments induced surgically, for instance, such as biliary fistula, and diseases of that kind artificially produced. Now are not those a most uncertain and most unnecessary class of experiments, which lead to results which you cannot rely on as being at all like the results which would have followed the natural disease?—I have not myself much experience in those operations; but I feel sure that, so far as they are open to fallacy) which sometimes to some extent they are) those who do them are taking all the pains they can to guard against sources of fallacy, and to improve their methods of procedure.

1483. I have noticed the expression several times in your evidence "within limits." I did not quite understand that you really put any limits on this class of experiments, except, of course, that where pain can be avoided, it should be avoided; but beyond that do you put any limit at all on these experiments on living animals?—It is a question of proportion. I should, myself, not hesitate to sanction the infliction of a great deal of pain on an animal for a very great result; but I should not justify the infliction of the same amount of pain for a trumpery result.

1484. But as you never can tell what your result may be, would you justify the infliction of a very great amount of pain for a scientific purpose without knowing what your result will be?—If you knew what your result would be, your proceeding would hardly be an experiment.

1485. I mean to say without a very clear anticipation of what your result would be. Would you regard this practically, as you regard chemistry, as a science of experiment in which you must not take account of the pain, except where you can prevent it?—I think a great deal must be left to the individual conscience.

1486. And the conscience of the most zealous man of science would, of course, be more callous on that subject than the conscience of a humane person who was less interested in science?—The scientific conscience is subject to correction by comparison with other consciences. There is a great deal of our common life that has to be governed in that way; a great deal, in respect of which the conduct of persons cannot be governed by statute.

1487. And therefore you would not object, perhaps, to bring in the conscience of the public as a modifying influence in relation to these men of science?—Of the thoroughly educated public, not in the least; of the mob, very much.

1488. Then would you object to limiting these kinds of experiments to special places and special persons, and to subjecting them to inspection?—It would be a sham security, I think, as compared with that which already exists.

1489. But if not a sham security, you would not object to it?—On the contrary, if it were so constructed as to be a real security, it would probably interfere with work to a degree that would oblige workers to take refuge in other countries.

1490. But that would depend surely on the certainty that a thoroughly scientific man would have of obtaining a licence?—Legislation such as you suggest might be proper, no doubt, if it were shown that there already existed any great wrong; but I dispute the existence of such wrong.

1491. Do not you admit that on the Continent, certainly in Italy, and probably elsewhere, these experiments have been pursued in a very reckless spirit?—I do not know enough of what has gone on in Italy to speak positively about it; but I can speak with comparative positiveness of what goes on in this country, and my belief is that, according to our received ethics, there is not any such wrong in this country. I say "according to our received ethics," because you must take this question of physiology with the general social context; and I do this as impartially as I can when I think of projects of legislation in the present matter. I take up, for instance, one of these Bills, and I find this: "Whereas it is "expedient to prevent cruelty and abuse in the "*experiments made on living animals for the purpose* "*of promoting discoveries in the sciences of medicine,* "*surgery, anatomy, and physiology.*" Now suppose this preamble to be before either of the Houses of Parliament,—I speak, of course, with great respect to those Houses,—do you suppose that one of Magendie's experiments, the worst of them, is more cruel to the sufferer of it than hare-hunting to the individual hare, or fox-hunting to the individual fox? And is animal life less to be protected against "cruelty" when the life is sacrificed for purposes of sport than when it is sacrificed for purposes of science? You are proposing that physiologists shall be treated as a dangerous class, that they shall be licensed and regulated like publicans and prostitutes: what I would venture to put before you is, that this would be fancy-legislation, touching the relations of man to the lower animals at a little bit, and a comparatively unimportant little bit, of the subject-matter; and that society would come to such legislation with unclean hands. Who is the accuser of the physiologists? Society assumes an universal right to slaughter animals for its food, to cut their throats or wring their necks at its discretion, and neither stints its luxury (much less its hunger) in reluctance to take life, nor troubles itself much about painless methods of killing. To kill particular animals in particular ways is a considerable branch of national amusement, and the wealthy breed certain animals on a large scale exclusively to have sport in killing them, and for the unpractised often to mangle where they do not kill. Also with a view to slaughter, or with a view to other service, society inflicts sexual mutilation on nearly all the males of the cattle, horses, sheep, and swine, which it controls, Mr. J. Simon, F.R.S.

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and, as to swine, on many of the females. You never eat a mutton chop that does not come from an emasculated sheep: you rarely use a male horse that has not had its testicles cut out. Now when the common habit of society treats domestic animals in this way, slaughtering and mutilating them at its will, for use, for convenience, for luxury, and while battues and wager-slaughtering and hare-hunting are elements of national sport, it seems altogether monstrous to put into a separate category the extremely small use which physiology (for great human interests) makes of the lives of brute animals, and to have as the preamble of a Bill that cruelty is to be prevented in that one relation while all the other relations are to be left loose.

1492. Are you aware that in this handbook a very large variety of experiments on frogs, and of very painful experiments, are contained, in very many of which nothing which has any of the effects of an anæsthetic is used except curare, which is not an anæsthetic; and do not you think that the effect of those kinds of experiments on medical schools is to diminish the sympathy of men with the lower animals? You see this Commission is not occupied with sporting, or we might have something to say about that; it seems to me that we have no business to go into that subject?—Questions put to me have regarded the propriety of submitting physiology to a system of licences and inspections. It was in reference to that that I spoke.

1493. If we had to consider the subject of sporting I might have something to say upon that; but surely in relation to the healing art it is of the greatest possible importance that habits of cruelty should not be in any way encouraged. Now we have a new school of teaching which puts before the public a large variety of experiments, some of them very painful experiments, as a discipline through which the physiological students ought to be passed. It seems to me a fair question to ask you whether you do not think that a dangerous school to put them to?—Instead of answering that *à priori*, I would answer, as matter of observation, that doctors who have passed through these studies are certainly not unfeeling people.

1494. I should think that was a very questionable thing with reference to the Continent. I have heard a great deal of evidence to the effect that, especially in Germany and Italy, a great deal of the hospital practice is cruel in consequence of the schools through which the medical men have passed. At all events, surely you will not deny that if a great variety of painful experiments are recommended to young medical men, that is a dangerous school to which to put them?—I am not aware, as matter of fact, that such experimental physiology is recommended as a study for all medical students.

1495. No; but for the higher class, I suppose?—Certainly not such experiments as I believe you to refer to: but probably sometimes an advanced medical student individually uses a living frog for an observation which causes it pain.

1496. Are you aware that a class of frogs is imported from Germany for the express purpose of these experiments, the English frog being not so able to bear the pain of them?—I have heard it stated that for some physiological uses foreign frogs are preferred to English.

1497. Does not that fact itself imply that a considerable number of very painful experiments on frogs are now made before our physiological students; and are you prepared to say that that is in itself a good school for physiological students?—Where such experiments are performed, it is by the masters, not by the pupils in physiology, and gives perhaps to a large class of the latter (as I before explained) some very desirable opportunity of personal observation which otherwise each could only get by separate experiment done for himself. I see no reason why this should be of bad influence, unless the method of teaching were abused.

1498. I do not say that a large number of frogs is used at haphazard, but Dr. Michael Foster in this handbook speaks of the frog as being the chief subject of physiological experiments; and what strikes me as most remarkable is that it is the frog which is least frequently given an anæsthetic. You say that the number of experiments performed under anæsthetics is very much larger than that of those not so performed; it strikes me that there is evidence of the other kind in this handbook?—I had throughout been speaking particularly with reference to domestic animals.

1499. Still we have no reason to believe that frogs suffer materially less than the domestic animals, have we?—I have difficulty in comparing the two, but of course have no doubt as to the frog's full susceptibility of pain.

1500. Still if it be a fact that although anæsthetics are given to a number of domestic animals they can with difficulty be applied to frogs, surely the number of the physiological experiments in this handbook, to which frogs are subjected, is a very dangerous discipline to which to submit young men? Would you not admit as much as that?—If it were recklessly done, yes: but I think that you misapprehend (as I before explained) the meaning of the handbook, and that you regard it as prescribing a common curriculum for students, where it is in reality only an encyclopædia of reference for experts.

1501. Then you would have no objection, if there were any fear of that, to a certain amount of control and restriction?—I should have a very strong objection. If there be evil, I think that would not be an available way of dealing with it. It is a matter, I think, within the province of conscience, and not one to be managed by legislation; I cannot conceive that there could be legislation for the protection of frogs. I think that none of the physiologists I know would recklessly hurt even frogs. I admit that, taking all physiologists together, there is a good deal of hurting of frogs; but the gain to physiological science is very great; and, according to our common estimate of the relation of man to brute, the importance of the result to mankind justifies the sacrifice of the brute.

1502. And that as a rule they find it exceedingly difficult to give a real anæsthetic to frogs?—Yes; but again I would say *respice finem*. It is done for a purpose, and it must be considered with reference to the purpose; and it must be considered, I again venture to say, with the general social context. The special case cannot be fairly considered without reference to the whole treatment of the lower animals by man. In regard of "severely painful operations," let me again particularly refer to the many hundreds of thousands (probably some millions) of castrations which are annually done on domestic animals, as part of the ordinary routine of farm management;—vivisections, in comparison with which all which physiology performs are as nothing. And for that vast system of popular vivisections, what justification can be pleaded except that man thinks it for his advantage?

1503. (*Chairman.*) I think you just now, in answer to some questions that I put to you, intimated that it might be proper under certain circumstances to strengthen the general law with regard to cruelty to animals, and to extend that law to other animals which are not now included within it. That was a different kind of legislation from that about which you have last been asked. You have understood I suppose that the last questions pointed to regulation of a special character, like giving licences, and inspections, and so forth; but you did not intend to withdraw the answer that you have previously given on the general subject?—No.

1504. Now, with regard to this particular preamble which you just now quoted with objection, that is found, is it not, in a Bill introduced by Dr. Lyon Playfair in the House of Commons?—Yes.

1505. You probably have the same information that we have about that Bill, namely, that it emanated in a considerable degree from some of the most eminent names in physiological science in this country?—I am aware that several most eminent persons in science

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had some concern in it; I do not know exactly what concern; but I presume not to the extent of being responsible for it in all parts.

1506. You are aware that the preamble is not the operative part of a Bill?—I am.

1507. And if, therefore, any particular objections were taken to the preamble, that, probably, might easily be accommodated?—Yes; but when I argue against the preamble, it is not as against something which shall be in print, but as against something which is the mental premiss from which these proposals start.

1508. Therefore, it is not an argument against a Bill, really prepared by eminent physiologists, that those particular words might bear a disagreeable interpretation?—No; but I think, perhaps, the eminent physiologists may not quite have considered what the actual operation of this, if it became law, would be.

1509. I think you expressed the opinion that it would not be an efficient security?—I think it would not be a security for animals, and I think it would give facilities for the persecution of physiologists. I think that physiologists under law of this sort would in these days run some risk of being treated as Vesalius was three centuries ago. The Commission will at once recognize how easy it would be, if this Bill were law, for some one to move in Parliament for a return, under section 5 of clause 3, with regard to all experiments which had been made; that is to say, to convert the law into means of popular attack on individual physiologists.

1510. (*Mr. Hutton.*) It would be possible to refuse that return?—It would perhaps be possible, but probably difficult, to refuse it. I should be very unwilling, I confess, to see physiologists put in a position in which those who are now making clamour on these subjects should be able to hold them up individually to popular odium; and under this clause that could be done.

1511. (*Mr. Forster.*) With regard to the investigations conducted by you and your assistants under the Parliamentary vote, I gather from your answers that there are positive instructions that there should be no operation on any living animal without anæsthetics?—On receiving your minute I immediately communicated it to Dr. Sanderson, with a request that he would "please read, and note as an instruction, the above minute by Mr. Forster;" and Dr. Sanderson returned me the paper as "read and noted." I should like to bring if I may under notice of the Commission that 30 years ago discussion was going on such as has been going on lately about vivisection, and that at that time there was written a pamphlet, I believe under pseudonym, entitled "Remarks on the use of "Vivisection as a means of Scientific Research, in a "letter addressed to the Earl of Carnarvon, President "of the Society for Preventing Cruelty to Animals, "by Richard Jameson;" and that this pamphlet goes through much of the matter which the Commission is now considering. It is so thoughtfully, and I think justly written, that if it is not out of print, I would suggest that the Commission might like to see it.

1512. (*Chairman.*) You say it is under a pseudonym?—I believe it to be so.

1513. You are not at liberty to say who the author is?—I am not at liberty to mention my suspicion of the authorship.

1514. (*Sir John Karslake.*) Do you believe him to be a man of eminence?—Yes.

The witness withdrew.

Adjourned.

Tuesday, 19th October 1875.

PRESENT:

THE RIGHT HON. VISCOUNT CARDWELL, IN THE CHAIR.

The Right Hon. W. E. FORSTER, M.P.
Sir J. B. KARSLAKE, M.P.
THOMAS HENRY HUXLEY, Esq.
JOHN ERIC ERICHSEN, Esq.
RICHARD HOLT HUTTON, Esq.
N. BAKER, Esq., Secretary.

Mr. JOHN COLAM called in and examined.

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1515. (*Chairman.*) You are the Secretary of the Royal Society for the Prevention of Cruelty to Animals?—I am.

1516. How long have you been so?—Fifteen years.

1517. During that time I presume it has been your province to carry into execution the wishes of the society, as far as you could, in reference to the subject which we are appointed to consider?—Yes.

1518. Has that given you the means of knowing, with any considerable degree of accuracy, what is the actual practice in this country on the subject. Do you know, that is to say, what things are done to animals habitually in this country?—Personally, I know a little, and from reading, I think I know a little more.

1519. And can you inform us what is the actual state of things now in existence in this country?—Vivisection is practised of course, and very often with very much pain to the animals; that is undoubted.

1520. Have the society arrived at so much of a conclusion on the subject as to be enabled to put their wishes into the form of a Bill?—They have.

1521. Have you got that Bill with you?—I have.

1522. Are you prepared to give it to the Commissioners?—Yes (*handing it in*).*

1523. Will you tell us, if you please, in general terms, what the provisions of that Bill are?—The main feature is that it prohibits all painful experiments.

1524. By "painful experiments," will you tell us what exactly you mean; is an experiment in its nature painful within the prohibition of your Bill when performed under complete anæsthesia?—The experiment is without pain then.

1525. Then it would not fall within the provisions of your Bill?—Certainly not.

1526. Will you tell us in general terms what the Bill provides?—With regard to that question of inflicting pain, the ninth section provides: "That no "person shall perform, or cause to be performed, or "take part in performing, any vivisection upon any "animal, without having first of all subjected such "animal to the influence of an anæsthetic, so as to "render it wholly insensible to pain;" and then the next section provides: "That no person who shall "perform, or cause to be performed, or take part in "performing, any vivisection upon an animal so subjected as aforesaid, shall omit to destroy such animal, "before the effect of the anæsthetic ceases."

1527. Then you seek entirely to prohibit the infliction of any pain upon any animal for the purposes of scientific investigation or discovery?—Yes, that is so.

1528. Now suppose the case to be this: that an

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* Appendix III., § 5.

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experiment is performed under an anæsthetic, and the actual operation is not therefore painful to the animal, but suppose the recovery from that operation to involve some pain; that would be an operation prohibited by your Bill, or your Bill would require that the animal should be destroyed?—Undoubtedly.

1529. So that if an experiment were tried for the purpose, for instance, of improving the mode of tying arteries, and the principal pain were removed by an anæsthetic, it would be contrary to your Bill if the animal were allowed to recover afterwards, and there were some pain in the process of recovery?—Yes, I think it would be, because it provides that the animal must be rendered "wholly insensible to pain," and that the animal must be destroyed while anæsthesia remains. But I would like to add, that very frequently the pain is quite as severe after the ligature has been made as it is at the time of the operation.

1530. Now as regards the practice, so far as you are aware of it at the present time, are experiments performed in which no anæsthetic is used?—Undoubtedly.

1531. And experiments in which an anæsthetic might be employed without prejudice to the supposed object of the experiment?—Upon the opinion of others I should say so; but as a layman I am scarcely able to answer that question.

1532. But that is the conclusion at which, as the secretary of the society, you have arrived?—Quite so.

1533. Will you give us some instances which, in your opinion, best illustrate what is actually done?—I have here a mass of evidence consisting of 370 pages, which has been culled from various books reporting experiments, in which there is evidence of pain. I have also other quotations in which there is evidence of prolonged pain. With regard to the question put to me, of course it would be impossible for me to go through this large number of cases. I will just name one or two. It does seem to me that there was a great deal of pain caused by the suffocation of dogs when a number of animals were drowned and half drowned and again restored to life and then drowned again by a Committee of the Royal Medical and Chirurgical Society in consequence of an application by the Royal Humane Society to them to consider some better means for the resuscitation of persons apparently drowned. The experiments which are recorded here will show you that the animals were, several of them, two or three times put into water half drowned, and kept in water three, four, or five minutes, and then brought again to life, and thus put to horrible agony. It might be said that the object was very good, but the answer to that is that the cruelty was really fruitless, as the report admits that no conclusion could be drawn from the experiments. The report shows that experiments on dead human bodies were useful. Practically, Dr. Sylvester's method was recommended by the committee appointed by the Medical and Chirurgical Society, which method was in use before the inquiry took place, and is still in use. I wish to add that the Royal Humane Society is not at all responsible for these experiments.

1534. I understand you that you have there 370 pages of evidence, from the length of which you wish us to take them as read without putting you to the task of reading them through. Did I rightly understand you to say so?—Yes, I put them in for your perusal.

1535. Have you got the references and the names specified?—In every case.

1536. Will you be so good then as to put in the references to those instances, and the authority upon which in each case your statement rests?—I will.

1537. (*Sir John Karslake.*) I understand that these are extracts written out by yourself from different books?—Yes, or by my orders; altogether about 800 pages of foolscap.

1538. (*Chairman.*) Will you at the present moment make a selection of a few instances which you think will best inform the Commission of the points which you desire to bring before them?—I am scarcely prepared to analyse, because I thought that I should just simply have to put my evidence in for your perusal; and, therefore, the analysis consists of divisions in this form:—evidence of painful experiments, evidence of pain prolonged, evidence of design to teach students vivisection in laboratories, and evidence of vivisection performed by students themselves (which I have only found one instance of in the whole of my inquiry), and opinions in favour of restriction, and against vivisection.

1539. You said, I think, that some of the experiments you have collected were instances of protracted agony?—Yes.

1540. So far as you know had any attempts been made to use anæsthetics in those cases?—I believe not. In some cases it may be that it would defeat the object of the experiment if an anæsthetic were used.

1541. But as a matter of fact where there has been an experiment involving what you have called protracted agony, is there any reason to suppose that it has either been possible, or that at any rate it has been the fact that agony has been counteracted by chloroform or other anæsthetic?—No.

1542. In that case it would defeat the object of the experiment to use an anæsthetic?—Yes, generally, I think.

1543. In the cases in which it would have been possible so far as time or the object was concerned, do you consider that anæsthetics have been used always?—I believe that generally the English physiologists have used anæsthetics where they think they can do so with safety to the experiment.

1544. Then may the Commission take your belief to be that there is a desire on the part of the scientific men in this country so far to get rid of the infliction of pain as is compatible with the scientific object which they have in view?—I should say so, generally, but in some cases there appeared to be some heedlessness with regard to the suffering of the animal; for instance, in some of Brown Séquard's experiments, where animals were kept for weeks in suffering.

1545. May I take it to be your view that the general tendency of the English scientific world is not at variance with humanity?—I believe it is very different indeed from the practice of foreign physiologists.

1546. So that you would treat cases of wilful cruelty, if they exist at all in this country, as exceptional cases rather than as fairly chargeable upon any want of proper sentiment on the part of the profession?—Undoubtedly with regard to wanton cruelty. I do not know that I know of a single case of wanton cruelty, by which I mean suffering caused without any object except to gratify a cruel mind.

1547. Then you give the scientific men of this country credit for using anæsthesics, and dealing tenderly with animals so far as is compatible with the objects which they have in view?—Yes; I think so, speaking generally. As regards tenderness, I have no evidence to prove they are tender to animals.

1548. That the cases where that is not so are exceptional cases, and not cases fairly chargeable to the profession generally?—I think so.

1549. But you think that experiments are performed which are in their nature beyond any legitimate province of science, and that the pain which they inflict, is pain which is not justifiable to inflict, even for the scientific object which they have in view?—That is the opinion of our society.

1550. Now with regard to students, have you any evidence that you can lay before us as to what is done in respect of students?—With regard to London I have inquired at every school, and I have not found a single place where experiments are actually performed before students. The animal is operated on in the laboratory and brought out thence in a narcotised condition, and then it is examined by the students.

1551. Do I rightly understand you to draw a broad distinction between an experiment performed for the purpose of establishing some new scientific truth on the one hand, and on the other hand the repetition of such experiment, after the truth in question has been established, for the purpose of illustrating it to students?—Undoubtedly the society draws a very marked line there. A difficult question, of course, does arise as to when the truth has been fully established, but illustration to students is altogether objected to.

1552. But supposing that the object is to establish some new scientific truth, and that anæsthetics are as much employed as the nature of the case admits of, I still understand you to say that the society object unless the anæsthetics can be so far employed as to take away the pain altogether?—Yes, the society is a society for the prevention of cruelty, and would not step out of its direction to legalise anything contrary to its nature; it would be a misnomer for the society as a corporation to legalise cruelty for any purpose. What individuals in the society might be willing to do would be a different matter.

1553. Do I rightly understand you that there are individuals in the society who maintain a different view, but that the society in its corporate capacity speaks through the four corners of this Bill which you have put in?—Undoubtedly.

1554. Does that include this, that some members of the society would probably go beyond this Bill?—Undoubtedly there are some who would prohibit all experiments, and others who desire that no Bill should be framed, believing the present statute sufficient to suppress cruel experiments.

1555. And there are others who you think might not go so far as this Bill?—Yes, undoubtedly.

1556. But in its corporate capacity the society speaks through this Bill;—Yes.

1557. This Bill was framed after the society had before it the two Bills that were introduced into the two Houses of Parliament last year?—It was; but we had previously drafted two other Bills.

1558. And of the two it is framed more upon that introduced into the House of Commons by Dr. Lyon Playfair than upon the other?—I think so; but certainly it takes some of the provisions of the other Bill.

1559. Now would you have the kindness to state to the Commission what are, in your opinion, the principal differences between the proposals which were made last year to the two Houses of Parliament, and the Bill which on the part of the society you are authorised to submit to us?—The first thing that I note is, that Dr. Lyon Playfair's Bill provided that it should come into operation immediately, and ours provides on the 1st of January. That is perhaps an immaterial matter, but this Bill provides that there shall be no pain inflicted; that is the leading feature of it. Then, too, it asks for power to go to a justice of the peace and get a search warrant if there is reason to think that the law is being infringed.

1560. That provision perhaps is due to some experience that the society may have of the way in which their attempts at prosecution have heretofore been defeated?—Undoubtedly; and especially with regard to this question; for now it is next to impossible for the society to know what is going on actually. Then there is another difference which I should like to point out, which is that there is a more ample provision in this Bill for the registering of experiments, especially with reference to the results of the experiments; the object of the society being two-fold: first, that it might have an opportunity of testing whether the experiments were doing any good, and secondly, that it might by the publication of the results prevent the repetition of experiments; because if those results were published it would undoubtedly prevent much experimentation.

1561. Your main difference from the Bill of Dr. Playfair is that you omit altogether the provision which he made for the infliction of pain in a certain class of experiments?—Just so.

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1562. And you comprehend all painful experiments in the prohibition of your Bill?—Yes.

1563. Then, like Lord Henniker, you adopt the proposal for a license in regard to places?—Yes.

1564. And you extend that to persons?—Yes.

1565. So that if your view were to be adopted by the Legislature no painful experiment could be performed at all?—None.

1566. And no experiment, whether painful or under complete anæsthesia, except by licensed persons in licensed places?—Undoubtedly.

1567. Is there anything that you desire to bring before the Commission which has not been included in the questions already put to you?—I do not know that there is, excepting that the committee are very anxious first of all that you should understand that the Royal Society for the Prevention of Cruelty to Animals is not the society advertised for the total abolition of experiments, and that it is not responsible for the rash literature which has been printed on this subject; and secondly, I was instructed when I came here to read to you what the society have done on this subject, and what the committee, upon the presentation to them of a memorial against vivisection, did in reference to the examination of the allegations of the memorial, and deliberations upon its prayer.

1568. That is a document prepared under the sanction of the society, and you are speaking their language when you read it to us?—Yes. 'To the 'Honourable Royal Commission on Vivisection. 'My Lords and Gentlemen. In response to your 'invitation, the Royal Society for the Prevention of 'Cruelty to Animals have desired me to appear 'before you for the purpose of placing before you 'the evidence they possess in relation to the practice 'of vivisection in this country. Instead of a layman 'they would have preferred that their representative 'should be a medical man or a physiologist, whose 'experience and special education would prove to be 'of more service to you than my information can 'possibly be. There are, however, good reasons to 'prevent the attendance on their behalf of an expert, 'some of which may be found in my better acquaintance with their views, and with the proceedings of 'the society in reference to this practice. They 'have desired me to place myself entirely at your 'service. To this end I have already suggested the 'invitation of several witnesses who may be expected to place information and arguments before 'you. It will be convenient, perhaps, for me to 'state (1.) What course the society have hitherto 'taken against vivisection, and what means they have 'employed to get evidence. (2.) The nature of the 'information they have obtained and propose to place 'in your hands. The practices which they opposed 'many years ago at Alfort and Lyons Veterinary 'Schools, with much success, scarcely come within 'the meaning of the term 'vivisection,' as they 'were designed to teach dexterity of manipulation, 'and were, therefore, more of the nature of operative surgery. As similar practices do not prevail 'in this country, and, on the contrary, are reprobated by physiologists on this side of the channel, 'it will not be necessary to allude further to them. 'Thirteen years ago I was instructed to make an 'inquiry into the practice of vivisection in Great 'Britain, and addressed letters to the schools of 'medicine, asking for information. The committee 'of the society were much gratified to find that at 'such time there was every willingness on the part of 'professors to supply the data asked for, and particularly that there appeared to them to be comparatively few operations being made at such institutions. Nevertheless, it was considered desirable to 'use every possible moral means to check the spread 'of vivisection. The committee, with this view, 'offered prizes for the best essays against that practice, placing two questions before the essayists, as 'follows:—I. Is vivisection necessary or justifiable '(when performed as at certain veterinary schools) 'for the purpose of giving dexterity to the operator?

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'II. Is it necessary or justifiable for the general purposes of science, and if so under what limitation? Assisted by eminent judges the awards were made, and the essays by Mr. Fleming and Dr. Markham were printed, a copy of which I now lay before you. Other and many means were used to the same end. Last year the committee felt bound to institute a prosecution against Dr. Magnan and others at Norwich for alleged cruelty to dogs during an experiment at the meeting of the British Medical Association. The evidence in that cause with a report of its result they put before you on this occasion. Last year also an international congress of societies assembled in London to discuss common objects tending to promote kindness to animals, at which resolutions were unanimously passed by the delegates condemning the growing practice of experimental physiology so far as it tends to animal suffering, and pledging itself to check vivisections in the various nations of Europe and America. Owing to the alleged increase of experiments an influential deputation appeared before the committee last January, and presented a memorial to the society, praying for its active interference to repress vivisections. A copy of that memorial is now handed to you with the names of the principal persons who signed it. A committee of the society was at once formed, charged with the duty of inquiring into the allegations of the memorial and deliberating on its prayer. That committee held frequent and long sittings, and delivered its report to the general committee in due course, the following being the substance thereof. "We have held several meetings and have taken much pains to obtain evidence to verify the allegations made in the memorial as regards the extent to which the practice of vivisection prevails in the United Kingdom, where, by whom, and for what objects, experiments are performed; and whether or not animals experimented on are previously made insensible to pain, and kept during experiments, and until destroyed in a condition of anæsthesia. At every stage of our inquiry we have experienced great difficulty in procuring such evidence. Avenues leading to private laboratories are closed to this society, and private operators whose conduct is under investigation and public stigma have not been found willing to volunteer information. Moreover, the gravity and complexity of the subject call for more than ordinary care and exactness in the collection of reliable data. In the first instance we addressed a circular to the several hospitals, infirmaries, and medical schools of the metropolis, to which places the memorial appeared to refer [a copy of which I hand in to you]. The managers were respectively invited to supply particulars relating to operations on living animals in those institutions, and to permit the secretary to be present on occasions when experiments were being conducted. The following return will show the results obtained by such measure. No replies have been received from St. Thomas's Hospital, West London Hospital, Infirmary for Consumption and Diseases of the Chest, North London Consumptive Hospital, City Orthopœdic Hospital, Harrison Spinal Institution, National Orthopœdic Hospital, Royal Orthopœdic Hospital, Central London Throat and Ear Hospital, Ear Infirmary, Central London Ophthalmic Hospital, Ophthalmic Hospital, St. Peters' Hospital for Stone and Urinary Diseases, Epidemiological Society. Acknowledgments only have been received from the Medical Society of London and King's College Hospital. Evasive answers were received from University College Hospital, St. Mark's Hospital, City Road, and Great Northern Hospital. No experiments are performed at East London Hospital, Royal College of Surgeons, Hospital for Diseases of the Nervous System, Western Ophthalmic Hospital, British Hospital for Diseases of the Skin, London Fever Hospital, Royal College of Physicians, Hospital for Hip Disease in Childhood, Royal Hospital for Diseases of the Chest, National Hospital for the Paralysed and Epileptic, King's College Hospital, London Hospital, Hospital for Consumption and Diseases of the Chest, Royal Dispensary for Diseases of the Ear, City of London Hospital for Diseases of the Chest, Metropolitan Free Hospital, Obstetrical Society of London, German Hospital, Poplar Hospital for Accidents, Female Lock Hospital and Asylum, Male ditto, Royal South London Ophthalmic Hospital, St. Mary's Hospital, St. Saviour's Hospital, Royal Free Hospital, Cancer Hospital, Royal London Ophthalmic Hospital, St. Mark's Hospital, London Infirmary for Diseases of the Legs, and Great Northern Hospital. Vivisections are performed at the Westminster Hospital, Broad Sanctuary, Guy's Hospital, and the Pathological Society. Admission has been refused by the Middlesex Hospital (from medical committee, not board of management), Charing Cross Hospital, Pathological Society of London, Chemical Society of London, and the Royal Medical and Chirurgical Society. The following have consented to admit: Westminster Hospital (qualified persons only), St. Bartholomew's Hospital, Guy's Hospital, the Brown Institution, and St. George's Hospital. Mr. Fleming and the Secretary accepted the invitation to attend at Guy's Hospital, and have reported thereon as follows: 'In compliance with an invitation we visited the above Hospital to-day, when we had an interview with Dr. Steele for the purpose of ascertaining the nature of the experiments performed on animals at the hospital. During our conversation Dr. Steele said: 'I can give you positive assurance that students do not perform any experiments on animals here, for Doctors Pavy and Pye Smith are the only operators on animals. I do not believe there is any cruelty to animals performed in any experiment as anæsthetics are invariably used.' He handed to us the annexed Hospital Report, and upon our calling his attention to a passage marked at page 48 [that passage seemed to imply that there was vivisection performed before students; but it was explained to be rather ungrammatical language], he said it was an unfortunate sentence, and that the secretary of the medical department should have qualified it by adding 'all animals being narcotised.' He said, he was glad to see us for the purpose of correcting wrong impressions, and would admit us to the hospital at any time. He then conducted us to Dr. Pavy's private laboratory, where we saw three living dogs (a long haired terrier, a pug, and a mongrel,) which, we were informed, were shortly to be the subjects of experimentation. We also saw an operator's table, and several appliances for experiments. Dr. Pavy said, during our conversation with him, 'I am glad you have come to my sanctum to judge for yourselves whether we are cruel or not. There is much misrepresentation abroad on this subject, which no doubt arises out of an unfortunate expression coined by the College of Surgeons some time ago, when they desired that students should be taught what they called 'practical physiology.' Students are not, and never have been, permitted to perform experiments at this school, neither do they ever see experiments performed on animals which have not previously been rendered insensible to pain. I will tell you all I do before the students, and privately in this laboratory. Every animal dissected before students is either narcotised or pithed' (an operation on the *medulla oblongata* which destroys life), 'except in the case of frogs, which I decapitate in the presence of the students, and before I commence to operate on them. The chloroform is administered, and the pithing is performed in this laboratory before the animal is taken down into the lecture room for demonstration, and the animal is killed before consciousness returns. I would not dare to perform an experiment before the students upon an animal not previously narcotised, any more than I would

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“‘dare to commit an act of cruelty before your eyes in Jermyn Street. In all my lectures I have always felt that I have not done a single thing, nor said a word I should be afraid of your seeing and hearing, and, indeed, I have felt it my duty to act as though you were present in the room. Could I have been guilty of giving pain to a dog or cat during my lectures, I am sure the students would have been the first to protest against it. I do not use more than 18 animals during my lectures in a term.’ He then showed to us his appliances for administering chloroform and his instrument for pithing dogs, also a book which contains directions to his assistant for the preparation of lectures, in which it is provided that the animals should be pithed or rendered insensible to pain by means of chloroform or puff balls. From this book the lectures given by Dr. Pavy appear to be mostly of a physical nature, and the animals alluded to are shown only to advanced classes. In answer to questions Dr. Pavy said that his private experimentation referred chiefly to the secretions. He said, ‘I invariably use chloroform in all my private experiments, and am convinced after much thought on the subject that, taking the aggregate of experiments required by science to be performed on animals, not one in 500 need cause any pain whatever to the animals, and in those which I perform an anæsthetic may be administered in every case. I have told you that I do not use more than 18 animals per term for my lectures, but I cannot say how many I use in this private laboratory. I have often said here what a boon chloroform is, as it enables us to perform almost any experiment we like upon an animal that does not quiver, or start, or struggle, but allows us to cut away as we require, and does not prevent reliable deductions except in a very few cases. Although I always narcotise an animal, I do not always kill it before sensibility returns, but I nearly always do so. Sometimes it is necessary to keep it alive after the anæsthetic has ceased to act, especially in any researches relating to digestion. On one occasion I produced fistula in an animal’s stomach to study the process of digestion, and kept it alive for such purpose several days, but the dog was as happy as possible, and even sat on my operating table while other dogs were being operated on without any sign of uneasiness. When animals recover from chloroform and are still kept alive in the manner described they suffer no pain. During the cutting process they are narcotised, and after such operation when sensation returns I am convinced they do not suffer pain. Even human patients in our hospitals whose limbs have been amputated while they were under the influence of an anæsthetic feel no pain at the wound upon their return to consciousness. In all possible cases, however, I destroy the animal during its insensibility. I do not believe that students perform experiments on animals as they have no time for laboratory work. Experiments by students would be discountenanced by every teacher at this school. I shall be glad to see you at any time or any person you may send. If your society acts discreetly at the present time in reference to this subject, it may do much good by checking excess in experiments on living animals.’ Dr. Pye Smith also expressed himself as glad that the society was engaged in investigating into the allegations made against experimenters. His lectures were confined mainly to histology.’ He said, ‘I have a summer course, however, when I experiment on animals, but never without chloroform having been previously administered; indeed I perform no experiments without an anæsthetic. I have done many experiments, but I am not aware that I have ever given pain to an animal. I am in favour of students seeing experiments on animals which are at the time insensible to pain. I have not seen anything performed at the Brown Institution of which I disapprove. An anæsthetic agent may in nearly every case be administered to animals before the experiment commences. I am not now attending to the trial of new medicines on animals, which is necessary before giving such drugs to human beings. Alluding to a mistake made by the memorialists respecting experiments on the eyes of animals by students, Dr. Pye Smith admitted that it was a natural error arising out of the construction of the sentence quoted from the report of St. Thomas’s Hospital, which was calculated to mislead. (Signed) George Fleming, John Colam.’ The secretary attended at Saint Bartholomew’s Hospital by invitation of Dr. Lauder Brunton, when he witnessed several experiments on cats and frogs performed by that gentleman in his private laboratory, in all cases the animals having previously been rendered wholly or almost entirely insensible to pain.” [I ought to say that I found Dr. Brunton extremely willing to give me every information, but I cannot believe that in one or two cases the cats which I saw there were entirely without pain, not simply because of their movements, but because of the necessity there appeared to be again and again for him to insert blotting paper into chloroform and put it to the nostrils of the animal in order to lull it when it commenced to cry a little.] “Dr. Brunton stated that such vivisections fairly represented a day’s work, that each would have taken place precisely as then seen had the secretary not been present, and that even if for no higher purpose animals are invariably narcotised in this, and he believed in other laboratories, for the advantage of easier manipulation before the experiment is commenced, and are generally destroyed before sensibility returns.”

1569. (*Mr. Forster.*) Were all the animals destroyed which you saw?—They were. In fact the dose of chloral was so heavy that I have not the slightest doubt they could not have recovered from it. “The object of the experiments which Dr. Brunton was then employing was to ascertain the action on the heart of certain poisons used by the natives of Jamaica and Southern Africa for the destruction of Europeans, and to discover a remedy for the same. Two lectures delivered by Professor Ferrier at the London Institution on the results of his experiments on animals have been attended, from which it does not appear conclusive that an agent of insensibility is always employed by that physiologist during his experiments, although this may be done during the operation; but, on the contrary, that sometimes the animals have suffered pain. The medical journals and works published by physiologists have been examined minutely, and many pages of manuscript extracted therefrom. These conclusively prove: (1.) That vivisectional experiments on animals are frequently used by medical men and physiologists in this country. (2.) That in many instances no anæsthetic agent is employed throughout the experiments when much pain is necessarily caused. (3.) That the alleged object of such operations is scientific research and demonstration. (4.) That the same experiment is repeated many times on different animals. (5.) That an effort is being made to instruct students, by means of handbooks, in the practice of vivisections. Evidence has been taken from persons who have witnessed vivisections at medical schools, whose testimony shows that professors seldom operate on living animals before their classes, but frequently exhibit them alive after an operation has been performed, and always while the animal is insensible to pain; that the object of such experiments which are repeated *ad infinitum*, is solely to demonstrate facts previously established, and that students do sometimes perform experiments on living animals at their own chambers without the knowledge of their teachers.” [I have a paper with regard to this which I shall leave with you.] “Dr. Hoggan, whose letter on vivisections has caused much anxiety, has appeared before us on three occasions. He cannot give any particulars of practices which prevail in this country, his experience having been confined to laboratories on the Continent. He,

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“nevertheless, states that Continental views on this subject and experiments are being rapidly introduced into England and Scotland, an instance being the employment of living animals to illustrate lectures in medical schools, which mode of teaching has long been the custom in Europe, but has only recently become general in the United Kingdom. The secretary of the deputation, Miss Cobbe, has been invited to supply evidence on which the allegations of the memorial were based, and more recently the medical signers of the memorial, about 70 in number, were addressed by letter, asking them to be good enough to supply data respecting those vivisections of which they had complained, or to point to sources from which we could obtain the same; but we regret to say that no new evidence has been obtained from the memorialists of a specific nature. We have also endeavoured to obtain information respecting the supply of animals to physiologists for the purpose of being used in vivisections. Much secrecy necessarily prevails on this matter, but we have discovered persons who are engaged in providing animals for the operations alluded to. These men are dealers in cats and dogs, and although naturally very reticent they have admitted taking animals to St. Bartholomew's and Guy's Hospital for experiments, and under regulations of a private and confidential character. Vivisection is a practice in which many conflicting elements are present, and where science, acting for the prevention of human diseases and human suffering, is brought into contact with suffering entailed on a vast multitude of animals, and it is therefore extremely difficult to arrive at a just determination as to measures which are necessary for the protection of animals. It becomes our duty, however, to endeavour to procure the adoption of some of stringent legal restrictions of the practice of vivisection. Avoiding any extreme measure, such as the total prohibition of experiments, a medium course should be taken with a view to prevent abuses and to confine experiments within as narrow limits as possible. Repetition of operations when deductions have already been established should be forbidden, and unskilful and unauthorised persons should not be permitted to perform experiments. It is still more essential that anæsthesia should be insisted on whenever experiments are performed, and continued during the experiment.” ‘The committee cannot doubt that vivisection has been considerably extended during the last few years; and though they cannot prove that students to any extent engage in it personally, they regret that lectures are illustrated to them by animals previously operated on, and that in such matter, as in others, Continental usages are being imported into this country. Such is the growing carelessness, that in one instance, perhaps unintentionally, a living lobster, they have reason to believe, has been cut up in the presence of a class of young ladies by their physiological teacher; while popular lectures are being given of a sensational character by a learned professor, who made his audience laugh over the grim behaviour of his unfortunate victims. The evidence which is now placed before you gives proofs only too abundant of the prevalence of painful experiments on animals, painless ones being left out of the extracts now submitted; that such pain is often protracted for days and weeks; and that a determined effort is being made to train students by the practice of experiments. Foreign experiments have been purposely omitted, except when these have been approved and recommended by English papers. (Signed) John Colam, Secretary.’*

1570. (*Chairman.*) That is the report of a committee appointed by the society to consider the allegations of the memorial?—Yes, with additions ordered by general committee for presentation to you.

1571. And it has been your instruction to make it known to us?—Yes.

1572. It may be taken then as the general opinion of the society that that is a document of great importance for us to consider?—Yes.

1573. You have said that a professor gave public lectures in which he sought to amuse his audience by a description of the grim behaviour of the victims of his experiments?—Yes.

1574. Who was that professor?—Professor Ferrier.

1575. Upon what evidence is the allegation founded?—Upon the evidence of newspapers, which reported his lectures, and upon my own personal presence at his lectures.

1576. We may take it that you personally have heard Professor Ferrier describe the infliction of severe suffering upon the animals upon which he operated?—I have heard him say that the animals “appeared” to be in intense suffering, and then joke about the stupidity of the animal, especially if the animal happened to be a monkey, giving humorous descriptions of its behaviour; so much so that at times there was general laughter in the lecture place. Three gentlemen were with me who are members of the committee of the society, and one of them left the room in disgust.

1577. (*Mr. Erichsen.*) Was that a popular lecture or a medical one?—A popular lecture. I think the place where it was given is called the London Institution, in Finsbury Circus.

1578. (*Chairman.*) Is that lecture published?—I really do not know.

1579. Were newspaper reporters admitted to it?—Yes, I presume so, as the public could enter, and the members of the institution were admitted. I think I have got a report of similar remarks which were made at a lecture in the north. I should say the whole of the lecture, so different from anything I had heard by experimental lecturers before, was interspersed with parentheses of laughter.

1580. (*Mr. Erichsen.*) The audience was not shocked then?—Not in the least.

1581. (*Mr. Huxley.*) Professor Ferrier's experiments were experiments on the different parts of the cerebral hemispheres, were they not?—They were.

1582. He was not operating before his audience, was he?—Not at all.

1583. But he was describing to his audience the effect of the irritation of certain parts of the cerebral hemispheres?—Yes.

1584. I take it, among those effects he described contortions of the face?—Yes.

1585. And contortions of the muscles of the limbs?—Yes.

1586. I presume that it may have been the case that those contortions were in themselves very grotesque?—Yes, one might have laughed if one did not know that the poor creature's head had been opened.

1587. As a matter of fact, when a monkey makes contortions of the face or moves its limbs about, it is actually very grotesque, is it not?—Yes.

1588. Then the laughter which it induced may have been from the inherent grotesqueness of the thing, and not from any desire of Dr. Ferrier to make it grotesque?—Yes; but the skill of the lecturer was used to cover the grim character of the experiment by his humour.

1589. The question here is a matter of interpretation as to what Dr. Ferrier meant. I presume it is perfectly impossible for a man to describe, even with the most serious intention, the curiously grotesque grimaces that a monkey would make under those circumstances without producing a laugh?—And without laughing himself, it would appear.

1590. (*Chairman.*) At any rate this statement is made by you not on your own authority, but it is laid before us from official documents, and by the desire of this society?—Yes.

1591. It was on some particular day, of course, that this lecture occurred?—Yes.

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* Appendix IV.—Documentary evidence handed in by the witness.

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1592. Can you tell us what day it was ?—I should think it is six months ago.

1593. But will you furnish the Commission with the exact date ?—Yes, if I have notes of it.

1594. And you can assist us in discovering whether any London newspaper contained any report of what actually took place ?—Yes. I could give you also a copy, I think, of a report of a similar lecture which he delivered in the north of England, when the same laughter happened. I would like to add that I should be very sorry myself to say that there was any design to make mockery of any suffering of the animals, because, so far as I could see, there was some evidence to lead one to believe that the animal was incapable of suffering at the time when the contortions he described took place; but what did strike me, and struck a good many other people, was that as there were several young people there, and several young ladies too, it was a long way out of good taste to be making use of such remarks.

1595. (*Mr. Forster.*) But am I to understand that these contortions that you are referring to were contortions of the animal under the influence of anæsthetics ?—Yes, I think it was after the removal of the brain when it was alleged that the animal would be incapable of suffering in some cases.

1596. (*Chairman.*) But what it is important for us to understand is this, whether this is brought before us as evidence of great and unfeeling cruelty on the part of the lecturer, or merely, as what you seem now to have put it, a departure from good taste ?—As a departure from good taste, and as sensational.

1597. Do I understand you on the part of the society to withdraw that altogether in the sense of being an allegation of great and deliberate cruelty ?—That was not meant for a moment. I attended those two lectures endeavouring during the whole time to discover evidence of suffering, and, with the exception of one small remark, which might be understood in two ways, it was impossible to discover whether there was any suffering on the part of the animals at all in the descriptions given.

1598. I consider this an important document put before us by the society, and I should like very much that we should thoroughly understand what we are to consider them as putting before us. Are we to consider that it is to be examined by us as an allegation made against this particular professor of doing that which I understand you to say professors generally do not do, namely, exhibiting great habitual indifference to the sufferings of animals ?—No; but merely as a case of levity, likely to produce a bad effect.

1599. (*Mr. Forster.*) What do you mean; levity at the contortions of the animal, or levity at the contortions of the animal caused by pain ?—Simply levity on the subject generally; the lecture was made what is called popular.

1600. (*Chairman.*) But the important thing you will observe is, that we should know whether it is an allegation against a particular professor of great indifference to the suffering of the animals ?—I think not.

1601. Then for what particular purpose is it made so important a feature in a document of the Society for the Prevention of Cruelty to Animals ?—Because it struck the members of the committee, as it did myself, that there was scarcely that decorum which you would expect, and that one ought really to see, in a man who was describing the condition of animals which had been mutilated by himself. There was not a word of commiseration or regret that the experiments were necessary, but there was amusement offered for the audience.

1602. But I understand you now to express belief that the animals had not suffered, but that they had been under chloroform the whole time ?—He did not say; we could not gather at all whether they had been under chloroform; but I am inclined to think, from the nature of the experiments, it was absolutely necessary that they should be.

1603. Do you mean us, as far as you can guide us, to understand that these animals had been subjected to great suffering, or that, whatever had been done to them had been done under complete anæsthesia ?—I would rather be inclined to think that the animals did not suffer, judging from the description given of the experiments, and from what I have read myself in the various books which I have been obliged to read. The allusion to the experiments of Dr. Ferrier in the paper which I have read, where there is an intimation that sometimes there is suffering, is to cases where he states that the animals have remained under the operation for several days.

1604. (*Mr. Forster.*) I understood you to say that you thought the reason why there was no suffering was because the brain had been removed ?—Yes, after the operation had been completed the effect of the narcotic would cease in most cases before the experiment had been concluded.

1605. Do you think that it was that there was no suffering because chloroform, or some anæsthetic, had been administered, or because the brain had been removed ?—Of course we did not see the experiment, we only heard a description of it, and I should think that chloroform must have been used during the operation. I am inclined to think it would kill the animal to perform the operation without chloroform, the effect of which would last until perhaps after the brain has been removed partly or wholly; therefore in the first part of the experiment there would be no feeling owing to the presence of a narcotic, and in the second part of the experiment owing to the removal of the brain.

1606. (*Mr. Hutton.*) May I ask you to explain your statement that the lecturer joked about the stupidity of the monkey. Those movements were involuntary, were they not ?—Of course they would be, because he showed that the animal had no volition at all.

1607. And therefore it was meant as a sort of joke upon the apparent expressions of the monkey ?—Yes, and the loss of intelligence of the animal also.

1608. (*Chairman.*) Have you any record at the society, any newspapers filed, that contain a record of this lecture ?—I do not think that a report of the lecture appeared in any London newspaper. Probably newspaper reporters do not attend at the London Institution on such occasions; and therefore there would be no report of it. Two barristers were with me, one Mr. Robert Sawyer, and the other Mr. Thomas Allen, and another member of the committee, Mr. Thomson; and one of those three gentlemen left the room in consequence of the pain with which he saw the laughter of the young people.

1609. (*Mr. Forster.*) I thought you stated that there were reporters there ?—I did not see any reporters there.

1610. But you stated something about reports of the lectures appearing in the newspapers ?—That was in the north of England.

1611. (*Mr. Erichsen.*) Did not this lecture consist of a description of the movements induced in an animal by stimulating certain portions of the surface of the brain by electricity ?—Yes, in some cases.

1612. And consequently the brain could not have been removed ?—I cannot pretend to be very accurate in these matters, except with regard to the impression produced on one's mind by what we heard. I think the skull was only opened in some cases, and in others the brain was also partly removed; but I would rather not answer any questions in detail about that, as the experiments were not made before us, but only described.

1613. The contortions of the animal in that case were simply the natural movements of the animal brought into operation by the stimulation of certain portions of the brain ?—Undoubtedly, in some cases.

1614. (*Mr. Forster.*) You mentioned another case, that of a living lobster being cut up before a ladies' school; could you give us any more particulars about that case ?—It was a lady who was the teacher, who had commenced a course of physiological lectures at

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a school which is about 20 miles from London. The complaint was sent to us in the ordinary way under a pledge of secrecy; that is to say, of course the complainant did not wish to be known. It had been told by one child to its parent, and the parent communicated it to us, and we caused inquiry to be made. I sent down one of my assistants. (I did not send an officer down; under the circumstances I decided not to do so), and he saw the lady to whom the school belongs, and he learned that the facts were—that the teacher did take a lobster and put it upon the table, and then cut it; and after one portion of the lobster had been severed there was something more than muscular action, for a part of the lobster crawled upon the table.

1615. Was any chloroform or other anæsthetic administered?—No. The explanation given by the lady is, I believe, that she bought the lobster on her way there, and did not know that it was alive; and I think that is likely to be the case, only I think she was a bad physiologist.

1616. (*Mr. Huxley.*) The shortest and best way to kill a lobster is to cut into its pericardium, and she may have done that as the best way of killing the thing without any bad intention whatever, may she not?—I am told that she concluded that it was dead.

1617. (*Mr. Forster.*) But I think in the mention of this case in the report you make no allusion to that explanation?—It is said that it was carelessness; there was no desire to misrepresent the matter, and we were really under a pledge of secrecy not to divulge the name. These are the words of the report: "Such is the growing carelessness that in one " instance, perhaps unintentionally, a living lobster " they have reason to believe has been cut up in the " presence of a class of young ladies by their physio- " logical teacher." The description given to us by the lady of the school showed that it was true.

1618. (*Sir John Karslake.*) Had she heard it from the little girl?—She was present when it was done, and was horrified by it.

1619. (*Mr. Forster.*) With regard to this lady lecturer, I do not wish to ask her name, but was she a teacher in the school?—No; a physiological teacher.

1620. (*Mr. Erichsen.*) Was she a medical practitioner may I ask?—I think not.

1621. (*Chairman.*) Is it possible to imagine that a lady could buy a lobster, carry it in a cab, put it on a table, and keep it on the table, and then proceed to cut it up, and then for it to crawl about afterwards, and that she did not know during the interval whether it was alive or not?—The lady had in her bag at the same time a dead rabbit on which she experimented before the class, and she is in the habit now of doing the same thing; and I believe that it is her explanation, that she thought the lobster was dead; and I think it is consistent. I think Professor Huxley will agree that so far as we are concerned we are not guilty of any misrepresentation; we have simply used the words put into our mouth, that it was cut up,—in what way we cannot say,—except that there was one portion severed from the body, which did move, and which horrified the people there. To tell the truth, the lady of the school was extremely anxious that no further inquiry should take place lest it should do damage to the school.

1622. (*Sir John Karslake.*) There was a portion of the report of your society in which reference is made to the growing practice among students in some parts of the country of carrying on vivisection; will you just read that portion again?—I think there is an allusion to the circumstance that evidence had been obtained in one case of experiments by students at Edinburgh (as is shown in the paper which I hold in my hand), which experiments were performed by them in their private rooms; but it is qualified here by saying "unknown to their teachers."

1623. Will you read the paragraph of the report in which that is referred to?—I will read from the report an allusion to students being taught vivisections, which is as follows:—"A determined effort is " being made to train students in physiology by the " practice of experiments."

1624. On what foundation does that statement rest?—On several facts. In the first place, a handbook has been published which is designed for no other purpose than to teach beginners, the preface of which declares that it is for beginners, which gives 63 experiments involving pain, and gives a full and detailed account of every instrument that is required for experimentation in order that it may be as plain as possible to the mind of a beginner. This book says the experiments may be repeated any number of times.

1625. What is the name of that book?—The Physiological Handbook, edited by Dr. Burdon Sanderson, in which papers appear by Dr. Klein, Dr. Burdon Sanderson, and Dr. Michael Foster.

1626. Is it upon the fact of that handbook having been published that that paragraph finds its way into the report?—That is part of the reason.

1627. What is the other part?—The other part is that at various meetings which have been held, teachers of physiology have insisted not only upon the desirability but upon the duty of bringing students into laboratories, and instructing them in laboratories by experiments upon animals performed in the presence of the students. Dr. Rutherford is an instance.

1628. What other gentleman can you mention as an advocate for that practice?—Dr. Brunton also told me that if the students were not taught to manipulate and to experiment the next generation would be found without any experimenters, and therefore it was desirable that they should be taught. So far as he was concerned he thought it was right that they should be taught the experiments.

1629. Did that apply, as you understood it, to all students, or to a certain class of students?—To all students who wished to become physiologists; but all medical students are required now to study "practical physiology."

1630. And did he limit at all the cases in which he proposed that these experiments should be made before them?—No.

1631. He did not define them at all to you?—No.

1632. Now, in your own personal experience, which you say has existed for about 15 years, have you known instances yourself where cruelty has been practised by private medical men in their own houses towards animals?—Not any.

1633. Or, as far as you know, has that come to the knowledge of the officers of the society?—As far as I know, it has not.

1634. I ask that question because, as I understand the Bill which the society has laid upon the table here, it is proposed in that Bill that, in the event of reasonable suspicion attaching to a particular person, a justice of the peace might issue a search warrant?—When it becomes necessary to obtain a license, and when persons have to go through a form to get that license, there might probably be a temptation to some persons to perform experiments without really having been properly qualified. This will account for that provision.

1635. With regard to that proposal in the Bill which you have laid before us, is there any instance that you know of in the course of your experience during the last 15 years, or any instance reported to you by your officers in which you would have put that section into force?—Yes; cases have often been reported to me as rumoured.

1636. Was it a mere rumour or was it founded upon any evidence at all in those cases?—Not evidence exactly—strong suspicion perhaps, but not conclusive evidence; but observe, your question was with regard to medical men. Of course medical practitioners as a rule do not perform many experiments.

1637. Now, I understood you to say that there were a class of men who were known to your society who supply animals to the different laboratories or schools?—Yes.

1638. From whom did you get that information?—We got information by watching.

1639. Now, may I ask you this. I think you said that there were certain regulations under which those men supplied the animals to those who employed them?—Yes.

1640. From whom did you hear that there were certain regulations?—From the men themselves; the officers of the society report that to me.

1641. Can you tell us any hospital to which it is alleged animals are supplied, and where particular regulations are issued to the suppliers of those animals as to the mode in which they are to be supplied to the hospital?—Yes, certainly; St. Bartholomew's.

1642. Who would be the person who would probably have to give those instructions if they exist?—The porter at the gate.

1643. And I suppose he would act under the physiologist or some person there who has the conduct of the experiments?—Yes; Dr. Legge or Dr. Brunton, I should think.

1644. (*Mr. Huxley.*) With regard to the case of the lobster which you mentioned, I suppose you know what commonly happens to lobsters?—With regard to boiling them, I suppose you mean, yes.

1645. Does the society ever interfere on that point?—I have been down myself to see them at Billingsgate put into the water, and they die instantly.

1646. But they are put into boiling water?—Yes; I have been there several times in the middle of the night for the purpose of seeing it done, and have found no cruelty.

1647. I should like to ask you a few questions as representative of the society, as to what you conceive would be the effect of the Bill now before us, supposing it to become law. There is a famous experiment which was made a number of years ago on which a great deal of our present knowledge of the physiology of the nervous system is based, called Bell's experiments on the roots of the nerves. It cannot be performed without the infliction of pain. Would the Bill, if it became law, stop an experiment of that character?—I scarcely know the nature of the experiment, not being an expert.

1648. I take it for granted that the experiment is a very painful one, and cannot be otherwise. The Bill would stop it, would it not?—Yes; but you must remember that Sir Charles Bell said about that particular experiment that he merely performed it for the purpose of convincing people who did not use inductive reasoning, and Dr. Ebenezer Watson, himself a vivisectionist, the other day publicly stated that it was the greatest satisfaction to his mind to know that that great discovery was established by only one experiment on one animal, and that experiment was for demonstration.

1649. Suppose that an experiment having a similar value in relation to physiology needed to be performed would the Bill, if it became law, prevent its being performed?—As in our Acts of Parliament magistrates have to construe what cruelty is, and as now there are some things which might certainly be considered cruelty which no magistrate in the world would convict for, so magistrates might be left to construe what pain is under the new statute.

1650. Does this Bill of yours leave any discretion to magistrates?—I must admit there is no room left for much discretion. Such animal, it says, is to be submitted to the influence of an anæsthetic, so as to render it wholly insensible to pain.

1651. The Bill says that the animal is to be wholly insensible during the time that the experiment is being performed, does it not?—Yes.

1652. In that case a magistrate would have no discretion whatever, would he?—A magistrate would have the discretion certainly of dismissing the case if he chose.

1653. That is to say, if he thought it could not be proved that there was pain?—Yes.

1654. But I am now supposing that to be admitted. I am supposing the case to come before the magistrate in this way, that it is admitted that as much pain was given as would be given in that particular case I have mentioned, would your Bill prohibit it?—I admit it would. I do not wish to be uncandid.

1655. I understood you to mention with reprobation the experiments made by the Medical and Chirurgical Society on dogs in order to ascertain whether the methods of restoring life in cases of suspended animation could be improved. Therefore, I presume that this Bill, if it becomes law, would stop all such experiments as those?—I hope so.

1656. Can you tell me whether the Bill, if it became law, would have the effect of stopping such experiments as those which have been recently made by Dr. Klein, and which have thrown such very great light on the nature of small-pox; that is to say, sheep have been inoculated with sheep-pox, and the whole course of the disease has been studied in sheep. No doubt that gave the sheep a certain amount of pain; now would the effect of the Bill be to stop all experiments of that kind?—I really am not competent to say whether the experiment would in that case be considered pain, or whether it could be performed with an anæsthetic.

1657. Undoubtedly sheep-pox is a painful disease. I am supposing that to be admitted?—That is hyphothesis, I think.

1658. There is as much reason to suppose that a sheep is uneasy when it has sheep-pox as that a man is uneasy when he has small-pox, and we know perfectly well that a man is uneasy when he has small-pox. Now, supposing that Dr. Klein, or any such person, were engaged in experimenting in that way, under your Bill, if it became law, it would be possible for a person to lay an information against him, and he might be dragged before a magistrate and charged with committing a breach of the law. I wish to ask whether that would be a breach of the law in your judgment?—As the representative of the society I must say yes, if painful.

1659. About a hundred years ago there was a French Abbé, named Trembley, who made a number of experiments upon what is called the hydra, a fresh water polyp; he made transverse sections and longitudinal sections, and he discovered, what was a very wonderful fact, and one which effected a great revolution in the way of thinking in those days, that each of these things would grow up into a new polyp. Suppose a person were to repeat those experiments in the present day, or anything like them, in your judgment, would he come under the operation of this Bill if it became law?—I must answer in the same way, that assuming that there is pain he would; but I am not inclined to say there was pain inflicted in that case.

1660. There is no limitation in your Bill as to the kind of animal or as to how much pain is inflicted?—No.

1661. One of the commonest things shown in a class is the circulation of the blood in a frog's foot. To show that you have to tie the frog's toes out, and otherwise make him more or less uncomfortable, and then you see the circulation, which is a very wonderful sight. Should I, as a teacher of physiology be prohibited from performing that experiment if this Bill became law?—I should think not. I should think that would scarcely be called pain.

1662. I beg your pardon. I should admit at once that there was pain there?—Speaking microscopically it would be pain, I suppose.

1663. I want to know what physiological people are to expect if the Bill becomes law?—At the present time you must remember there is an Act of Parliament which provides that you shall not illtreat or torture an animal; and in applying the law therefore at the present time, it has to be determined what is torturing or ill-treating an animal. It might be contended in a given case that there had been ill-treatment or torture in a very small degree, and thus if Dr. Klein's sheep were tortured no more even than your frog in the case you have put you might possibly contend that now under the present Act we could take Dr. Klein

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before a magistrate; but you must remember that no society, no prosecutor, can ever go beyond public opinion, and therefore it is idle to talk about infinitesimal cases of that kind, I submit.

1664. I think you said that you thought it would be desirable that the Bill should give the society power to test whether experiments were doing any good?—I said that the object would be twofold; the first advantage would be that it would enable the society and every person else to judge, from the register of experiments performed and the results which had been achieved, whether the experiments were of any value at all. Secondly, it would prevent a repetition of experiments, because the publication of the experiments would go to the ends of the earth, and prevent any unnecessary repetition of experimentation.

1665. Then am I to gather that you think that the Society for the Prevention of Cruelty to Animals is a fit high court to which to refer scientific inquiry, to judge whether a particular class of experiments is doing any good or not?—I beg your pardon, I did not mean anything of the kind.

1666. I ask the question because that is one construction which your words would bear?—I did not mean that; I meant this, that all persons would be enabled to form a judgment, and I suppose members of the Royal Society for the Prevention of Cruelty to Animals can form a judgment as well as those who are not members of that society, some of whom are very well able to form a judgment scientifically. It would enable them as well as other members of society at large to form a judgment on that question.

1667. But in the long run it would constitute the society judges of the scientific value of a given result?—Only as we are judges now of anything; judges, for instance, of the operation of any Bill that may be brought in during the next session of Parliament.

1668. (*Mr. Forster.*) You mean that it would give that publicity which would enable you to form an opinion of the value of the experiments?—Yes.

1669. (*Mr. Huxley.*) But I do not see how any but experts can form any opinion as to their value. I want to know whether that is one object of the Bill to constitute the Society for the Prevention of Cruelty to Animals a sort of court of revision in these matters?—Do you not think, I may ask, the society would be able to judge whether for instance the experiments referred to by you had prevented sheep disease or cattle diseases or not, if those diseases disappeared by the application of the remedies discovered by the experimenters.

1670. No, I think not. I think that if the society contented themselves with reading Dr. Klein's memoir recently published, unless I am much mistaken, there is not one in 500 who would understand anything about it?—Not the technicalities; but the general results we might understand. I hope so, else I think the experiments would be of no great advantage to humanity.

1671. (*Chairman.*) What I understand you to say is that you wish this Bill of yours to be understood as subject to reasonable interpretation like all other Bills for the prevention of cruelty?—Of course.

1672. And if the terms of it are such as to exclude a reasonable man in the chair of the magistrate from determining whether there is cruelty in the particular case, you do not think the society would insist upon those particular words?—I think not.

1673. Then the only purpose for which the society are to be judges is whether it is a case in which they ought to institute a prosecution?—That is so.

1674. And then it would be for the court to determine whether they had sustained that prosecution?—Yes.

1675. And experts might be brought forward for the defence if it was desirable that the evidence of experts should be heard. Is that what you wish us to understand?—Yes, as in the case of the Norwich prosecution, which I conducted myself. We had there doctors against us and doctors for us, and the contention was then, as would be under our Bill, was pain given or not.

1676. The phraseology of the Bill, as you have laid it before us, does not seem to some of us to admit of that reasonable interpretation, but seems to be too precise, that the court must necessarily act upon the mere words without taking the reasonableness of the thing into consideration at all. If such should be the true legal interpretation of this Bill, are you authorised on the part of the society to say that they would wish the words to be qualified?—The society consists of practical men and reasonable men. In discussing this Bill before the Government I am obliged to state that it would be impossible for the society to move from their position.

1677. But you have now been asked whether, if your Bill were to pass through Parliament in the shape in which you have submitted it, such and such consequences would not follow; and you have said that in all other cases, and particularly you say in the Norwich case, the reasonableness of the conclusion is submitted to the judgment of the court; would you take upon yourself to say that the society really wish that in any Bill which should be framed in obedience to their suggestions, the reasonableness in this case also should be taken into consideration?—Yes; of course they would be very glad to have the most reasonable phraseology used.

1678. (*Mr. Forster.*) I want to ask you a question following upon Professor Huxley's questions, not so much as regards the actual wording of the Bill, but as to the intention of the committee of the society with regard to legislation so far as you can give it. Now it has been stated to us in evidence by Mr. Simon that he considers that by giving animals a disease, and I imagine a painful disease, he obtained information with regard to cholera which he considers is of very great service in sanitary legislation, and in enabling an Act of Parliament to be passed which will very likely diminish cholera. Now if this Bill had been passed, or if any Bill had been passed such as your society wishes before he tried such experiments, would they or would they not have desired that he should have been prevented by law from trying them?—I think if it were any very severe infliction of pain on animals, perhaps the society would like to have prevented that. But the society has come to the conclusion, from reading the history of vivisection, the history of the experimentations, that one generation of experimenters arise for the purpose of correcting the other experimenters, and that each generation of experimenters has really some panacea which turns out afterwards to be of no value at all.

1679. My question was not with regard to vivisection, but with regard to the giving of disease. What I wanted to know was the exact desire of the society. Do you think that the object of that committee of the society which has framed this Bill would be to prevent such an experiment as that, and that in fact the law which they would wish passed would have prevented that if it had been passed?—I think so if it were a very severe act of cruelty.

1680. I have interpreted it to be the giving of a disease?—A disease might be given, I apprehend, without much pain being caused to the animal.

1681. But before you would have allowed that thing to have been done you would have required not merely to have been satisfied that there was a reasonable probability that it might result in the prevention of cholera, or do something towards the prevention of cholera, but you would also have wished it to be proved that there would not be severe suffering caused to the animal upon whom the experiment was tried?—I am quite sure that the society would not prosecute, although they might desire the law, in any case such as you are suggesting, and I say that from the course which is taken now very frequently, for instance, in a case where a horse had been over-ridden to save a life.

1682. (*Mr. Hutton.*) Have you handed in the evidence of the cases of experiments by students in Edin-

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burgh, to which you have referred in your written statement?—Yes, I have.

1683. You have nothing to add to what is written there?—No; except that in regard to London I have not been able to find a single case where the students have been employed.

1684. Are the names of the students given and the nature of the experiments in the paper which you have handed in?—Yes, of my informant.

1685. (*Chairman.*) Will you read the paper you have with regard to experiments made by students?—Yes. This is a statement made by a student of Edinburgh, and since Lecturer at the New Veterinary College, Edinburgh, on Materia Medica, who saw Mr. Ernest Hart's letter in the newspaper and wrote to us in consequence.

1686. And by the desire of the society it is laid before us?—Yes.

1687. (*Mr. Huxley.*) What is the date of it?—I think it is about six months ago. "Observing from the daily papers that Mr. Ernest Hart alleges that students do not perform experiments on living animals as an exercise in the prosecution of their studies, I beg to forward to you a summary of my experience in that respect during my college career at Edinburgh. I am a veterinary surgeon, and comparatively unknown, but I feel it my duty to aid your society in repressing unnecessary experimentation, surveying the past as I do with much regret, so far as I have participated in the practices which I am now compelled to condemn. At Edinburgh the veterinary students and the medical students frequently associate for pleasure and for study. During my first term I was admitted only to two private meetings where experiments were conducted by students alone; but in the following term, having become a senior, I was introduced to a great number of such vivisections, and on some occasions operated myself. The experiments were certainly never designed to discover any new fact to elucidate any obscure phenomena, but simply to demonstrate the most ordinary facts of physiology. Our victims were sometimes dogs, but more frequently cats. Many of the latter were caught by means of a poisoned bait, the animal being secured whilst suffering from the agonies caused by the poison, when antidotes were applied for their restoration. They were then imprisoned in a cupboard at the students' lodgings, and kept there until a meeting could be arranged. Sometimes the students secured their victims by what is known as a cat hunt, that is a raid on cats by students armed with sticks late at night. I am not prepared to say that the object of the students was to commit cruelty, or that there was any morbid desire to witness pain, but I say emphatically that there was no other motive than idle curiosity and heedless reckless love of experimenation. What, for instance, could justify the following experiment, performed for the purpose of witnessing the action of a cat's heart? The operator first of all made an incision through the skin of the animal's chest extending from the neck to the belly. The skin was then laid back by hooks, in order to enable the operator to cut through the cartilage of the breast-bone, and to draw his knife across the ribs for the purpose of nicking them. This process is necessary to enable him to snap the ribs and lay the fractured parts back, which also are secured with hooks. It is needless to say that such operation is a most cruel one; but it is only one of several others performed at Edinburgh. Now, the action of the heart is well known, and is one of the first things taught to students of physiology, and can be taught as well without experimentation as with. In a few cases the animals were narcotised, when no suffering was caused either in the process of poisoning or in the after experimentations. The securing an animal for an operation like the above requires experience and care, and it is fearful to witness the struggles of the animal while this is being done. I desire to exonerate the professors from any participation in the experiments performed by students which were conducted at the private lodgings of students, when none but students were present. I merely write this in order to give my humble corroboration of the statement made in the memorial, that students are in the habit of performing experiments. James B. Mills, M.R.C.V.S." The writer of that letter is a veterinary surgeon at the present time in the Royal Artillery at Woolwich.

1688. He does not state whether that particular experiment which he describes with so much detail was made under the influence of narcotics or not?—No, he does not, except by saying that it was a most cruel experiment.

1689. (*Mr. Erichsen.*) I should like to put a question in reference to what came out in the evidence of a very eminent surgeon at the commencement of this inquiry, with regard to the provision in the proposed Act by which it becomes incumbent to administer anæsthesia in all painful experiments. An inquiry was mentioned to us, as of the very utmost possible practical character, which is necessarily attended with considerable pain, and that is in the discovery of an antidote to the snake poison; there are at least 10,000 people who die every year, in India from snake bite; and there is no possibility of arriving at any knowledge of an antidote for snake bite, and consequently no hope of saving the lives of those people except by submitting animals to be bitten by snakes and then testing the power of the alleged antidote upon those animals. That, I imagine, would be a very painful experiment to the animal, and it would be impossible to perform that experiment under anæsthesia, and yet the result of such an inquiry might have been of inestimable service to humanity and a direct service because it is not a scientific, but a purely practical inquiry. Would the provisions of the Bill interfere with such an inquiry or investigation?—I presume they would, but at the same time I would say that Sir William Fergusson, another eminent surgeon, has expressed a very strong opinion that these experimentations are entirely useless.

1690. No result has as yet been arrived at, no antidote has yet been found, but the only hope of discovering an antidote, as Sir James Paget told us, in the next 20 years, would be by continuing experiments of that kind on animals. Is it your opinion that the Bill as proposed would interfere with the performance of such experiments, which would be of the most direct practical benefit to mankind, and would, if successful, save the lives of thousands annually?—It would be of value to mankind if it would save lives; but that is the whole question I presume. If it could be established at once that an experiment would yield a blessing to mankind the result would be already known, and there need be no experimentation at all; it is the uncertainty of the thing that requires experimentation. I therefore would not admit the premiss that the experiment would necessarily be of the most direct practical benefit to mankind.

1691. (*Chairman.*) But the question put to you is whether your Bill would prevent the process which the Government of India are now carrying on for the purpose of preventing the loss of life by snake bite?—I am afraid it would.

1692. Take another case; we have been told that the condemnation of Palmer for one of those numerous murders, and the condemnation of other criminals, has been very much due to certain experiments made upon animals by the use of strichnine or other poisons; would your Bill prevent that?—I am afraid it would. But at the same time, I would like to say that it is a very difficult thing to answer these questions in the way I am doing now, because I have in my mind at the same time the statements made by a man like Sir Charles Bell, who says that experimentation has not yielded the results claimed for it. I am not going to say that experimentation has not yielded anything; at the same time, I cannot allow it to be assumed that the experiment is going to answer.

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1693. But we are assuming nothing, except that it is the object of the experiment to discover a cure for snake bites, or to bring a murderer to justice. Would your Bill prevent an experiment as to the effect of strychnine on an animal, or subjecting an animal to a snake bite?—I should say so; but I am sure Sir John Karslake will tell you that there are many things which you must not do, even to bring a murderer to justice. Although it would be a good thing to bring a murderer to justice by certain short cuts, the practice of the courts do not admit of them, because the end does not justify the means.

1694. I want you to understand that the object of the questions which have recently been put to you, and are now being put to you, is not to argue it one way or the other, but merely to ascertain whether the effect of this Bill, if passed in the sense in which it is submitted to us by the society, would be to render such experiments impossible?—I think so.

1695. However strong the scientific opinion in favour of the experiment might be, do you think that those who have submitted that Bill to us would wish the Bill to pass in a form which should render the experiment impossible?—I think that they would like to have it passed in this form, and if that is not possible, then to get it as near as possible to this form.

1696. (*Mr. Forster.*) Do you think that they have had before them at all this case of snake poisons?—Yes, I think those cases have been before the committee.

1697. Could you tell us whether it occurred to them that there was any other way of finding out whether a supposed antidote was likely to be an antidote than by giving an animal poison, and then the antidote, to see whether it cured it or not?—There are many ways, perhaps. I have seen experiments to test the action of poisons upon dead animals, upon the hearts of frogs, for instance. The heart has been taken out from the body of the animal, when the pulsation continues for a long time; and I have seen the poison tested in that way without any cruelty at all. I do not know that that would be applicable in all cases, but there may be some other means by which the want would be met. It may be a more easy and a more rapid way of doing the thing, and one that would prevent a good deal of thought and study to experiment on a living animal, but the question is, is it right. I do not say absolutely that it could be arrived at in any other way, but I say it is possible it might be; but it would be better to kill the snakes than to allow them to go on biting people, and then provide antidotes by killing and torturing other animals.

1698. (*Mr. Huxley.*) Did I rightly understand you to say that the society, in drawing up this Bill, have been actuated, among other things, by a firm belief that physiological experimentation has not done much towards the discovery of new truth?—I think that is the general impression of the society, as regards the treatment of disease.

1699. On whose authority may I ask?—When I speak of the society I am speaking of the committee rather, the society itself consists of many members.

1700. Is there anyone in the committee who is conversant with the history of physiological science in the last 20 years?—Yes, I think so. There is one member of the committee who is a doctor of medicine, and has had a good practice at the West End, who says he does not believe vivisection has done any good to his profession; and another member has made the subject a special study, is the author of several medical books, and the editor of a journal which includes physiology and anatomy, entitled *The Veterinary Journal.*

1701. (*Chairman.*) In giving in the documents connected with the committee, have you given the names of the committee?—No, I have not.

1702. Will you do so?—Yes.

The witness withdrew.

Adjourned to to-morrow at 2 o'clock.

Wednesday, 20th October 1875.

PRESENT:

THE RIGHT HON. VISCOUNT CARDWELL, IN THE CHAIR.

The Right Hon. W. E. FORSTER, M.P.
Sir J. B. KARSLAKE, M.P.
THOMAS HENRY HUXLEY, Esq.
JOHN ERIC ERICHSEN, Esq.
RICHARD HOLT HUTTON, Esq.
N. BAKER, Esq. Secretary.

Mr. A. de Noé Walker.

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MR. ARTHUR DE NOÉ WALKER called in and examined.

1703. (*Chairman.*) I think you were originally a military surgeon, were you not?—Yes.

1704. You then became a private practitioner in London?—Yes.

1705. And on the occurrence of the Crimean War you desired to return *sub modo* to the military service, and were attached to a regiment in the Crimea?—Yes.

1706. And you have since that returned to practice in London, and are now a physician?—Yes, I have been in practice ever since in London.

1707. Now I believe you have had considerable knowledge on the subject into which we are inquiring, and have been a student in physiological laboratories chiefly on the Continent?—Yes, in two or three, and I have read up generally what has been since published.

1708. And you have kept yourself acquainted, through foreign publications and English publications, with what is going on generally upon the subject?—Yes.

1709. You have paid special attention to it probably since it became rather more generally interesting to the public than it was before?—I particularly looked into the working of physiological laboratories from 1856 to 1857–58, and again in 1872.

1710. When you were studying in physiological laboratories abroad what was your general impression with regard to the sufferings of animals?—My general and very decided impression was that a great deal of it was wanton and unnecessary.

1711. Do you believe that there is anything of the kind which you there condemned in this country now?—Not from my own personal experience in physiological laboratories in this country, but from reading experiments published in this country, there is, I believe, much to be condemned.

1712. Are you disposed to say that what you saw uncondemned in foreign laboratories still continues?—I have every reason to believe so.

1713. Can you support that opinion by a reference to any publications, foreign and English?—Yes, certainly.

1714. You understand that I particularly put that question to you with reference to what is going on abroad at the present moment?—I read every week

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the *Gazette Médicale de Paris* which contains more about experiments on living animals than any weekly publication that I am acquainted with.

1715. Now could you give us from that publication, or from any other Continental publication, extracts which would illustrate the opinion that you have expressed?—Yes.

1716. Will you have the kindness to do so?—Yes.

1717. Have you observed in the last two or three years any tendency to the extension of these practices in this country?—As far as I can judge, from reading what is published, I should say they were very much extending indeed. It is almost a new phase of research, and is extending everywhere.

1718. Do you consider that there is a necessity for interference on the part of the State, to prevent the occurrence in this country of what you have seen obtain in other countries?—Decidedly; and I think it can be done without the least interference with scientific research, but rather the contrary.

1719. Are you prepared to support that view by extracts from publications in this country?—Yes, to a less degree than I could abroad, but I can bring forth enough to support my assertion.

1720. I think you have been suggested to us as a witness of medical authority who will represent to us the evils which are already resulting, or are likely to result, from the extended practice of vivisection, as it is called, in this country; you are in communication, I think, with the Society for the Prevention of Cruelty to Animals, they have asked you to come and give evidence?—Yes. But I perceived the necessity of limiting and legally controlling the practice many years ago, and commenced agitating the question long before I had any communication with the said society. I had compiled my notes, in the hope of appearing before this Commission, long before the society communicated with me.

1721. Will you specify the points, as they occur to you, in which you considered those consequences likely to follow?—The knowledge of vivisections, or rather experiments, that I have, consists, first, of vivisections performed solely with the object of studying physiology, of acquiring that is, knowledge of the healthy functions of animal life; secondly, of vivisections occasionally made with the object of ascertaining if certain surgicial operations are likely to prove successful in the human subject; thirdly, of vivisections made with the object of acquiring manual skill, with the view of performing the same operation on the human subject; fourthly, of experiments made with the object of studying the effects of traumatic lesions; of experiments made with the object of studying the effects, pathogenetic or toxic, of certain gases, mineral and vegetable substances, of certain animal fluids, and of the electric current. The greatest number of vivisections are made of course under the first head (to my knowledge at least), that is to say, for the purpose of acquiring knowledge of the healty functions of the human body; and it is here that I think the greatest amount of evil to be done away with exists. I need not, I suppose, enter into the question of how many physiologists, I mean attached to schools of medicine, are constantly misleading their pupils as to the purport of these experiments. Practically, as I was told by the last vivisector under whom I studied, this knowledge "has no other object in view than that of "contributing to the progress of medicine, and medi-"cine has no other object in view than that of healing "disease, the restoration of health to the sick being "evidently the ultimate end of all the physician's en-"deavours." If therefore physiological knowledge is or has been, therapeutically, of any use to the physician, it must have been the means of directing his mind to the discovery, or at least selection, of certain remedial agents. This is in fact that which experimental physiologists have been promising the medical profession for some centuries, and particularly since Réaumur commenced his experiments on the digestive functions 122 years ago. If now we place before us all the oldest and most recent works on practical medicine, as well as the best clinical records ever published, and patiently search through them all, in the hope of finding a single instance in which this promise has been fulfilled, we shall fail. I will explain what I mean by that, because it is very strongly stated. I mean to say that the knowledge of the healthy functions of animal life does not contain in itself any knowledge that can lead us to therapeutic agents for the treatment of any disease by which those parts may be affected. If one therapeutic agent has been occasionally suggested, has anyone ever found it of any use? I will not seek to prove this by referring to any of the complex and often mysterious functions or diseases of the nervous system, but will refer to the physiology of the heart, and to the discovery of the circulation of the blood, as not only highly interesting, but very well known, and the constant boast of vivisectors. Did physiologists, ancient or modern, after mastering the mechanism and innervation of the heart and general circulation, announce that digitalis, aconite, spigelia, belladonna, iodide of potassium, &c., &c., would affect the heart and circulation in any one way whatever? Secondly, now that physicians have informed physiologists of most of the effects and symptoms those agents produce on the heart, can they tell us why and how they so act? Thirdly, even if they could now explain the elective action of those particular substances on the healthy or diseased heart, how could that knowledge enable them to state what other agents could act in an analogous way on the same organs? Ignorance, however, of how or wherefore a remedial or other agent acts in a particular organ or tissue would, I think, be condoned if any vivisector, from his knowledge of the healthy functions of any one organ or tissue, had in a single instance deduced that a certain agent would act in that particular organ or tissue, either in health, or when attacked by even one, of the many diseases to which it is liable. Again, I say, how was it that after mastering the physiology of the iris, the physiologists did not turn round to the physician and surgeon and inform them that belladonna would dilate, and that the Calabar bean would contract it? Was it through a knowledge of the healthy functions of the human body that the discovery was made that lemon juice could cure and prevent scurvy, that opium will lull pain, mercury and iodide of potassium neutralize syphilitic virus; that colchicum will often cure gout, and quinine intermittent fever? If as some, indeed most physiologists, believe, that the disease called diabetes originates in the liver, and therapeutics are to be studied and practised according to our knowledge of the healthy functions, why is not some course of treatment suggested for that disease? I beg to observe to the Commission that I am not now dealing with the question as to whether physiological knowledge is or is not useful to the physician; neither is it my wish to appear to undervalue physiological knowledge. I am confining myself under this first head to vivisections made with the sole object of acquiring physiological knowledge, and my proposition is that that knowledge, acquired at the cost of intense suffering to millions of victims, has never been the means whereby any one remedial agent has been discovered for the cure or even treatment of any one single morbid state, a point on which vivisectors habitually mislead themselves and their pupils by reckless assumptions renewed every year. I am alluding of course to experimental physiologists attached to universities and schools of medicine. The pretentions of many vivisectors, however, go much further. The physiologist I have alluded to not very long ago informed his class that "they must never lose sight of the funda-"mental principle that physiology is the foundation "stone of pathology." He said that in my presence. One instance only will place before the Commission the whole truth respecting the vanity of the above "fundamental principle." What the nature of marsh miasma is the physiologist does not know. What the organic changes are that take place in the organism when inquinated by marsh miasma the physiologist

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does not know. How quinine acts curatively in intermittent fever generated by the marsh miasma he does not know. The assertion published by Professor Rivière, of Montpellier, is perfectly true. He said "that the discovery of the circulation of the blood had not advanced medicine a single step." What, then, is the use of physiological knowledge to the physician? Knowledge of the healthy functions, as far as they are known, or can be known, enables the physician, not in all but in many cases, to determine the seat of disease. This knowledge must not be underrated. If by comparing the normal functions of, for example, certain portions of the nervous system with the disordered functions of the same parts in a patient, I am enabled to localize the disease in one of the nervous centres, I have attained unto important knowledge, notwithstanding that for the remaining elements of the diagnosis and for the treatment I must leave the dominion of physiology and seek for information elsewhere. I should like now to say something about the second head, as to surgical operations. As regards vivisections made with the object of ascertaining if certain surgical operations are likely to prove successful in the human subject, I have not myself any personal experience; I mean that I have not seen any surgeon perform any such experiments. All the knowledge I possess about them is from reading. Thus I have read of the transplantation of periosteum, of the deligation of arteries, &c., and of the transfusion of blood. The two latter are the most important, and have undoubtedly saved many lives. I beg to observe, however, that vivisections made under this head must be appreciated in a very different way from vivisections performed under the first head; so much so, that in my humble judgment all the legal restriction that I would urge regarding them is this, that they should be performed only by clinical surgeons. The art, and I may add, science, of surgery is nearly perfect, and I believe few, if any, experiments on the lower animals will be necessary to bring it to perfection. The misplacement of many facts I alluded to, earlier in my evidence, consists in this, that all the useful discoveries made under this head are sometimes ignorantly, and sometimes designedly, ascribed to vivisections made by experimental physiologists, whereas they have been suggested, as far as my knowledge goes, by physicians and surgeons in practice. Thus, transfusion is no doubt a vivisection just as bleeding is, but it is not a discovery made by professional vivisectors. A woman, for example, may be flooding, or a man wounded may have lost blood, and the self-evident cause of danger is want of blood. This can hardly be called a discovery. Neither was the remedy, or rather the expedient of transfusing blood into the patient a discovery; it was an invention, and an invention recurred to, not by professional vivisectors, but by men called to the highest scientific calling, I mean men in the actual practice of medicine and surgery. I do not wish to underrate anything or to overrate anything, but the attributing of certain results in order to justify certain abuses is what I have in my mind in mentioning these examples. Regarding vivisections under the third head, made with the object of acquiring manual skill with the view of performing the same operation on the human subject, I have only this knowledge: a surgeon under whom I studied took six goats and triphined the skull of all six, to no purpose in my opinion.

1722. (*Mr. Erichsen.*) Was that in England?—No.

1723. How many years ago was that?—It was when I was a student.

1724. (*Mr. Forster.*) Was any anæsthetic used?—Anæsthetics were not known then.

1725. (*Mr. Huxley.*) What did he do it for; for amusement?—No, he wanted to see the result of triphining the skulls after the animals were dead. If they had recovered he would have killed them, and when they died he made a post-mortem examination of them.

1726. So that it might have been possible that he performed his operation for the sake of knowing what takes place in the human subject?—Certainly. It is fair to state that there is no doubt as to the object of his performing the operation; but I remarked at the time that six were too many, and it would have been quite enough if he had done it on three. Under my fourth head the experiments are very numerous and extremely abused, often aimless and ill conducted by experimenters. I will not enter on that because it is the subject of a separate paper, but the experiments I would desire to bear testimony about are:—(1), on electrophysiology; (2), on the artificial generation of morbid states; (3), on the pathogenetic physiological and toxic effects of certain mineral, vegetable, and animal substances.

1727–8. (*Mr. Forster.*) Have you any special remark on the artificial generation of morbid states?—Yes. On the induction of morbid states much may be said, as these experiments have greatly increased within the last few years. My own individual experience regarding these experiments, with one exception to which I will advert in a few minutes, is limited to inflammatory states caused by caustics, cutting instruments, the insertion of foreign bodies into the tissues, and to the inquination of corrupt animal fluids. Inflammation by chemical and traumatic agents was set up in the joints, and in the transparent cornea of the eye by passing a thread through it, and establishing a seton. These experiments also caused great pain, and the lambs and dogs on which they were performed were unable to rest day or night, and if some ease enabled them occasionally to rest, the experimentor used to exasperate the wounds afresh, and thus made rest impossible. I cannot say whether these experiments have revealed any facts that have led to useful therapeutic results. I wish particularly to declare, however, that if they have, their use will be limited absolutely to cases where the injury has been caused by traumatic or chemical agents, on parts *previously healthy.* This condition, as far as my experience goes, is always ignored by experimental pathologists, and they lecture or write on inflammation so induced just in the same way as if it had all arisen from any one of the many diathetic diseases to which joints are liable. A traumatic lesion, bringing on inflammation and its effects, in the healthy tissues or joint of a dog or lamb, cannot be compared to a disease in the corresponding joint of a human being or other animal, in whom the morbid state has an hereditary or scrofulous origin, and causing a long series of morbid changes, ending in, *e.g.*, "white swelling."

1729. (*Chairman.*) Has your attention been called to any legislative remedies, which you are desirous to recommend for the consideration of the Commission?—Yes; I can answer that question by stating two or three heads which, in my opinion, would meet all the cases that I know of. I would propose first that no person should be allowed to perform experiments on living animals without legal permission from some competent authority. Exceptions should be made, however, in forensic cases when the analyst might find it necessary at once to test suspected matter on some animal. Secondly, the number of animals allowed yearly to each licensed experimentor should be limited. I think if this is not done it will invalidate any other restriction that can possibly be thought of. Without casting a reflection on anybody, I will only say that if I were a licensed vivisector, I might go on all the year, and have persons in my laboratory who are not licensed, and they might perform experiments under me, or nominally under me, and the number would be excessive. I could bring forward many cases in which ten, twenty, or thirty animals have been subjected to the same experiment and have given in each case the same result; and I consider that a cruel abuse of power. The third provision would be this: that every licensed experimentor should send in every year to the same competent

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authority two returns; one showing the nature of the experiments he intends to perform; the other the results obtained by these experiments. The first would prevent a great many useless experiments, either because they are quite unnecessary, many having been probably performed before, or because they might be aimless or useless. The second return would test the ability and humanity of the experimentor, and the utility of the experiments, which, if published, could at least be referred to by other experimentors. This return should also show whether anæsthetics were used, how long the animal remained insensible, and how long it was kept alive, this last item should not be omitted. There are very great abuses, I think, on that head; animals to my knowledge have been kept alive when they might have been destroyed long before. I will give you an instance of that. In looking over my papers, notes, and memoranda, made while attending lectures, I have found a small diagram proving that which I have always asserted, that experimental physiologists attached to schools of medicine have been to my knowledge guilty of great abuse of power. The sketch represents a frog prepared in this way; the two sciatic nerves are laid bare for about half an inch; the animal is then placed into a small trough containing oil or glycerine and kept *in situ* by nailing its feet; in this state the animals live as long as nature can endure such torture while the experimentor may apply the galvanic current to the nerves, or otherwise "stimulate" them whenever he feels disposed to do so.

1730. (*Mr. Huxley.*) What was the purpose of that experiment?—To preserve the nerves from drying up and withering. I said at the time, "It is a "great abuse of power. There is a great deal of "suffering in nature, but not cruelty;" and I got my usual snubbing. I think also a physiological laboratory ought to be under some sort of proper supervision, because I have seen animals very uncomfortable from want of cleanliness and air, and I have seen frogs kept in closed jars for months till ulcers formed round the mouth and nose; and what pained me most was that they excited no pity, and the person who had charge of them was not at all found fault with, but the animals were exhibited by the professor as showing the evil effects of close confinement, as if similar results were quite a novelty for all the rest of the world.

1731. (*Mr. Hutton.*) Was that in England or abroad?—I am unable to remember at this moment; I can ascertain by referring to my notes. Finally, vivisections for "demonstrations" should be entirely abolished. I am speaking to men of very considerable knowledge of the subject—much more than I have. I do not deny that nature intended the eye to teach the intellect. It would be absurd to do so. Impressions on our minds are very forcibly made by vision, and, therefore, to say that demonstrations do not impress phenomena upon the student is not right; but I think myself that, the moment we have knowledge of the topography of the parts we are describing, the function of those parts is admirably well understood by a verbal description of them, and the amount of suffering is so great, that where knowledge of functions can be attained without demonstrations, they ought to be done away with. For topographical anatomy and the relationship of parts demonstrations are absolutely necessary. For a knowledge of the functions of those parts and systems demonstrations are not necessary. I have got a note here that I should like to read, and that is, that wurari is not, as far as my experience of it goes, an anæsthetic at all, and, therefore, its use for that purpose should be forbidden.

1732. (*Mr. Forster.*) Could you give the ground upon which you form that opinion?—Yes. A dog, for example, inquinated by a dose of wurari, may feel pain. And I judge that he feels in this way:—If, for example, he is petted and spoken to kindly he will try and wag his tail, from which, I suppose, there is still some amount of intelligence and feeling, and if you walk round the room he will follow you with his eyes.

1733. (*Mr. Huxley.*) When he is full of wurari do you mean?—When he is lying down intoxicated with it he will try and wag his tail if you talk to him.

1734. Can a dog full of wurari use a single muscle?—He will try and wag his tail.

1735. Will he when he is fully poisoned with wurari?—Not when he is "fully poisoned," but he will do so under a dose that will paralyse the respiratory organs. I have memoranda in the next room that would clear up the subject very much. Claude Bernard, in 1872, in my presence, and since, has used these words, that "the dog cannot manifest pain, but he feels it." I have got all that Claude Bernard said in the next room. It divides the nervous system in fact, the sentient from the motor. I should like to explain what I meant by saying just now that the legislative means which I suggest would rather promote science than the contrary. A great many experiments are performed in one laboratory which are not known in others, and if there was a return constantly sent in one experimentor would very easily compare his experiments with others, and then the faults of experimentors might be very well detected, and the number of victims very much diminished.

1736. I rather desire to make perfectly clear in my own mind what you mean on some important points. In the first place, I gather from what you have stated that you do not wish to interfere in any way with scientific research. Your opinion, however, is that scientific research might be conducted with a much greater economy of pain than is now the case?—I am sure it could.

1737. But I understand that you do not sympathise with those who hold that such inquiries ought to be put down altogether?—I will answer that question in this way: if you were to ask me, personally my wish would be to see vivisections entirely abolished; but, as is the case with many things that I consider evils, or necessary evils (or if you think the word too hard I will alter it), feeling perfectly satisfied that I cannot ever see them entirely abolished, my object has been for the last two years as much as possible to get vivisection put under legal restrictions.

1738. Then I understood you to say, and this is another point on which I should be glad of some enlightenment, that knowledge of healthy functions does not contain in itself any knowledge which can lead us to therapeutic agents which may help us to cure diseased parts; but from what you said subsequently, I gathered that you meant this that the knowledge of healthy functions does not actually enable you to say what particular drug should be used in any particular case?—Not only what particular drug, but it does not result in any course of treatment.

1739. And I further understood you to say afterwards that you did not by that intend to throw the slightest discredit upon knowledge of healthy functions as the principal instrument of diagnosis?—That is too strongly put for me, "the principal instrument of diagnosis." No one single science whatever can be the principal instrument of diagnosis, except in a few cases, such as, *e.g.*, the detection of sugar in the urine by chemical science. A diagnosis comprehends the seat, the pathology, and etiology of a morbid state. Physiology can only help us, occasionally, to determine the seat of disease in the living subject, and there its use in medicine stops, absolutely. Chemistry and pathology, on the other hand, greatly help us in diagnosing the nature and causes of disease both in the living and in the dead. The localisation of disease, which we are able to attain to by knowledge of the healthy functions, is very much limited to the nervous centres. A knowledge of the functions of the liver would not at all help us in diagnosing disease of the liver.

1740. You do not deny the value of knowledge of healthy functions as a means of diagnosis of disease?—I will repeat what I have said in my statement;

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I do not wish to put it too strongly. But I do say that the knowledge of the healthy functions of the human body enables us, in some cases, but not in all, and especially in the case of the nervous centres, to localise the seat of the disease. You know better than I that the knowledge of the functions of the kidney, we will say, does not enable us to know the nature of the disease there, but chemistry might. You may know the functions of the bladder perfectly well, but you would not recur to the knowledge of its functions to find out what disease affected it.

1741. Would you say that the knowledge of the structure and functions of the urethra, or the neck of the bladder, was of no use to you in treating diseases of that region?—I am certainly not aware that it is; the anatomy of it is; but I am not aware of the knowledge of any functions of the neck of the bladder being so.

1742. Then if the structures round the neck of the bladder were not contractile, would it be the same thing as if they were?—But that is a case of pathology, I should say.

1743. Of course I need not ask whether you attach great value to the use of the stethoscope?—Yes, certainly.

1744. Or whether you attach weight to certain modifications of the second sounds of the heart?—Certainly.

1745. You base certain conclusions as to the kind of disturbance of the functions of the heart on the modifications of that second sound?—Yes.

1746. Can you tell me whether there is any doubt at present that the cause of the second sound of the heart is the action of the semi-lunar valves at the origin of the aorta?—What I was taught by the physiologist was, that the cause of the second sound is a complex phenomenon, and that it is not only due to the action of the semi-lunar valves of the aorta.

1747. Are you aware of any experiments made on that subject?—No, I am not.

1748. That 25 years ago the whole question was settled by direct experiments, and there was no other way of settling it?—I am not aware of that. The sounds of the heart, as far as I remember back, were not very satisfactorily accounted for.

1749. At the time to which you refer it was a matter of great controversy, for no other reason than because it had not been examined experimentally. It was then examined experimentally, and now it is placed on a certain basis, and it is a matter notorious to physiologists?—I do not profess to be a special physiologist, but I was taught that the sounds of the heart are due to complex causes. What I insist upon, however, is this,—a physician knows the parts involved in causing the first and second sounds of the heart. If a patient consults him in whom the sounds are not physiological, he at once bears in mind the anatomy and physiology of the parts involved in causing the sounds, and is thus often able, at once, to determine the seat of disease. But physiological knowledge cannot lead him any further, absolutely. In order to determine whether the mischief is due to a rheumatic or gouty diathesis, or to some strain of the parts, he leaves physiology altogether, and recurs to other sciences. Sir William Gull has lately published his judgment, that "'the clinical physician knows that the "'phenomena of disease are not explained by the "'phenomena of healthy texture,' nor by the action 'of healthy organs."

1750. I observed that you passed over the whole of that enormous province of human suffering, perhaps the very largest class of all, nervous diseases. I think I understood you to say that your observations did not apply to them at all?—I said that the knowledge of the healthy functions of the human body, more especially in morbid states of the nervous system, was decidedly useful.

1751. So that there your statement as to the absence of any use of knowledge of the healthy functions did not apply?—Do not run away with the idea that I say that the knowledge of the healthy functions of the human body is not of use. I have said that it is.

1752. I wish to bring out the whole meaning of your statement to the members of the Commission, who may not be so familiar with the subject as you are?—I am very glad that that should be done.

1753. So that I think I may take it as your opinion, as much as my own, that anything like rational diagnosis, or any sort of conception of disorders to the nervous system, is entirely based on knowledge of the functions of that system?—No. The only one element of the diagnosis, and the only conception we can form of any one disease of the nervous system that can be based on our knowledge, of the healthy functions is, that it may reveal to us, in most cases, the seat and extent of the morbid state. Nothing more.

1754. Will you tell me any functions of the nervous system that have been ascertained except by experiments. Will you tell me whether the function of nerve in giving rise to the contraction of a muscle has been ascertained by experiments?—Entirely by experiments.

1755. Have not the functions of the sensory nerves been entirely determined by experiments?—Entirely by experiments.

1756. Have not the functions of the spinal cord been entirely determined by experiments?—Yes.

1757. Supposing you were called in to a case of paralysis, and found one side of the man paralysed, could you diagnose the amount of injury that had taken place, without knowledge based on experiments?—It would depend on my knowledge of the nervous system. Comparing the morbid phenomena of my patient with what I know of the healthy functions, I should be able to determine the seat and extent of the injury, not the nature of the injury; not the cause, nor the therapeutic indications. Other sciences would teach me regarding those points.

1758. Although it may be that experimental physiology up to this time has not actually suggested the precise remedies that you ought to employ in a case of disease, do you think that there is any more chance of understanding the nature of disease, unless you know the healthy state of the healthy action, than there is of knowing how to repair a watch if you do not know its structure?—We know the functions, I suppose, of the kidneys tolerably well, as well as any other parts; but my knowledge of the healthy functions of the kidneys will not enable me to diagnose the essential nature of one of the diseases to which the kidneys are liable. It will teach me that the kidneys are disordered, but will not tell me whether there is fatty degeneration.

1759. That is to say, the knowledge of structures has taken you a certain way, but not all the way?—I said just now, when I was speaking on that subject, that I believe myself that the knowledge of the healthy functions of the human body will enable us in many instances, but not all, to localise the seat of the disease, but not to diagnose the nature of that disease, or the cause of it.

1760. Let us take the case of the commonest of all morbid affections, inflammation; have we any knowledge of the minute nature of inflammation, I mean of the actual processes which go on in the tissues, which is the first step towards anything like a rational treatment, except from experimental inquiry?—All our knowledge of the first processes is due to experiments; but if you ask whether that knowledge has revealed any remedial agent, whereby those morbid states can be neutralised, I deny it. This question involves pathology, not physiology.

1761. But seeing that in all cases you cannot hope to deal with a thing rationally unless you know its nature, is it not to be anticipated that a full knowledge of the nature of a process will eventually lead to a rational mode of treating it?—This again regards pathology, not physiology. No doubt pathology sometimes points out, in the clearest way, the one efficient therapeutic indication, but not the therapeutic agent itself. It revealed to us the presence of the acarus scabies, but did not teach us that sulphur could destroy it. "A *full knowledge* of the nature of a process" would be highly important, but are unable to observe the consecutive series of organic changes that occur within our organism during the course of a disease. We talk of a cancerous diathesis, but cannot trace the organic changes ultimately generating a cancerous tumor in the breast.

1762. There is another very important set of diseases, take the case of ringworm for example; I presume that no rational treatment of that was adopted; that the theory of that disease was not understood until it was known that it was a parasite?—They used a good many efficient remedies for it before they knew it was a parasitic disease.

1763. It was sometimes treated with calomel, was it not?—Yes.

1764. Inasmuch as in a case like that the knowledge of the nature of the disease has led to its rational treatment, is it not to be expected that a knowledge, if we could obtain it, of the nature of, we will say, small-pox, may by and bye help us to treat it properly? —My answer to that question is this: if you could attain to the essential nature of what small-pox is, and the organic changes which take place in the human system during, we will say eight days, that amount of knowledge would be most important, but it is almost supernatural.

1765. Are you aware of the fact that within the last ten years such progress, as you are now regarding as supernatural, has been made in regard to sheep-pox, a perfectly analogous disease?—If by that question is meant that they have attained to a knowledge of the organic changes that take place in a sheep affected with sheep-pox, I know nothing about it.

1766. You are not aware then that investigation has shown that that disease is the work of a small organism?—I am perfectly aware of that. What I mean to say is, that the organic changes that that small organism produces in the animal are not known to us.

1767. But the first step to understand the nature of the disease, and therefore to get at a rational treatment of it, is to know what is the cause of the disease? —Etiology is no doubt very important; chiefly, however, to prevent the advent of disease. It seldom indicates a rational treatment of it. I refer you to what I have said about murch miasma, intermittent fever, and quinine.

1768. You think that purely empirical treatment is better than rational treatment, do you?—No; but I deny that the knowledge of the first initial stage of a disease gives you a direct clue to the right treatment of it.

1769. In the case of ringworm, I thought we agreed that knowledge of the disease has been a great help to right treatment of it?—Yes, it has. It indicates that the parasite must be destroyed, but does not teach us what agent could affect that. Sulphur was used long before it was known that scabies was due to the presence of a parasite. Parasitic disease, however, is not the best example to discuss. They involve no, or only slight, organic changes. Kill the parasite, and the disease is cured, just as a surgeon removes a splinter from the soft parts, and cures his patient.

1770. By the rule of thumb you mean?—Science has made as many blunders as anything else that the world has seen. Anything can be defended in the name of science. If you say that blunders have been committed by empiricism, I reply that as many blunders have been committed on the other side. I am not an empiric, although I verify everything by experiment.

1771. Do I rightly understand you to say, as among the measures which you propose, that the experimentor should send in two returns, one of experiments intended to be performed, and the other of the results of experiments performed?—Yes, I did suggest that.

1772. And your object was that somebody should be the judge whether these experiments were necessary, or not?—Yes, that was my object.

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1773. Has it occurred to you to consider who would be an appropriate somebody to undertake that office?—I am sorry that I am not in a position to reply to you in a satisfactory way. I will, however, suggest that probably no control would be efficient that has not the result of the united judgment and action of a board composed of physiologists, an equal number of physicians, surgeons, and pathologists; and even a chemist might sometimes give valuable suggestions.

1774. But still you incline to institute a sort of board of control for scientific experimentation?—Yes, on every kind of experiments on living animals.

1775. (*Mr. Erichsen.*) There are two or three points in your evidence which I should like cleared up. One is this: if I understand rightly, you seem to draw a distinction between pure physiologists or biologists and practical medical men?—A very great distinction.

1776. You mean, I suppose, that there is a class of men who are devoted now to the study of the structure of animals and the functions of their organs, quite irrespective of medical practice?—Yes.

1777. But it is not the business of those men in the study of the healthy organisms, to discover new methods of treatment, or to devise new plans of operation for instance; their business is simply to discover the actions of the bodies of animals in the healthy state?—And it would be very important if they kept to that, but my teachers always misled me for some years by saying that the knowledge of the healthy functions which they were teaching us was to form the basis of therapeutics, and of pathology.

1778. And is it the fact that among medical practitioners there are many men very distinguished in practice who never performed any experiments?—A great many.

1779. One of those gentlemen we have had before us, and in answer to a question of mine, he stated that he was greatly indebted in his practice to the experiments of the physiologists?—Did he specify what advantages he had gained from them, may I ask?

1780. He only stated it generally?—Then in my estimation it is worth nothing; we may quite agree or totally disagree. If, as Professor Huxley was endeavouring to establish, these were of use for localising disease, then he was right. If he went beyond that, in my estimation, he was thoroughly mistaken.

1781. There was another point on which you made a statement somewhat at variance with what we have formerly heard, that is with regard to the influence of experiments on therapeutics. There is a disease called angina pectoris, which has been beyond the reach of medical science, and a remedy for it has now been discovered called nitrite of amyl, and Dr. Brunton was led to the application of that drug to angina pectoris by experiments on living animals, and finding that nitrite of amyl produced dilatation of the arteries in them. That is a direct application of a therapeutic experiment to medical practice, is it not? —If the doctor found out from the action of that agent, that it was a remedy for angina pectoris simply from seeing that it dilated the arteries it was a happy guess.

1782. He was led to that inference by seeing the way in which it acted on the arteries of living animals? —We do not know what angina pectoris consists of, and therefore I do not see how he could have been rationally led to that conclusion. The tendency in the medical profession is, when any agent has an elective affinity for a particular organ to try it for several of the diseases of that organ. But I am far from wishing to depreciate the experiment. I believe the specific use of every drug can be known from its action on the healthy organs and tissues.

1783. Then there were a series of experiments performed by Majendie which have a very close bearing on this point. It was supposed before those experiments were made that an animal could live and thrive upon a simple substance such as gelatine, and Majendie, Mr. *A. de Noé Walker.*

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by direct therapeutic or diatetic experiments, found that the animal would starve under such a system, and that certain diseases were set up in consequence?—Yes.

1784. That I take it is, one may consider, the direct application of a therapeutic or rather a dietetic experiment to medical practice. Since that time we know that it is useless to keep a patient upon one kind of food, and that knowledge is based upon those experiments?—Yes. I was present when Majendie fed his turkeys for 40 days on albumen, and they all died of inanition. You must call that a diatetic, and not a therapeutic experiment.

1785. It is comparatively of late years that the application of experiments to pathology, I mean in the artificial induction of diseases, has sprung up?—Yes.

1786. So that it would be scarcely reasonable to expect that any great results should have been arrived at yet?—No, scarcely fair.

1787. But I suppose we may take it as a fact in pathology that we have come to a complete end of the ordinary naked eye dead house pathology?—Certainly.

1788. And that if we do advance in pathological knowledge for the future it must be through the means of the microscope on the one hand, and pathological experiments on the other?—Yes.

1789. Then there was another statement which you made; I do not know whether you gave it as your own opinion, but you said it was the opinion of another gentleman, Rivière, that the knowledge of the circulation of the blood had had no influence whatever upon medicine?—He said medicine, he ought to have said therapeutics, and I agree with him perfectly.

1790. Did he include surgery?—No; I do not think he could have meant to apply that to surgery at all.

1791. With regard to that experiment of trephining the skulls of six goats, it is something so entirely novel to me that I have a difficulty in knowing what could have been meant by such a procedure?—As far as I could make out, it was done by the experimentor partly to accustom his hand to the use of the instrument, and partly to see whether the goats would recover, and then to destroy them, and to see what nature had done in the process of healing.

1792. That experiment was done abroad, not in England?—Yes; the man is dead who performed it.

1793. You mentioned, amongst the various reasons why experiments were done, the one of acquiring manual dexterity. To your knowledge is that ever done in this country?—I am not aware of its ever having been done in this country. I think it is done in America; I may say that Gooch excised part of a rib of a dog to see whether he thought the corresponding operation could be performed on the human subject.

1794. That was not with the view of acquiring manual dexterity?—No.

1795. That, to your knowledge, is never done here?—No, never.

1796. (*Mr. Hutton.*) You were stating that your object in appointing a kind of inspector of experiments of this sort would be to see whether the experiments were in themselves useful; you did not at all mean to deny, I suppose, that useful experiments might be prohibited by even the wisest inspectors in those cases, but your object simply was to suggest that of the two evils, that of inflicting needless pain upon animals and that of losing a scientific discovery, you must choose the less, and that the less was, in your opinion, the loss of scientific knowledge?—I will explain further what I meant. Supposing I send in a list of experiments that I wish to perform, the persons I should appeal to should be in my estimation so competent, so full of knowledge of physiology that they would at once know whether my experiments were original ones or whether they were necessary to confirm experiments previously made. Then, supposing they were to confirm experiments previously made, it would be their duty to point out to me where the previous experiments had been defective, and to instruct me against a repetition of those imperfections. The persons I would appeal to would, so far from being obstructive to science, rather promote it.

1797. However, you would not deny that useful and competent experiments might be prohibited by that means; but you would say that so many useless and incompetent experiments would be prohibited that the balance of advantages would be greatly in favour of some such restrictions?—Certainly they would.

1798. (*Chairman.*) And I understood you to say that the probability was that if the experiment was really useful it would be approved by those to whom you appealed?—Yes; but they would use their discretion in more ways than one.

1799. And therefore that few cases of useful experiments would be likely to be excluded?—Yes.

1800. But that at present a very large number of entirely useless experiments involving great pain are performed by incompetent persons?—Yes; as I hope to prove on another occasion, if the Commission will permit me. I would add this: if I asked permission to institute a series of experiments, not only I say, would the competent authorities I appealed to, point out that the experiments had been done before (if that were the case), and that they were thoroughly unnecessary, but they might point out the deficiency of previous experiments of the same nature, and teach me how to avoid them; and I consider that would be quite in favour of science, and do away with a great number of ill-conducted and painful experiments.

1801. (*Mr. Hutton.*) I suppose you would go further and say that unless a specific object was contemplated the experiment should not be allowed; supposing, for instance, it was merely in the nature of an experiment to see what would happen. Unless you could see a specific scientific object in it such an experiment should be prohibited, you would say, if it were of a very painful kind?—Not only should it be prohibited if of a very painful kind, but every kind of experiment on living animals, if there was not a foregone object in the experiment, should be forbidden. What do you think of pouring half a pint of boiling water into the stomach of a dog, for instance? Or of injecting a quantity of sand into the veins of a dog. What foregone object was there in that?

1802. (*Mr. Huxley.*) Are we to take it as your opinion that eminent physiologists, whose time is valuable, perform such experiments with no object whatever, and for mere wantonness?—My reply is, that a certain number of experiments performed by them, I deem to be wanton cruelty.

1803. In the cases to which you refer did you take pains to inquire from the persons who performed the experiments whether they had any object in view, or not?—I only judged from what they told us during the course of the lecture. I will give you particulars on another occasion.

1804. And during the course of the lecture was an experiment performed and you not told that it had any object or purpose whatever?—Every experiment I refer to was not performed before the students at the lecture, but the experiments were referred to.

1805. And referred to for no purpose are we to understand you to say?—I must postpone my reply to all this to another day.

1806. I wish to be perfectly clear as to what your meaning is. As I understand the matter now it stands thus, that these persons introduced into their lectures for no discernible purpose whatever accounts of experiments which appear to have been excessively cruel; that is so, is it?—Yes; but I will more fully explain my meaning another time.

1807. (*Mr. Hutton.*) Do you mean for no scientific purpose, or for no demonstrative purpose?—For no demonstrative purpose.

1808. Had they no object in view?—I did not mean that; I will refer to those experiments another day.

1809. I did not quite understand what your view was as to demonstrative experiments. You said (I

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thought very justly) that no one could deny that the eye was a very much more powerful teacher than mere reading could be in such matters; but whether or not you justified experiments by that observation or condemned them, in spite of that observation, I could not quite understand?—What I meant was this, that it is really going against, I was about to say common sense (I say it with deference), to say that the eye was not intended to instruct the mind. If I see certain phenomena evoked in an animal in the theatre I am much more likely to be impressed with everything concerning those phenomena than if I had only read about them; but what I mean is that the knowledge of functions is so easily understood by oral instruction and reading, that I do not consider demonstrations to be either necessary or justifiable; and if it was otherwise, allow me to say that the greatest number of physicians and surgeons in practice would be nearly entirely ignorant of physiology, because since they attended lectures it has made tremendous strides; and since 1868 all the knowledge which I have of modern physiology is from reading, and I find no difficulty in understanding it. You have heard, for example, that there are two sets of roots coming out of the spinal cord. If any one member of this Commission, not having received a medical education, were pointed out on the slate that the spinal cord gave out two roots, and I upon the slate endeavoured to explain the different functions of these two roots, nothing could prevent any member of this Commission understanding thoroughly the functions of those roots.

1810. (*Mr. Forster.*) You stated that you did not think that there was as much of these experiments at home as abroad. Have you any opinion as to whether the experiments which are made in this country are generally made with anæsthetics, or not?—The question of anæsthetics is not so very simple. When an experimentor says, for example, as is said in a very recent publication, that "before and throughout these experiments anæsthetics were used," it is perfectly true; but if by that you choose to understand that while the animal lived and was experimented on, he was throughout insensible, it is the greatest delusion that ever was. I have written a paper upon that very subject. Then with regard to the question of the excess of cruelty in this country, I am bound to say that I am not so competent a judge of what goes on in this country, as I am of what I have seen in foreign laboratories. I chiefly judge from publications, where I see, however, a great excess in the number of the experiments. I would have suspended from rank and pay, as we say in the army, a certain physician at a certain hospital for vivisecting 16 cats, upon which he performed the same experiments, and obtained identical, or nearly identical, results. I do not consider him worthy to be an experimentor at all. I consider that a man who does that ought not to be licensed.

The witness withdrew.

Dr. Lawson Cape, M.D., called in and examined.

Dr. L. Cap
M.D.

1811. (*Chairman.*) You were formerly, I think, in practice in London, were you not?—I was.

1812. You have now retired from practice?—I have.

1813. Are you in communication with the Society for the Abolition of Vivisection?—I have seen Mr. Jesse; I am not a member of that society.

1814. Are you aware that you are desired by them to appear and state your views to the Commission?—No.

1815. Do you consider yourself identified in sentiment with them?—To a certain extent.

1816. You are desirous of stating to us your views on the subject?—I am prepared to do so.

1817. Will you be so good as to state your views?—My views on the point are, that practitioners now and for the last 20 years and more, surgeons and physicians, have taken very good care of the public health without ever having attended anything in the way of vivisection; in the medical schools where they were educated there was no such thing as vivisection.

1818. And you think that the recent introduction of that practice has not been called for by any necessity existing on the part of the public?—Certainly it has not.

1819. Are you of opinion that any legislative measures are called for by the present disposition to extend that practice?—I should deprecate any extension of the practice.

1820. Do you think that it was in former times carried too far?—I have never seen it in England or in Scotland; I was educated at St. Bartholomew's Hospital, and at Edinburgh, and lectured for ten years at St. Thomas's Hospital, and no such thing was heard of as vivisection. At the time I was in Paris for three winters and two summers, they had vivisection, but it was not recognised by the *École de Médicine* or by the authorities at all; it was done on the sly, and they had dogs there for the torsion of arteries and so on.

1821. Is it your opinion that any legislative measures should prohibit experiments altogether, or that they should tolerate it and regulate it?—I do not see how the public would be benefited, as far as medical advance or surgical advance goes, by the practice of vivisection in the medical schools.

1822. Would you allow it anywhere?—It would depend upon the animals. I do not think that in the case of highly organized animals you would be justified in performing vivisection upon them.

1823. Would you allow it in any case?—Well, it must be a strong case.

1824. But suppose it were what you would consider a strong case, would you think it justifiable then?—I could not answer that question unless I had a strong case put before me.

1825. You are not prepared yourself to mention to the Commission any cases in which you would think it justifiable?—No, I am not.

1826. You are not therefore prepared to recommend any set of provisions like that contemplated by Dr. Playfair, by which the Secretary of State should license certain persons to perform such experiments?—I think that license ought to be guarded by certain regulations.

1827. But you would contemplate such a system if it were properly guarded?—If it were properly guarded.

1828. Are you prepared to suggest to the Commission anything that you would consider to be proper security?—Whatever physiological question may have to be decided, if it could be done by experiments upon the lower animals, I should say that that would be preferable to experiments on the higher ones; I mean the more highly organized ones, fully organized ones, like the dog.

1829. By "lower animals" what do you mean?—There are all the phases of animal life below those to which I have referred, and the lower they are the less the nervous system is developed, and the less the pain. I do not think there is much pain, for instance, in a worm to fish with. It has no brain, and I do not think that it suffers, although the popular notion is that it does.

1830. Then you are not prepared to suggest any detailed scheme by which the Commission could propose to regulate a system of experiments on living animals?—I have not had the advantage of reading Dr. Playfair's suggestions about that.

1831. They would empower the Secretary of State to license certain persons to conduct experiments?—I think that is the way it ought to be done.

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1832. Would you be prepared to recommend that plan for adoption, or would you be prepared to say with the Society for Abolition, that there should be no experiments allowed at all?—No, I should not be prepared to go so far as that.

1833. (*Sir J. B. Karslake.*) May I ask how many years you have been out of practice?—Eight or ten.

1834. I suppose you have not attended to the subject very much since that time?—Well, I may say that I have not attended to it since I was at a medical school.

1835. As I understand, you have never seen any instance of vivisection in England?—Not in England.

1836. (*Mr. Erichsen.*) You said you thought that vivisection was of no utility, so far as the education of medical practitioners was concerned?—As a part of medical education in a medical school I meant.

1837. Are you aware that the physiological laboratories in medical schools have been established, not by the medical schools themselves, but under the direction of the examining and licensing bodies, and of the General Medical Council of Education?—That has not been in my time.

1838. They have been established since your time?—Since my time. I lectured at St. Thomas's from 1837 to 1847, and since that the schools are almost a blank to me.

1839. You have no practical knowledge of the schools since the year 1847, I understand you to say?—Not since the year 1847.

The witness withdrew.

Mr. G. Macilwain, F.R.C.S.

Mr. George Macilwain, F.R.C.S., called in and examined.

1840. (*Chairman.*) You were formerly in practice in London, I think?—Yes.

1841. You have retired from practice for some time, have you not?—Yes, for three or four years; rather more.

1842–3. Have you been in communication on this subject with the Royal Society for the Prevention of Cruelty to Animals?—Yes.

1844. And with the Society for the Abolition of Vivisection?—Yes; I have had communications from them.

1845–6. Will you have the kindness to tell us what is the nature of your acquaintance with the subject; have you yourself ever practised any vivisection?—Yes, I have, in early life. Early in life I was very much associated with Mr. Abernethy, and consequently with the writings of John Hunter; and my studies were based upon what I had read of John Hunter, what I had seen of Mr. Abernethy, and what I had tested by the writings of Lord Bacon; and if anything occurred to me which it appeared to me was not reconcilable with their precepts, I strongly suspected it, and was induced therefore again to sift it with regard to the conclusions which I might have been more or less inclined to draw from it. The result of that was that in about 1836 or 1838 it appeared to me that the best thing that I could do would be to endeavour to inaugurate a more logical and inductive inquiry into medical science. It appeared to me that the state of medical science depended upon the imperfect manner in which it had been investigated; and of course that directed me especially to anything which might be, or which might be conceived to be, a fallacy in its investigation. Amongst these fallacies, and I wish particularly to put it in this way, because I am no anti-cruelty man, or anything of that kind; that is no part of my business. I merely looked at the fallacies in common, and it appeared to me that there were very few greater or in more direct violation of the precepts of inductive philosophy or logical reasoning than the dissection of living animals. I could trace nothing that I thought useful to it. I heard then, as I have heard since, that many things were discovered by vivisection; I can only say that it is not the fact. For instance, I find that the discovery of the circulation of the blood is referred to vivisection. In the first place, any man who knows what the circulation is will see that intrinsically that could not be; you do not want the authority which is suggested to you, because you could not discover the circulation in the living body; I do not see how it is possible to do it. If you had a dead body, then it is so easy to discover the circulation, that it is difficult to understand how it was not done before; because if you inject by the arteries you find that it is returned by the veins. Harvey was a pupil of Fabricius, of Aqua Pendente, and Fabricius discovered the valves in the superficial veins. Of course the blood can only move in one direction; but Fabricius did not see that; Harvey did; and that is the real seed of his discovery. But you see it said every day, and I see medical men say, it was from vivisection, from experiments at least on living animals. Then there is another thing, I would say, I see it stated that John Hunter made experiments on animals. Undoubtedly he did. Now it so happens that I have brought a book here, which I published in 1838, which was the first volume of my work on "Medicine and Surgery—1. Inductive Science." I did not go beyond the first volume, because I found that though it was recommended creditably by the reviews to the "scientific few," the scientific few neither paid me for the expenses of that book, nor for my loss of time. I therefore could not go on with it; it would have been too expensive to me to publish the four volumes. But I observe that in that book I state my objections against physiological investigations, by means of dissecting living animals. Now that book is still in existence. It has been very much complimented by the Americans. One of the first professors in America offered to print it again, at his own expense, and I think that I may with confidence state that fact to this Commission, and put it in their power to put that book before any medical men, or any set of medical men now existing, to see if they can really overthrow one single position that I have taken. Now, as of course my object was truth, I take, not every man who thinks it necessary to cut an animal and say, "You see that that nerve makes that muscle move," or anything of that kind, but I take John Hunter himself. And what did John Hunter do? There is no doubt that he made experiments on animals, but I have it in that book that there was not a single thing that he discovered or did, or a single conclusion that he drew from experiments on animals, that might not be much more clearly proved by the ordinary practice of surgeons. I will give you an instance (and this is a kind of *ex pede Herculem* case) with regard to one class of vivisection. It had been thought that when matter was formed, what we called suppuration, there was a destruction of the part; that is to say, there had been a foregone conclusion without investigation, that the nature of the suppuration was what I have stated. It was John Hunter's object to overthrow that, and therefore he made experiments on animals of this kind. He injected irritating fluids into contact with the mucous membrane, that is the lining of some of the canals of the body, and of course he produced matter; but then why did he not look at the disease which we call empyema? Because there he might have seen matter formed in the chest without any dissolution of solids. Another point in regard to John Hunter is this: they say that his discovery, as they call it, for the relief of aneurism was arrived at by experiments on living animals. Now that is also entirely untrue; but with it there is just that mixture of truth which leads people, who will not study their profession, and who are not accustomed to strict study, to draw wrong conclusions. Now I dare say you all perfectly well know what an aneurism is, but in giving evidence it is important not to suppose anything. An aneurism

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therefore is a giving way of the internal coat of an artery. There is another form which consists in the enlargement of the artery; but there is an aneurism, which is the ordinary case, which consists in the giving way of the internal coat of the vessel. The blood becomes then propelled against the yielding coat; the blood forms a pulsating tumour, and that is what we call an aneurism. Up to that time I must tell you that the operation on that disease was a very formidable operation, and too frequently fatal. It consisted in opening the sack and tying the artery on both sides of it. This was found to be a very bad operation, frequently attended with fatal consequences. I need not detain you with telling you the process. Now Mr. Hunter said that the cause of that was that the artery was tied in a spot where it was diseased, and that if he tied the artery in a sound part he would most likely find that the thing would do very well. He accordingly did so, and the operation proved successful; and that has been certainly a very desirable and excellent improvement in the practice of surgery. But there was not a single thing with regard to it that he could have discovered in a living animal. Now the thing which has probably caused some unthinking persons to infer that is this. There was a great contest at the time. They said that Mr. Hunter is wrong, and that the arteries were generally diseased. Then Mr. Hunter made an experiment on an animal, that is to say, he tried to make an aneurism. He bared an artery, and he dissected off the coats of the artery, only leaving the internal coat, so as to make it as weak as he could, and then he bound up the wound; but after a time he killed the animal, opened the wound, and found that everything had healed, just as if nothing had been the matter. In fact, he could not make an aneurism, and as animals do not have aneurisms, but only the human subject, it is quite clear that there is not a shadow of shade of evidence that his discovery was the result of experiments on animals.

1847. Your opinion, I gather, is that experiments on living animals do not conduce to the cure of infirmities, whether surgical or medical, in the human frame?—I have a most matured conviction of that.

1848. Is all this set forth at length in the volume that you have been so good as to bring to our notice?—No; not at length.

1849. But I mean at as much length as would be possible for us to take in examination?—Yes; I should think it would.

1850. Have you in any way changed your opinions, or seen any reason to modify them since?—Not the least; on the contrary, everything which I have seen since has convinced me, not merely that physiological investigation by means of dissecting living animals is an inauspicious mode of proceeding, because it violates the first principles of inductive philosophy, as also of logic, but that it actually has led to most serious practical errors. I know of no error in the whole practice of surgery (and now I am obliged to mention myself, which is not a very pleasant thing) which has produced an evil equal to that which I myself have practically corrected, and that is the employment of purgatives after the operation in strangulated hernia.

1851. Has that mistake resulted from experiments on living animals?—Yes; and now I wish to show you that. I must tell you that strangulated hernia was, and is now, a very dangerous disease. The intestine escapes from its natural cavity. It is constricted by the tendinous structure through which it escapes, and the consequence is that the case is a very uncertain one; I have seen a patient escape after days, and I have seen mortification take place within an hour or two; so that it is a case of great gravity. The operation consists of cutting down to the part and enlarging the opening of the tendinous structure and replacing it. Now, of course, it is a very natural anxiety for a man to know whether the bowels have acted, because that is a direct proof that the stricture has been removed, but that led to a most grievous mistake. And now I must go back to the experiments. Mr. Travers made experiments on dogs. His book is entitled "On Injuries of the Intestines and on Strangulated Hernia." He made some experiments on animals, and divided the intestines and sewed up the wound and guts, and did a great many things of that kind, and showed that animals have certainly very great powers of repair under such circumstances. But now the inductive philosophy comes in. He left out some of the most important parts of the subject as regarded the human subject because he never purged these dogs. If he had wanted to carry the analogy close for strangulated hernia he should have placed the dog (even supposing it was feasible to do anything with the dog at all), as nearly as possible under the same circumstances as the human subject. But now mark the difference, and just mark the consequence. He does no such thing; but he goes on to the treatment of strangulated hernia, and he says that after the operation the great thing is to get a discharge from the bowels (now this is true); and the great danger is from the inflammation of the peritonœum, that is the membrane covering the bowels, and lining the interior of the body; and he says that purgatives are the great thing; that if there is no peritonitis, we use purges to prevent it, and if there is peritonitis, then we use purges to cure it. Now you must allow me just to refer to that passage, because this is a matter of extreme consequence. I shall not exaggerate if I tell you that a difference in practice saves in Europe, I should say hundreds of lives every year; and I will show you exactly how it was arrived at, because I did it myself, although it is very difficult to claim anything for oneself. In the same book in which Mr. Travers published these experiments he also published the treatment of strangulated hernia; he gave his directions for the treatment which is exactly that which destroys the patient.

1852. Without putting you to the trouble of reading the quotation, we understand you to say that you mention the case of Mr. Travers's experiments upon animals in regard to strangulated hernia as a proof that such experiments may not only not lead in the right direction, but may be absolutely misleading?—Yes; if you add to that "and the practice he deduced from them." At present you have only my word for this fact. But now here comes a very extraordinary circumstance. Here is the transcript of a lecture, the part I refer to being only a few lines, from a gentleman whose mistake induced me first to write upon that subject, and which has been the means of my having any power of claiming that improvement as my own. I could not get him to refrain from giving purgatives, and the patient died. That same gentleman, Mr. Stanley, of St. Bartholomew's Hospital, in lecturing to his pupils some 20 years afterwards, or more than that, says as follows: That at one time purgatives were employed in these cases, whereas it is now perfectly understood that they ought not to be so employed, and he had himself had bushels from cases of strangulated hernia—cases where the fatal peritonitis was traceable to the purge.

1853. (*Sir J. B. Karslake.*) You practised vivisection many years ago yourself, I understand you to say?—I did a little, but that was very early indeed.

1854. Your view is that vivisection is wholly useless, and worse than useless?—It is; and I beg leave to say this, that I have on several occasions offered in print to take any number of experiments which the profession were desirous of putting in force, or in which they had confidence, with the desire to give them my most respectful consideration.

1855. I understood you to say that if you could be induced to believe that vivisection was absolutely necessary you would not hesitate to practice it merely on the ground of the cruelty which is inflicted on animals?—I do not say so.

1856. You said it was not a question of cruelty, did you not?—I said it had not been with me; but I began like other people making great mistakes, and I have endeavoured to correct them. I wish not to

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encumber the real facts of the case by anything touching on the moral question.

1857. You wish to confine your evidence to this, that in your judgment vivisection is wholly unnecessary?—But please just let me put it in my own way: I only repudiate vivisection as one of the fallacies in medical investigation; I wish to make that distinction, because it mars the evidence to mix it up with other motives. I hope I do not yield to any man living in what I feel as to what is due to animals, and to the high nature of investigations, but I do not wish such considerations to be mixed up with the facts. I would rather it was supposed that I was wonderfully cruel than I would mar the evidence which I believe stands as evidence which is irrefragable.

1858. (*Chairman.*) What I understand you to say is, that you would not tolerate and regulate vivisection, but you would abolish it altogether?—If you are asking me that as a scientific man, I say most decidedly so; but as a fallacy, it is no business of mine to talk about cruelty. I wish you particularly to understand that; I do not wish to judge anybody; I have got my own feelings on that subject it is true, but it is as a fallacy that I condemn it. I think it is a demonstrable fallacy. I do not even admit that up to the present time it is matter of opinion. I believe I have offered two or three times of late years (I am not strong enough now to do it,) to bring the whole subject before the professional public in a course of lectures, provided only that anybody would pay for the room and the lights. When I say that that is a work of enormous labour, because it involves at all events a slight sketch extending over a couple of thousand years, you may suppose that I am in earnest about the scientific part of it.

1859. And you consider that, so far as the practice of vivisection has been used heretofore, it has not been leading in the right direction, but that it has been misleading the profession?—Most decidedly, I think that is a demonstrable thing; and I think the Americans agree with me, for the book which I have mentioned to you was re-published in America; and in fact after that I exemplified my mode of investigation, or rather I should say Lord Bacon's mode of investigation, by publishing that which was not much read in this country, but the Americans immediately reprinted it.

The witness withdrew.

Adjourned to to-morrow at 2 o'clock.

Thursday, 21st October 1875.

PRESENT:

THE RIGHT HON. VISCOUNT CARDWELL, IN THE CHAIR.

The Right Hon. LORD WINMARLEIGH.
The Right Hon. W. E. FORSTER, M.P.
Sir J. B. KARSLAKE, M.P.
THOMAS HENRY HUXLEY, Esq.
JOHN ERIC ERICHSEN, Esq.
RICHARD HOLT HUTTON, Esq.

N. BAKER, Esq., Secretary.

Rev. S. Haughton, M.D.

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The Rev. SAMUEL HAUGHTON, M.D., called in and examined.

1860. (*Chairman.*) You are a Fellow of Trinity College, Dublin?—Yes.

1861. You are the Medical Registrar of the School of Physic of Trinity College?—Of the School of Physic in the University.

1862. I think that is the largest school in Dublin?—The largest in Ireland.

1863. You are in Holy Orders?—In Priest's Orders.

1864. But you are also Doctor of Medicine?—Yes, I have had a medical education, and am a Doctor of Medicine and a Fellow of the College of Physicians and of the College of Surgeons of Ireland.

1865. You are also, I think, a Doctor of Civil Law of Oxford, and a Fellow of the Royal Society?—Yes.

1866. I think you are honorary secretary of the Royal Zoological Society of Ireland?—Yes.

1867. You have taken an interest I think in the subject which has been referred to us, and I think you had some correspondence with the Royal Society for the Prevention of Cruelty to Animals upon the subject?—Yes; I have taken for many years an active interest in the subject of vivisection, sometimes, in cases where I thought it proper, as a strong advocate for it and a defender of it, and sometimes, where I thought the experiments improper, as an opponent of particular classes of vivisections. If your Lordship will allow me, I think a correspondence that I had at the beginning of this year with Mr. Colam, the Secretary of the Royal Society for the Prevention of Cruelty to Animals, will put my position and views more clearly before you. The document in my hand, I presume, has been before your Lordship already. This was a memorial sent to the Society for the Prevention of Cruelty to Animals, praying them to take certain steps, amongst others to procure parliamentary legislation, and it was this memorial accepted by the Royal Society for the Prevention of Cruelty to Animals that led, I believe, in the first instance to Lord Henniker's bill being brought forward, and afterwards to Dr. Playfair's. It is signed by some of the most distinguished medical authorities in Dublin, and it is signed by myself, and in consequence of my signing that Mr. Colam wrote me a letter which I will read. That memorial is as follows:—
"The practice of vivisection has received of recent
"years enormous extension. Instead of an occasional
"experiment, made by a man of high scientific
"attainments, to determine some important problem
"of physiology, or to test the feasibility of a new
"surgical operation, it has now become the everyday
"exercise of hundreds of physiologists and young
"students of physiology throughout Europe and
"America. In the latter country, lecturers in most
"of the schools employ living animals instead of
"dead for ordinary illustrations; and in Italy one
"physiologist alone has for some years past ex-
"perimented on more than 800 dogs annually. A
"recent correspondence in 'The Spectator' shows
"that many English physiologists contemplate the
"indefinite multiplication of such vivisections; some
"(as Dr. Pye Smith) defending them as illustrations
"of lectures, and some (as Mr. Ray Lankester)
"frankly avowing that one experiment must lead
"to another *ad infinitum*. Every real or supposed
"discovery of one physiologist immediately causes
"the repetition of his experiments by scores of
"students. The most numerous and important of
"these researches being connected with the nervous
"system, the use of complete anæsthetics is practically
"prohibited. Even when employed during an opera-

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"tion the effect of an anæsthetic, of course, shortly ceases, and for the completion of the experiment the animal is left to suffer the pain of the laceration to which it has been subjected. Another class of experiments consists in superinducing some special disease, such as alcoholism (tried by M. Magnan on dogs at Norwich) and the peculiar malady arising from eating diseased pork (trichiniasis), superinduced on a number of rabbits in Germany by Dr. Virchow. How far public opinion is becoming deadened to these practices is proved by the frequent recurrence in the newspapers of paragraphs simply alluding to them as matters of scientific interest involving no moral question whatever. One such recently appeared in a highly respectable Review, detailing a French physiologist's efforts, first to drench the veins of dogs with alcohol, and then to produce spontaneous combustion. Such experiments as these, it is needless to remark, cannot be justified as endeavours to mitigate the sufferings of humanity, and are rather to be characterized as gratifications of the 'dilettanteism of discovery.' The recent trial at Norwich has established the fact that, in a public medical congress, and sanctioned by a majority of the members, an experiment was tried which has since been formally pronounced by two of the most eminent surgeons in the kingdom to have been 'cruel and unnecessary.' We have, therefore, too much reason to fear that in laboratories less exposed to the public, and among inconsiderate young students, very much greater abuses take place which call for repression. It is earnestly urged by your memorialists that the great and influential Royal Society for the Prevention of Cruelty to Animals may see fit to undertake the task (which appears strictly to fall within its province) of placing suitable restrictions on this rapidly increasing evil. The vast benefit to the cause of humanity which the Society has in the past half century effected, would, in our humble estimation, remain altogether one-sided and incomplete, if, while brutal carters and ignorant costermongers are brought to punishment for maltreating the animals under their charge, learned and refined gentlemen should be left unquestioned to inflict far more exquisite pain upon still more sensitive creatures, as if the mere allegation of a scientific purpose removed them above all legal or moral responsibility. We therefore beg respectfully to urge on the committee the immediate adoption of such measures as may approve themselves to their judgment as most suitable to promote the end in view, namely, the restriction of vivisection; and we trust that it may not be left to others, who possess neither the wealth nor organization of the Royal Society for the Prevention of Cruelty to Animals, to make such efforts in the same direction as might prove to be in their power. If the committee do us the honour to ask our opinion as to how such a work can be carried on, we beg respectfully to indicate some of the ways which we have had in contemplation in drawing up this memorial. 1st. By the appointment of a sub-committee expressly to deal with the subject, to be called 'the Sub-Committee for the Restriction of Vivisection.' 2nd. By instructing Mr. Colam to undertake as many prosecutions of cases of vivisection, involving severe animal suffering, as may prove to come within the scope of the existing law, and so to bring the matter prominently before the public eye. 3rd. Should it be found that grounds for such prosecutions are rarely to be obtained, owing to the secrecy with which vivisections are usually performed, or that, when undertaken, the state of the law renders them ineffectual, it may then be considered whether a bill should not be introduced into Parliament making the publication of any cruel experiment in a scientific journal or other work a legal ground for the prosecution of the publisher, and thus throw upon the operator the onus of justifying the act. In view of the ambition for scientific notoriety, which may be deemed a not insignificant motive for the performance of many of these experiments, it is believed that this provision would be eminently effective. 4th. If a bill on the subject were found advisable, it might properly contain other provisions, such as,—1st, the prohibition of all painful experiments on animals, except in authorised laboratories, and by registered persons, whose experiments should also be registered as to number, nature, and purpose; 2nd, the absolute prohibition of painful experiments as illustrations of lectures; and, 3rd, the extension to three months of the interval of a single month now allowed by law for the prosecution of cruelty to animals after the commission of the offence. All the provisions of such an Act would of course be carefully weighed by Parliament in debate; and while physiologists would contend for such liberty as they might be enabled to justify to the conscience of the nation, the society would endeavour to obtain security against its abuse." On the 25th February 1875, the following letter was written by Mr. Colam to me. "Sir, on the 25th ult. a memorial was presented to this society, of which the enclosed is a copy, and to which your signature was affixed, as I am informed. A special committee has been engaged since that period in collecting information, and in investigating the allegations of the memorialists relating to the practice of vivisection. They now instruct me to beg of you the great favour of placing before them the data upon which such allegations were made in your memorial, in order that before preparing legislative measures, they may be in possession of ample details should evidence be called for by the Houses of Parliament. Recitals of painful experiments on living animals published in medical journals or in medical works, or references to these, will be invaluable." I declined co-operating in providing evidence. I did not want to be made a detective, for the reasons I now give. My answer was this:—"Trinity College, Dublin, 10th March 1875. Sir, I received your letter of 25th ult., and do not wish my position with regard to the question of vivisection, and the more serious one of pathological experiment to be misunderstood. Both practices in the hands of skilful observers have conferred invaluable benefits on the sciences of medicine and physiology, and I must be distinctly understood to object to their abuse only. The practice of vivisection in physiological laboratories is notorious, but difficult to prove, as even those who object to the abuse of the practice (like myself) will not come forward to give evidence against scientific brethren, who are not so sensitive as to the infliction of pain upon the persons of other animals or men, although very sensitive to pain affecting their own persons. The enclosed prospectus will give you a very significant hint as to the practice of vivisections in physiological laboratories. The University of Dublin, in authorising a practical course of histology and physiology, has expressly prohibited vivisection, and will not recognise the certificates of a school in which it is practised for the purpose of illustrating class lectures, or in which students are permitted to practice it. The true use of vivisection and pathological experiment is for the purpose of original research, and they should be employed most cautiously and with a due sense of responsibility."

I now propose to put in the regulation of the University which is referred to in that letter. It is Regulation No. 40. It is resolved that a three months course of practical instruction be given in physiology and histology, placed under the care of the King's Professors of the Institutes of Medicine, and money is provided for that purpose by the order of the Board, and a note added to the order, "vivisections are strictly prohibited."

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1868. Now are those regulations concurred in by the College of Physicians in Dublin ?—Perhaps your Lordship will allow me to explain briefly the nature of the constitution of the School of Physic in the University. The University of Dublin was from the time of Elizabeth a very clerical body, and great difficulties were experienced in forming a School of Physic in it. It was therefore at a very early period associated with the College of Physicians founded by Charles II. There were a number of Acts of Parliament, called School of Physic Acts, passed during the last century, but they are all summed up in one School of Physic Act, which was the last Act but one passed by the Irish Parliament in the year 1800, namely, the 40th of George III., chapter 84, which repeals all preceding Acts, except as to some purposes, and in the 30th of Victoria, chapter 9, called the Amendment of the School of Physic (Ireland) Act. By those two Acts, summing up numbers of Acts that preceded them, the School of Physic is under the joint control of the University of Dublin and the College of Physicians; and the regulations made by either College affecting their professors and part of the school must be submitted under these Acts of Parliament to the other college. They do not require the approval of the other college, but the other college has a veto, and can challenge a visitation, which must be decided by the visitors of the college making the alteration. That has occurred sometimes, and each college in fact watches the other, and when regulations like this are passed, they are laid before the other college, and silence gives consent; if the other college takes no notice, it approves.

1869. Then, in short, the regulation which you have just put in may be taken by us to express the views of the College of Physicians in Ireland, as well as of the University of Dublin ?—Certainly; and I may add what I think much more important than the opinion of any College, the overwhelming opinion of the educated public in Ireland, who are extremely sensitive on this subject,—I mean vivisection for the purpose of teaching classes.

1870. You have mentioned also that this course of lectures is under the direction of a professor ?—Yes, Professor Purser, King's Professor of the Institutes of Medicine.

1871. Are you authorised to state anything on his behalf ?—I state from my own knowledge, as Medical Registrar, and I am authorised by him to state on his authority that practically he feels no difficulty in conducting his instruction in practical physiology and histology subject to this restriction, although individually he would prefer not having the restriction imposed on him.

1872. It is your opinion then that in regard to teaching there ought to be a total prohibition of vivisection ?—Yes, I would advocate that.

1873. Then, with regard to scientific research, what is the opinion which you wish to state ?—With regard to scientific research, it is a more difficult question. The use of vivisection in scientific research cannot be prohibited, but I would have it supervised, and I would use as an analogy for its supervision the corresponding case of human dissections. Before the year 1834, when King William's Anatomy Act was passed, anyone might hire a room and dissect a corpse, and he might, as was done in Edinburgh, receive that corpse warm, and not ask too many questions as to where it came from, which led to the hanging of Burke. When that Act was passed, all the evils of body snatching ceased at once, and I believe that now human dissection goes on quite quietly without causing any shock or any offence. I believe that public opinion on the question of vivisection of animals has now reached such a point that we require a supervision of it similar to that which the Anatomy Act produced in 1834.

1874. Then, with regard to teaching, you would prohibit vivisection altogether, and with regard to scientific research, you would tolerate, but regulate it ?—Yes; and I would give the following four reasons for advocating its supervision. If I could trust the conscientiousness and the common sense, and the *savoir faire* of the physiologists engaged in original research, I would leave it in their hands, but I find I cannot do so for the following reasons :—I believe that a large proportion of the experiments now performed upon animals in England, Scotland, and Ireland are unnecessary and clumsy repetitions of well-known results; that young physiologists in England learn German, and read experiments in German journals, and repeat them then in this country. There is a good deal of that second rate sort of physiological practice going on. All that, I think, requires control. The second reason I would give is that in many experiments which ought to be made, the mode of experimenting is unnecessarily cruel. And I would illustrate that by the case of the experiments made at Norwich in 1874 by Monsieur Magnan, of Paris. The object of the experiments was to show the effects of alcohol and absinthe, I presume, ultimately upon man when taken by the stomach, as we are in the habit of doing. The secretary of the department gave rather a hasty consent to having the experiment made, without much inquiry, and a large number of the medical men who were present at the Medical Association in Norwich, when they heard that these experiments were going on, protested against them. Although there was a majority of persons in the room where the experiments were performed in favour of the experiments, I believe that an overwhelming majority of the members of the association would have prohibited them if they had had an opportunity of expressing an opinion. The experimenting was unnecessarily cruel in its mode, because their object was to ascertain the effects of absinthe and alcohol when taken into the human stomach. If you did not choose to employ a man for the purpose (which, in my opinion, would have been quite allowable and easily done), you should then have employed dogs, and tried the experiment by means of the stomach; you should have given them the medicine by means of the stomach or rectum. Instead of doing that, they opened the femoral vein, and injected alcohol into one dog, and absinthe into another. Experiments of that kind I certainly give my hearty opposition to, and would always do so, because I believe that, though the experiment was an important one to do, it was done with unnecessary cruelty.

1875. (*Sir J. B. Karslake.*) Were the dogs placed under chloroform ?—No, the dogs could not be placed under chloroform for the purposes of such an experiment. Your object in such an experiment is to ascertain the effects of two powerful alkaloids, and the dogs could not have been under anæsthesia for such an experiment; which was an additional reason for giving it to them by the stomach, or employing men. I see by the newspapers that there are upwards of 2,000 citizens of Norwich whose votes can be purchased. I would undertake to purchase a score of them to come and get drunk on alcohol or absinthe if you wanted them. My third reason for supervision is that many of the experiments are undertaken without due deliberation and previous forethought. Perhaps it is scarcely necessary to illustrate that an experiment may be devised without sufficient care, and something that might have been foreseen occurs in the course of the experiment, which renders the whole result *nil*, and all that suffering might have been spared. Physiological experimenters would be greatly aided by some supervision, some kind of committee that would weigh the conditions of the experiments, and see that unnecessary pain was not inflicted. The last and perhaps in some respects the most important reason for supervision, is what I consider the exaggeration of the importance of many of the results. An investigator naturally exaggerates his own hobby, and considers the results more important than the rest of the scientific world will. Now the example I would give of that is the uncertainty of the results of many experiments made on animals being extended to men. The late Doctor Hughes Bennett made one of the most extensive and

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interesting series of experiments that ever were made on this very subject of the action of mercury on the liver of dogs, and he arrived at certain conclusions which must of course be accepted by medical men as ultimate facts to go upon if we were called upon to treat sick dogs, but they do not apply to sick men at all. Notwithstanding the publication of these results, there is not a medical man in all Europe or America who has altered his practice in regard to giving blue pill in cases of liver congestion. It is a very interesting illustration of the fact that you cannot argue by analogy from one animal to another. I would give as an illustration of that a case I am familiar with, as the experiments were made by myself. At the time of the Palmer murder attention was directed to the properties of strychnia, and a murder occurring at the same time in Belgium, when Comte de Bocarme, with the connivance, if not with the aid of his wife, murdered her brother with nicotine, led to a number of experiments being made by myself on strychnia and nicotine; and I discovered by experimenting on frogs that I could make a bath of nicotine which would kill a frog in a minute, and another bath of strychnine which would kill another frog in four minutes; but that taking a mixture of nicotine and strychnine, mixed in certain proportions, I produced a bath which the frog could live in for weeks. Those experiments were published by the Royal Irish Academy, and the first application made of them was by a young man who was present at the reading of the paper, who was clinical clerk in the Meath Hospital. He went out and became a practitioner in America, and he had the good fortune to save a sailor's life at Saint Louis, Cincinnati, who had taken a large dose of strychnia in a glass of beer, and whom he cured afterwards by tobacco. Since that, five or six similar cases of cure of poisoning by strychnia have occurred. I went on and extended the experiments to dogs, and was greatly struck with the result; I got no such result in dogs at all. That shows how uncertain *à priori* judgments are in reasoning from analogy. I found in that case that the action of strychnia and nicotine upon frogs was nearer to its action upon men than its action upon dogs was, which is contrary to what you would *à priori* suppose.

1876. (*Chairman.*) I understand you then to object entirely to the use of living animals for the purposes of instruction?—Yes.

1877. Whether under anæsthesia or not?—Yes.

1878. I understand that you would tolerate but regulate the same proceeding in reference to original scientific research?—Following the principle of the Anatomy Act of 1834, because that is an important guide to us in this whole question.

1879. Now have you seen the two bills that were in the last session before the two Houses of Parliament?—I have.

1880. Do you approve of either of those bills, or are you prepared to suggest anything yourself as an improvement upon them?—I do not entirely approve of either of the bills. There is a defect in Lord Henniker's bill, in clause seven, in his definition of vivisection. It is confined to cutting or wounding either with knives or galvanism any living vertebrate animal, or producing in any living vertebrate animal a painful disease. That leaves out of the question the case of the administration of poisons or narcotics, because they can hardly be said to produce diseases. It is a defect in the definition of vivisection. And then I object, in clause four, to the principle of charging 10*l.* for a license for a person to conduct experiments, because the Almighty does not always give brains to men who have 10*l.* in their pocket. I would have the poorest man in the country, if the Almighty had endowed him with the gift of research to advantage, at liberty to use it. And I would leave out details such as that bill goes into about urari, as to which it is very doubtful whether the physiology of them is correct or not; and I would secure such a supervision as would arrange details of that kind.

1881. You would leave the details to the judgment of the supervising body?—Yes. I would charge no fee for vivisecting, and I would include more in "vivisecting;" I make the range of it very general. Those are the only objections that I have to Lord Henniker's bill. Dr. Lyon Playfair's bill I have not considered so carefully; but I have noticed two things that I object to in it. I object to clause four, authorising "the President of the Royal Society, the "Presidents of the Royal Colleges of Surgeons in "London, Edinburgh, or Dublin, the Presidents of "the Colleges of Physicians in London, Edinburgh, "or Dublin, and also a professor of physiology, "medicine, or anatomy in some university in Great "Britain, or recognised by the colleges of surgeons "and physicians aforesaid" to give certificates to a person wanting to vivisect. I have no confidence in those gentlemen at all; they would give their permission or prohibit it according to individual opinions. That would be no control, but would be utterly insufficient. And I am entirely opposed to clause eight, "A license under this Act shall extend to any "person assisting the holder of the license, provided "the person assisting acts in the presence and under "the directions of such holder." That would open the door to vivisection performed by young men who are utterly unfit for the purpose; it would be gradually handed over, like post mortem examinations in a hospital, from the hands of the head man to the hands of the class, and it would introduce the practice of vivisection by students. That would be the inevitable result of it, and I believe that is intended to be the result of it in any medical school, that the person holding the license would really be very lightly responsible for what was done; his mere presence would be sufficient.

1882. Do you approve of the distinction drawn in Dr. Playfair's bill between experiments which do not cause pain, that is, which are made under anæsthesia, and experiments which must necessarily cause pain, and which are made by license from the Secretary of State?—That contemplates, as I understand it, the anæsthetical experiments for class teaching, which I wish entirely to stop.

1883. But with regard to those that are made for the purpose of original scientific research would you be satisfied with that distinction?—No, I would require the controlling power, whatever it is, to order the anæsthesia in cases where they thought fit, and to permit its absence where they thought fit, but I would leave no discretion to the operator.

1884. You would not approve of its being taken out of the operation of the criminal Acts against cruelty if anæsthesia was used?—Most certainly not, because I know the practice is to use the anæsthesia very imperfectly, and when the controlling eye is gone to drop the use of it altogether.

1885. The present law, I think, protects only certain animals, and leaves without protection certain other animals?—Yes, and there is great inconvenience in that.

1886. Then are you prepared to recommend a detailed mode of restriction based upon the view that you have given us, that there should be inspectors as there are under the Anatomy Act of 1834?—That hardly falls under the province of a witness; it is rather for the Commission; I merely threw out the suggestion, as a guide, of the supervision under the Anatomy Act of 1834, which has worked uncommonly well, and it occurs to me that if we availed ourselves of the services of the inspector of anatomy under that Act as one of two inspectors, and let the public appoint another, we should have every guarantee that is necessary. These vivisections will necessarily be always performed in connection with large cities and medical schools. There will always be an inspector of anatomy in such a town as Edinburgh, or any large town, or we can get them appointed if necessary at only a slight expense. In Cork, Galway, and Belfast there is an inspector of anatomy, whose expenses are paid by the price paid for the corpses. If the Society for the Prevention

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of Cruelty to Animals would not undertake it, I believe that local zeal would undertake it, and would appoint a colleague to him who should be elected by the town, who would be a lay person, and who would protect the public feeling, which is as much entitled to be considered as the feelings of animals in the question. I believe no difficulty would arise in a machinery of that kind.

1887. (*Lord Winmarleigh.*) The main operator, you think, would not have sufficient control over the young men that were under him, under the provisions of that bill which you have just referred to?—Not only so, but the intention of the clause is that he should have none. The intention of it is deliberately to let the youngsters do the whole thing. It is like post-mortems in a hospital; they are officially made by the medical officer who had charge of the case, but in practice they are made by the youngsters in the hospitals; and there is no harm in that if the responsible person actively supervises.

1888. You do not think that some restrictions might be put upon it so as to enforce a more exact compliance with the law?—In reference to the strong feeling I have of prohibiting students from interfering in such matters, I want to express a strong opinion based upon half a life-time's experience of medical students in dissection rooms and vivisections. Our medical students in Trinity College include some of the best and some of the worst young men in the college, and my experience is that the dissecting-room degrades some characters and elevates others; and knowing that it is a moral trial to any young man to pass through the ordeal of the hospital dead-house and the dissecting-room, that it tries and tests his disposition like the Lydian stone of the ancients, I would shrink with horror from accustoming large classes of young men to the sight of animals under vivisection. I believe that many of them would become cruel and hardened, and would go away and repeat these experiments recklessly, without foresight or forethought; science would gain nothing, and the world would have let loose upon it a set of young devils.

1889. (*Mr. Forster.*) I should like to ask you one question with regard to possible legislation, in order to enable me the better to form an opinion as to whether there should be any, and, if so, what? Both these bills agree so far, that they point to a toleration of vivisection under certain circumstances, that is to say, under circumstances of registered places or registered people; in fact, the license is under certain conditions. An objection is made by some persons to any license of vivisection, and another principle of legislation is suggested, that the Cruelty Act should be strengthened, so as to include all vivisectional experiments, both for domestic animals and for other animals, but with a safeguard that if the operation were proved to be (under certain conditions, which would have to be defined,) really for the purposes of scientific discovery, it should not be an offence. Have you considered that suggestion at all?—I have; and I disapprove of it altogether.

1890. I should be glad to know your reasons for disapproving of it?—My reasons are founded on my practical experience as a frequent prosecutor under Martin's Act. I find an extreme difficulty in getting convictions. Some one has to take the trouble to get the evidence, and to pay the expenses of bringing it forward, and for one man that is punished under Martin's Act, as it exists at present, ten escape. The difficulty of obtaining evidence of cruelty practised upon rats and guinea pigs (which are not under Martin's Act), by vivisection would be infinite, you would get no evidence. For example, in the letter which I just now read I declined to volunteer evidence against scientific brethren who have not the same feeling as myself; and, in the same way, you would find that feeling so strong in others that you could get no evidence.

1891. In the interest of the animals themselves, you would think it better to take the plan of a licensed place or person?—If a Machiavelli was putting forward a plan to promote the suffering of animals, he could not suggest a better one than extending Martin's Act, and leaving the troublesome machinery to work.

1892. You stated that you had no confidence in a mere regulation that experiments should be performed under anæsthetics. Could you give us any further reasons why you think that? I need not tell you that it is a most important part of the matter?—Well, that is notorious amongst physiologists. Unless the thing be inspected, I would not trust to it that complete anæsthesia would be produced. I would not trust the enthusiast who is dissecting to keep it up always as long as it was needed.

1893. We have heard that he would be likely to do it for the sake of the operation itself, because he would perform it with much greater ease under anæsthesia?—If it would facilitate his operation it would, of course, always be used by him, but not always if it would not facilitate it.

1894. Do you think that there are any other experiments besides those which actually could not be made under it in which it would rather interfere with the experiment?—Yes.

1895. What sort of experiments?—Experiments of very great importance and interest connected with the formation of complex chemical compounds in the blood. You always run a risk of disturbing an animal by an anæsthetic; you certainly disturb it by pain, but you further disturb it by an anæsthetic, and the products in the blood differ so from those in health that the case comes under the head, which I have already mentioned, of gross exaggeration of the importance of the result. The conditions of the experiment, both by pain and by anæsthesia in such cases are seriously altered.

1896. I have heard it stated that there is this objection to the use of anæsthetics, viewing it from a scientific point of view, that by the use of the anæsthetic you actually put the animal in a morbid condition?—Yes. If you were looking for secretions in the body, for instance, I would not trust to experiments under anæsthetics.

1897. And that is the reason why you would not allow in teaching experiments to be performed under anæsthetics?—Yes; and also for the reason that they are totally unnecessary in my opinion.

1898. (*Sir J. B. Karslake.*) What was the date of the Regulation in the Medical School of Dublin prohibiting vivisections?—The date of the formation of the practical physiology class, viz., a year and a half ago.

1899. Has Dr. Purser been the lecturer, and the sole lecturer, ever since that regulation existed?—Yes.

1900. Have you had communication with anybody, except Dr. Purser, as to the necessity of vivisections for teaching the classes?—We recognize five extra academical schools in Dublin, and they all accepted this condition, and we receive their certificates.

1901. Have you had communication with any of the lecturers in those other schools besides Dr. Purser whom you have mentioned?—Yes, with the professors in these five extra academical schools.

1902. And are they of opinion that it is sufficient to demonstrate at the lectures without having vivisection?—They are, I believe, of that opinion.

1903. I understood you to say that Dr. Purser was of opinion that that was sufficient, but that at the same time he should prefer vivisection to be allowed?—That is, perhaps, a point of dignity about his position as professor. He has authorized me to state that there is no practical difficulty experienced by him in conducting his teaching under that restriction.

1904. But that he should prefer vivisection to be allowed?—That I presume is only as a matter of dignity about his position as a man conducting original research.

1905. Did he state so?—He said "Personally, I should prefer not to have the restriction." I

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presume he meant that he felt himself slightly hurt by the university imposing a restriction.

1906. (*Chairman.*) I do not understand you to say that he wished to conduct vivisection ?—Certainly not; I think he simply meant that he would have preferred that the board should have trusted him.

1907. (*Sir J. B. Karslake.*) That is the extent of his objection, is it ?—Yes, it does not at all mean that he could teach better if the restriction were removed, but that personally he would rather that the board had left it to his own discretion.

1908. Now I understand that you propose that certain officers or inspectors should license every medical practitioner or scientific man who desires to carry on vivisection ?—Well, I can scarcely be said to propose that; I leave that to the Commission. I threw out a suggestion that, working on the lines of the Anatomy Act, you might have a similar machinery for this purpose.

1909. Have you worked that out at all further ?—No, it is not my business.

1910. Does it not occur to you that there is a great deal of difference between the operation of the Anatomy Act and any such Act as you might propose? —I think they might be made to work similarly.

1911. Let me put a case to you *à priori.* I suppose that very few people would think that nicotine would be an antidote to strychnine, yet you established that fact by experiments ?—Yes, I partially established that.

1912. You consider that you have established that by administering nicotine to a person who has taken strychnine you would effect a cure, or all events that it would be an antidote to the poison of the strychnine ?—Yes.

1913. Now supposing a person living at a considerable distance from any centre in which inspectors might reside, a man living at Penzance, for instance, had heard or read of your discovery, or that it might have come to his knowledge, and he was called in to attend a patient who had taken a dose of strychnia, and he was desirous of administering a dose of nicotine, but was not by any means certain of the result, would not your proposal prohibit that man from trying the effect of nicotine taken after strychnia on an animal ?—If you mean with a view to administering it to the patient afterwards, the patient would be dead before the experiment would be over.

1914. In that particular case it would be useless, you mean ?—Perfectly useless, and the dog experimented on would have been killed unnecessarily.

1915. In similar cases, in some cases at all events, might not the necessity arise for a medical practitioner not absolutely licensed to practice vivisection to make an experiment with a view of ascertaining the effect produced by a particular drug upon a poison taken by a patient of his ?—I do not think any hastily devised or badly considered experiment of that kind would be of any use. If a man was going in for a course of investigation, which is a long, laborious, and troublesome thing, he could get the necessary permission to do it; but I do not believe in these hasty experiments done in a hurry.

1916. May I take it that your proposal would be to prohibit, under penalty, any person under any circumstances from trying such experiments unless he was licensed to do so ?—I think so. The machinery I am not competent to suggest, but I think it might be worked through some such machinery as inspectors of anatomy associated with others.

1917. (*Mr. Huxley.*) May I ask in what sense the word "vivisection" is used in that regulation which you read to us of the University, sanctioned by the King and Queen's College of Physicians ?—It does not allow of the performance of experiments upon living animals either with or without anæsthetics, but it does not prohibit the exhibition of experiments on animals freshly killed, in which you can keep up artificial respiration, and a professor is perfectly at liberty to use that if he thinks fit; and you will understand what a range it gives us.

1918. So that supposing that I wish to exhibit Weber's experiment of the influence of the pneumogastric nerve on the heart, I could not do it ?—You could do it under that condition.

1919. What do you believe to be the difference of condition between a frog freshly killed and a frog under the influence of chloroform; do you think that a frog under the influence of chloroform is any more capable of feeling than a frog pithed ?—Certainly not. I think if you take care to keep up the anæthesia, if you did not mutilate the frog to which you applied the anæsthetic, you might let it live again and enjoy a few more months of life. If you pith it you destroy it.

1920. So that in point of fact the King and Queen's College of Physicians, by sanctioning the regulation, have deferred to an outside influence, rather than proceeded on their own sense of the propriety of such experiments ?—I do not say so.

1921. I take it from you that a frog decapitated and a frog under chloroform are in practically the same sort of state; the college says, "You may "operate on the one but you may not the other." What is the reason of that distinction ?—I must refer you to the college for an answer; I am merely a fellow of the college.

1922. (*Chairman.*) But I understood you to say that you, as the Registrar of the University, do not feel a trust that anæsthesia will always be used ?—I would not trust that, and I could not undertake to be always present to see that it was done; it would make my life miserable.

1923. (*Mr. Huxley.*) The object of my question is to show that there is no difference of state between the frog under the one set of conditions and the other, and that therefore there is no reason in the nature of things for prohibiting the one kind of experiments when you do not prohibit the other ?—The difficulty of securing complete anæsthesia during an experiment is a practical difficulty in our way.

1924. You have performed a great many experiments under chloroform; do you find any difficulty yourself in doing that ?—No, because I am most anxious to keep the animal completely under it, and I tell off an assistant for the purpose.

1925. So that supposing it were allowable to perform experiments under chloroform, if any person did honestly endeavour to carry out the provisions of the Act, he would be committing no cruelty, and doing nothing worthy of blame. You are rather assuming that nobody would try honestly to carry out the provisions of the Act ?—No, but that it would be laxly carried out.

1926. It is not necessary to suppose that anyone would violate the Act ?—From what I know of medical schools and medical students, I am of opinion that that would be the result.

1927. You told us that Professor Purser gave a course of instruction in histology and practical physiology ?—Yes.

1928. Of course I need not trouble you about the instruction in histology, as that does not concern us; but would you tell us exactly what his instruction in practical physiology consists in ?—I fancy he is limited by this regulation prohibiting vivisection.

1929. I want to know what the effect of that limitation has been ?—I do not think it has limited him seriously.

1930. Can you tell me exactly what he does teach; —I would rather you asked him himself; I have only a general knowledge; you should ask him these questions.

1931. In fact you cannot tell me of your own knowledge ?—No, except in a general way. My knowledge of the school is executive.

1932. And it is not certain, as things stand then, that anything which I should call instruction in practical physiology is given by him at all ?—It is quite possible that you might consider that the instruction in practical physiology is much limited by this restriction.

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1933. Is it not just possible that I might consider that none at all was given?—That depends upon your view of practical physiology.

1934. You said, at least I understood you to say, that no great good had been derived from these experiments regarding the action of calomel on the liver, because the doctors go on giving blue pill just as they did before?—Yes.

1935. Is there not a sort of fallacy involved in that?—You do not believe in doctors or in blue pills; you are a sceptic about doctors.

1936. What I suggest is this, that whatever might be the result of experiments on this question, the doctors probably would not alter their practice, because their practice is purely empirical; they find that giving a man a blue pill does him good, and they call it relieving his liver; but whether it relieves his liver or not, they do not know. Do you not think that the fact to which you allude arises from the circumstance that the practice of physicians in such matters is purely empirical, they have simply observed that giving blue pill under such circumstances does good, and therefore they are not in the least degree affected by the discovery of the true theory?—In either case the experiments on the livers of dogs would be of small value, because, the ultimate object being to apply it to the human body, the experiments have not altered our practice in the slightest degree, and the men who uniformly all over the world maintain that practice are a body of great intelligence; they have read all these experiments, and are able to form an opinion on them, as well as any physiologist. Even supposing a particular theory of the action of mercury on the liver is correct, it still proves that experiments on dogs have done very little for man, because they have not altered the medical practice.

1937. You are a great enthusiast as to Zoological Gardens, and as we all know one of the most valuable and efficient of secretaries of Zoological Gardens; did it ever occur to you that if absolute prohibitions of inflicting pain upon any kind of animal are carried out, you may possibly be prosecuted for keeping a Zoological Garden, shutting up animals, and bringing them into a state of disease as a consequence?—That never occurred to me.

1938. That might happen, might it not?—I think if I was prosecuted in Dublin under an Act for that purpose, I would have influence enough in Dublin to create a riot which would effectually stop all such absurd proceedings.

1939. (*Mr. Hutton.*) Have you formed any opinion of this Manual of Physiology published by four eminent men, Drs. Sanderson, Brunton, Foster, and Klein?—I have the book and have read it up and down, but never thoroughly.

1940. Do you think that its tendency is to introduce the practice of experiments amongst students?—Most unquestionably.

1941. And to spread it very much in this country?—Unquestionably that is the tendency. I have read a good deal of the book; it is a very valuable book.

1942. You have not formed any minute judgment on the particular class of experiments mentioned in it?—No, I have not.

1943. I understand you to be entirely opposed to Dr. Rutherford's view, which he declared quite recently in a lecture at Edinburgh, "that demon-"stration on living animals is absolutely essential "for the purposes of teaching?"—I am entirely opposed to that. I wonder greatly at Dr. Rutherford making such an extraordinary statement.

1944. You believe that the student can be brought up to the point at which he becomes really an accomplished physiologist without witnessing any experiments on living animals?—I believe so, with the condition annexed, which I have explained to Professor Huxley, that he is allowed to experiment on animals just killed.

1945. But with that condition you consider that a man might be brought to the point of an accomplished physiologist, and able to conduct a laboratory himself, without having himself practised on living animals up to that point?—I believe that if he was a person gifted by nature with the capacity for such a thing as original research, in a very short time he would make himself as accomplished as any experimenter. I may illustrate that by a thing that will occur to everybody's knowledge. We never teach students the practice of surgery on living men; we could not allow a student to amputate or cut for stone, but they are taught to practice again and again on the dead body, to tie up the artery, and do everything as if the living man was on the table; and some time ago one of our young students got a high prize in surgery, and was examined for this high prize before a large class; he got very great commendation, and is now high up in the Indian army. He excised an elbow-joint on the dead body, and shortly afterwards, having completed his education and taken out his degree, he performed the same operation on the living subject with as much readiness and coolness as if he had been all his life at it, and yet he had never in his life put a knife into a living man before. Therefore, I believe, in the same way it is not necessary for a student to practice vivisection himself.

1946. (*Mr. Huxley.*) I understand you to limit that remark to persons having decided gifts?—Certainly, I would not allow blockheads of any kind to cut up animals, and too many of them are at it.

1947. (*Mr. Hutton.*) With regard to anæsthesia, is there not extreme difficulty in giving anæsthetics to the lower order of animals without killing them, and so destroying the benefit of the experiment?—Yes, I have tried to produce anæsthesia in lions and tigers, and have completely failed.

1948. And in frogs, for instance?—I would pith them. I would divide the spinal cord, and operate on them.

1949. But can all the operations be performed after the frog is pithed which might be performed under anæsthetics, for the purpose I mean of physiological teaching?—No.

1950. And is it not true that a good many frogs have a great deal to suffer, because it is difficult to give them an anæsthetic without killing them, and therefore the operations are performed on them without anæsthetics?—There are experiments which are wanted to be performed on a frog, where neither pithing it nor anæsthesia would be allowable. I think that pithing (I speak subject to Professor Huxley's correction) would give you the means of making all the experiments that chloroform would; but there are a class of experiments where the pain itself is the subject of research, where neither pithing nor anæsthesia can be allowed, and then the frog must suffer.

1951. Have you ever had occasion to interfere to prevent what you thought useless and needless physiological experiments on living animals?—I have mentioned to the Commission that I took an active part in that direction at Norwich, and I did at Oxford also. In addition to that case at Norwich in the year 1868, at Oxford there were some experiments to be made by Professor Marey, and the question was raised at Oxford as it was at Norwich; but fortunately in Oxford it was decided to refer the matter to a committee, and the committee revised the experiments, in fact introduced practically this very supervision which I advocate. Certain experiments were allowed to be made, and others were prohibited, and everybody was satisfied because the thing had not been left to the discretion of this French experimenter or enthusiast, but had been carefully considered by competent men. If that had been done in Norwich the public would have been spared the shock, for it did cause a great shock; in fact the Frenchman was in danger I think.

1952. In regard to the ethical limits you would put on experiments, would you say that you would require that a real remedial agent should be in

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prospect, or would you allow them as a mere addition to knowledge ?—As a student I must believe in every advance made for science, and follow it with the greatest ardour.

1953. But would you limit such experiments by saying that a particular and a probable discovery should be in the mind of the operator at the time of the discovery, a discovery which seemed to him already probable ; that he should already anticipate what he expected or hoped to discover, and not that he should simply perform operations on animals, as he would on minerals or vegetables, with the vague hope of discovery ?—I have endeavoured to explain all that in what I said about foreknowledge and preconsideration of the whole question. No man is fit to be in the army of science who makes an experiment at random. He must, whether operating on brute nature or on organic nature, have in his mind's eye some clear thing, whether right or wrong, that he hopes to prove or disprove. And I would prohibit any man who had not such an object from making such an experiment at all ; he ought to be dismissed.

1954. Now these operations on biliary fistula, for example, of which we have heard so much, appear to me to open a great many more questions than they supply answers to, giving only results of the vaguest kind. Would you exclude them ? For instance, I do not know whether you have read the account of the operations on cats in St. Bartholomew's Hospital ?—I voted for the grant to be given to Professor Hughes Bennett for those experiments, because I believed that they had a definite intelligent object in view, and that it was worth inquiring into ; but I was disappointed at the smallness of the result. That we cannot help. I merely instance those experiments, not to say that I disapprove of them, but I think that their importance was exaggerated by myself and others before they were made, and the result was very small.—*Parturiunt montes ; nascetur ridiculus mus.*

1955. (*Mr. Huxley.*) As a practical investigator in science, may I ask you is it not often the case that getting no result is getting a very great result. You have decided at all events that a particular agent does nothing ?—In the case of the experiments on the liver of the dog, that result was very small ; but cases might occur in which a negative result would be very important.

1956. (*Mr. Hutton.*) Still supposing that the result opens up many more questions than it supplies answers to, would you be prepared to allow experiments to go on, as Mr. Ray Lankester said, in geometrical progression as in any other experimental science ?—I would, under proper supervision.

1957. That makes your limitation a very slight one, surely, upon this class of experiments. I did not fully understand you whether you would leave it at the discretion of the inspector, the inspector being an accomplished physiologist, whether or not to allow any vivisection ?—No, I would not set a thief to watch a thief ; I would not set a physiologist to allow another physiologist to go to work ; I would have a bonâ fide inspection. The difficulty is to get that. The public must be represented on it ; and I doubt if an inspection by less than three competent persons would have the confidence of the public. A single inspector out of laxity might be persuaded to give permission.

1958. But the representative of the public would have the power to refuse, I suppose ?—Yes, I suppose so.

1959. And the representative of the public would be very likely to refuse if he thought that the experiments were not of a class likely to do good to man ?—I think very likely he would, and very proper that he should, because I consider that the general sentiment among educated people outside is to be considered as well as the advance of science.

1960. That I think limits the answer that you gave me before, that you would allow any vivisectional experiments in the pursuit of science ?—Subject always to this enlightened control in which public opinion would be fully represented. How to provide that I think is the difficulty ; but I have no doubt the Commission will see their way to it. I might be allowed to say that there is a great deal of nonsense talked in the name of original research. It seems to be rapidly taking the same place in the creed of philosophers that the doctrine of original sin did amongst theologians. By putting a man to make a set of experiments you cannot make him produce results unless he is born into the world with the intellect and the ability that nature gives for such results. No amount of training will make a man an original observer. It is a case of *poeta nascitur non fit*, and I reiterate that in no science is the abuse of the idea of original research greater than in physiology. Every person, second or fifth rate physiologist, sets to work to make original researches, and they are not competent for it. I think that public opinion ought to be brought more fully to bear upon that, and to see whether the objects of this research are as important as their professors say they are.

1961. And that makes you less anxious about the fear of strangling original research ?—If you were to strangle nine-tenths you will do no harm, if you leave the one-tenth that nature meant to go on.

1962. (*Chairman.*) You have not come prepared to detail proposals to the Commission, not regarding that as your province ?—No.

1963. But you are prepared to say that experiments on living animals should not be tolerated at all for the purpose of teaching ?—I am.

1964. But that for the purpose of original research they ought to be tolerated, but regulated ?—Yes.

1965. That that regulation should be in order to take care that the vivisection is only done by competent persons ?—Yes.

1966. That it should only be done for an adequate object, previously well considered ?—Yes.

1967. Everything which leads up to knowledge being arrived at otherwise than by painful experiments, and nothing left to painful experiments except that which can only be arrived at in that way ?—Yes.

1968. That this should be done under a supervision which it has occurred to you might have an analogy to the arrangements of the Anatomy Act ?—Or might be based upon them, making that inspector one of the inspectors for this purpose.

1969. That the proceedings of all persons so engaged should at all times be subject to the influence of public opinion ?—I wish to express that opinion very strongly, that this control should be partly under the direct influence of public opinion.

1970. So that the legislature might know from time to time what was going on, and whether any further restrictions or limitations might be necessary ?—Yes.

1971. (*Mr. Forster.*) Referring to the questions asked by Professor Huxley with regard to Professor Purser's teaching, let me ask, do you or do you not consider that by being unable to perform vivisectional experiments in teaching he was prevented from carrying out fully what ought to be his object as a teacher ?—I do not think so, most certainly, because with the condition that I mentioned to Professor Huxley, of his having it in his power to use recently killed animals, I believe that he can teach everything that practically is necessary. That of course is only my own opinion ; Professor Huxley or Professor Purser might have a different opinion.

1972. Sir John Karslake put to you the case of a practitioner in a secluded part of the country having a man who had taken poison brought before him, and thinking it necessary to try some experiments with regard to antidotes. You replied that it would be doubtful whether that would be useful, because the poison might take speedy effect ?—It would be certain to be useless.

1973. But do not you think that a case of possible utility might be met by a clause in any bill which

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would prevent a man being punished who might try experiments solely in the cases of patients who had taken poison ?—I do not think such an experiment would be at all necessary. Where any accident of that kind occurs, if a man knows enough about it to know that a certain thing is an antidote, he will have at hand the details of doses and all that is necessary to counteract that poison, and he could not possibly set to work making experiments on it. It was quite an imaginary case.

1974. (*Chairman.*) If you had been yourself poisoned you would not like to be treated, would you, by a man who knew nothing except from experiments which he made in the interval himself ?—Certainly not, I should be dead in the interval. It is a purely imaginary case.

The witness withdrew.

Mr. A. H. Garrod.

Mr. Alfred Henry Garrod called in and examined.

1975. (*Chairman.*) You are Prosector to the Zoological Society of London ?—I am.

1976. Will you explain to us what the word "Prosector" means ?—The appointment is one which deals only with dead animals. Those animals that die at the Zoological Society's gardens pass through my hands, chiefly for zoological purposes, also for pathological purposes; but I have, in connexion with other appointments, opportunities of performing experiments in physiology, which are rather according to my wishes. I prosecute experiments for my own instruction.

1977. For scientific purposes do you mean ?—For scientific investigation.

1978. Is it your practice to perform them under anæsthetics ?—It is my practice always to employ anæsthetics so far as to produce complete anæsthesia.

1979. Do we understand that all the operations which you perform on living animals are performed when the animals are in such a state that they feel no pain whatever ?—I have every reason to believe that they feel no pain whatever.

1980. Do you approve of the unlimited toleration of the practice of experiments upon living animals, or do you suggest any, and if so what, limitations ?—I approve of the entire absence of limitation as regards the employment of vivisectional experiments in those cases in which original investigations are being performed by any man who is at all competent from physiological knowledge to undertake the operation; but with regard to the employment of vivisectional demonstrations for purely educational purposes, I have a personal prejudice, which I think may be more instinctive than otherwise, against looking on at demonstrational educational vivisectional experiments.

1981. Do I rightly understand you to say that you think in the hands of competent persons, original research should be left unfettered ?—I think entirely unfettered.

1982. In the hands of incompetent persons I suppose vivisection should be prohibited ?—I have never had an opportunity of witnessing any incompetent person attempting anything vivisectional of importance, so that I cannot feel that any restriction is required in that direction; because I think that the existing law with regard to cruelty to animals would include all the cases.

1983. Are you aware that animals like those in the Zoological Gardens are not within the Act for the prevention of cruelty ?—I suppose that is the case.

1984. How do you suppose that the existing Acts would operate sufficiently to protect those animals ?—I think that vivisectional experiments would be undertaken by hardly any men, of their own accord, without rapidly producing such experience in the individuals trying them as would justify their prosecution of those experiments.

1985. Your opinion then is, that although a person is not competent to perform such experiments to begin with, he becomes competent quickly, so that there is no need of restricting him ?—I think that two or three experiments will render persons competent to perform the experiments, or that if they feel then that they are not competent to go on, they will cease of their own accord.

1986. But with regard to demonstration for educational purposes, you would prohibit vivisection with that object ?—I think that for educational purposes quite enough can be taught without the employment of demonstrations by vivisection, and that a perfect understanding to that effect would be very beneficial to the country.

1987. But now are you seriously of opinion that a person who is incompetent to begin experiments on living animals, will become competent to perform such experiments after a few trials ?—I cannot believe that anyone who is so uneducated as not to gain rapidly the power of making such experiments would prosecute vivisectional experiments for any length of time, any more than any person would frequently torture a cat by vivisectional experiments if he is an educated individual.

1988. But suppose that he is uneducated ?—That is beyond the limits of my experience altogether. I do not believe that uneducated people would practise systematic vivisection; there is nothing inherently attractive in vivisection to the majority of human beings.

1989. Do you believe that there is no wanton cruelty at all in regard to vivisection, either in this or in other countries ?—As far as vivisectional demonstrations for educational purposes are concerned, any number of those in foreign countries, and perhaps in this country, would be beyond what I should approve of; but otherwise I do not think that there is any barbarism carried on.

1990. In any country ?—I think that vivisectional surgical operations are objectionable, and I know that they were conducted on a large scale in France some years ago.

1991. But as far as original research is concerned, you have a thorough conviction that there is nothing in this country which would, and nothing in any other country which, if adopted in this country, would be a fit subject of legislative interference ?—I quite think that there is no legislative interference required for that.

1992. (*Mr. Huxley.*) I should like to ask you one question about the employment of vivisectional experiments in education; are you speaking now of general education or special physiological education ?—My remarks referred to the education of classes in special physiology.

1993. That is to say, if I understand you, you do not think it requisite that a student devoting himself to physiology should have any practical instruction ?—I think in regard to vivisection that a student who could make any use of the physiological instruction he is receiving by it, could gain almost all the information he required without any special vivisectional operations being conducted by himself, or in his presence.

1994. By vivisection do you mean merely experiments on living animals while they are in a condition to feel, or do you include under that head experiments on animals pithed or decapitated, or under the influence of chloroform ?—I do not include experiments on animals pithed or decapitated, but I mean experiments on animals under the influence of anæsthetics. I should include them, because I know that when an experiment is being conducted before an audience, there are many causes which would make an operator, from slight nervousness, or otherwise, pay less respect to the animal than might be desirable, and than would be most likely the case during the pursuit of original research.

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1995. I am thinking, not of experiments made before a class of ordinary medical students, but of the kind of practical teaching which might go on in a physiological laboratory, practical instruction running *pari passu* with the instruction of a professor, in the same way as practical instruction goes on in a chemical laboratory *pari passu* with the instruction of a professor. Have you any objection to that kind of use of vivisection?—It seems to me hardly necessary that everyone should repeat the important fundamental physiological experiments for himself, because I think, as a rule, the descriptions in text books are sufficiently good to render any student of comparative intelligence fully able to understand what he would learn from the actual experiments.

1996. You have gone through a course of thorough instruction in chemistry, I think?—I have.

1997. Then I need not remind you that every student, it he is to become a chemist, not only attends the lectures of the professor, and hears the description of the properties of oxygen and hydrogen, and so forth, but is absolutely made to spend a great deal of time in the laboratory in studying the properties of these elementary bodies, which are as well known as anything can be, for himself; and any chemist to whom you suggested that a student of chemistry need not go through such a training would laugh you to scorn. I think you will agree with me in that?—Yes.

1998. Do you think that there is such a great difference between physiology and chemistry that mere description is not sufficient to give you a competent idea of the comparatively simple phenomena of chemistry, while it is sufficient to give you a competent idea of the complex phenomena of physiology?—You refer to chemistry being taught practically, but let me say that a subject quite as large, namely, surgery, is taught by purely theoretical lectures, because it is found impossible, on account of the nature of the operations conducted, that it should be otherwise. That has to be taught by theoretical means only. The student of surgery no doubt does suffer from not being able to operate on the human body, but he has to make up for that on account of the disadvantages connected with practical surgery on the living body by extra study. I think the disadvantages connected with practical vivisection in physiological experiments require the student to study so much more to get a thorough grasp of his subject, and I think that that extra study would make up for the deficiency, just the same as in surgery, in the practical operation.

1999. But is there not this very great difference between the two cases, that the surgeon is able by his study of anatomy beforehand thoroughly to know all the relations of the parts, and the only thing that he has to learn is that comparatively small difference which they present in the dead and the living state; whereas, in the case of physiology, I am sure I need not tell you that no man could obtain the slightest conception of the operation of a living muscle by merely looking at it in the dead state?—I quite agree with those remarks, but I think that the difference between the operation of cutting a dead body, which the ordinary student performs, and his first operation on the living body has something more in it than the difference of the appearance of the tissues, and that the responsibility connected with the whole subject produces a difference of sensation in the operator. So in physiology I think no doubt it is necessary to study the phenomena of living muscles, that as far as original investigation is concerned I think is quite correct, but for the demonstration of things already well known, I think that is not more necessary than for the demonstration of surgical operations.

2000. Let me ask you whether there is anything which is much easier to understand, as a mere matter to talk of, than the circulation of the blood in the capillaries?—It is very easy.

2001. And is there anything in which the idea present in the mind, after talking of it, is more different from what you get by your seeing it, than it is in that case when you have seen the circulation in a frog's foot?—I do not realise the difference as being so great.

2002. A distinguished witness who appeared before this Commission some time ago (and, by the way, an opponent on the whole of vivisection), was so much impressed by the difference between seeing a physiological phenomenon and merely talking about it, that I think he rather regretted he had not brought a fish's heart to show in pulsation. His opinion was that the difference between merely talking about the movement of the heart in circulation and seeing it was so great. I understand you not to agree with that?—I think a student looking at the heart for five minutes would learn very much less than he would by reading for the same length of time as to the circulation.

2003. But suppose he had prepared himself before by reading, and then studied the phenomena as they occurred in nature, would he not have a grip of them such as he could not have by any conceivable reading?—My experience is not quite in that direction.

2004. (*Mr. Erichsen.*) Am I correct in understanding that you do not object to certain kinds of experiments for demonstrational purposes, those, for instance, in which the animal is pithed?—I have not the least objection to demonstration where the animal is pithed or decapitated.

2005. But only where it is anæsthetised?—Only where it is in a condition from which it can return to the normal condition afterwards.

2006. You have had a good deal of experience in anæsthetising animals, I conclude, from what you said in the early part of your evidence?—I have anæsthetised a great many animals.

2007. Have you ever found any practical difficulty in anæsthetising animals so as to render them perfectly, so far as you can judge, insensible to pain?—I have had on more than one occasion to transfer one method of operation to another. For instance in giving chloral to a ruminant animal, the same dose that would anæthetise a horse or donkey will not anæsthetise a ruminant animal, and I have had to transfer my anæsthetic to vapours, but I have found no real difficulty.

2008. By employing the proper anæsthetic according to the animal you deal with, you have found no difficulty, employing one anæsthetic for one animal and another for another?—None.

2009. Have you been in the habit of anæsthetising the larger animals; you have mentioned the ruminant animals?—I have anæsthetised on one occasion the largest ruminant animal that ever lived, the giraffe, perfectly.

2010. Are any pathological experiments conducted at the Zoological Society's Gardens?—No, there is no systematic pathological experimentation; the Society has no object in view in conducting anything of the kind.

2011. The Society itself, I take it, does conduct a gigantic pathological experiment?—You mean in keeping the animals there at all?

2012. And in engendering tuberculosis; they almost all die of tubercule, do they not?—No; I do not think 40 per cent.

2013. But as many as 40 per cent. do you think?—Not so many; the larger number die from conditions which it is not possible to find any reason for; tubercule is the most common disease from which they do die.

2014. (*Sir J. B. Karslake.*) That is to say taken all round?—Taken all round more animals die of tubercle than anything else.

2015. The monkeys particularly die of tubercle, do they not?—The old world monkeys.

2016. (*Mr. Erichsen.*) That being generated by the conditions in which they are placed?—Undoubtedly.

2017. (*Lord Winmarleigh.*) Have you been able to form any judgment of the difference between the length of life of an animal confined in the Zoological

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Gardens, and an animal at large in its natural state?—Only with regard to those animals which are accustomed to be domesticated. Our cattle and deer live for a considerable length of time, quite as long as they are known to have lived when feeding in fields. We have had oxen live for 25 years, for instance, and that is a good age.

2018. As to lions and tigers is that the case?—I should say the tendency for them is rather to live shorter lives when they are confined.

2019. (*Chairman.*) But then they are free from the accidents to which they would be exposed in a state of nature?—They are free from accidents, but they suffer from plethora or want of exercise.

2020. (*Mr. Hutton.*) May we assume that you were not obliged to go through a practical physiological laboratory yourself before completing your education?—I was not.

2021. In fact you completed your education without any experiments on living animals at all, as I understand?—Exactly.

2022. (*Mr. Forster.*) You have had great experience in the administration of anæsthetics; have you formed any opinion on this matter as to whether it is or is not more difficult to be quite sure that the anæsthetics are completely administered to an animal, than you would be if they were administered to a human being?—I have never found any difficulty in that way. Perfect quiet on the part of the animal during the performance of an important skin section, for instance, I always take as a test of complete anæsthesia.

2023. But there are cases which we have heard of, in which a man or a woman has had anæsthetics administered, and during the operation they have groaned or shrieked, but afterwards have declared that they suffered no pain. That of course is a means of obtaining knowledge that would not apply to an animal?—I think you find that animals indicate to anyone watching them, quite sufficiently by sounds, whether or not they are in pain. They groan just in the same way that a human being does, they emit a kind of groan which is quite symptomatic of pain. I always take it for such.

2024. The meaning of my question is this, that there might be complete anæsthesia with a human being, and yet he might groan during the operation, but you find out from him afterwards whether the anæsthesia had been complete or not; but as in the case of animals you could not find out afterwards whether it had been complete or not, you would think that it was necessary to produce perfect quiet before you had done enough for an animal?—That is my general practice.

2025. (*Lord Winmarleigh.*) Did I rightly understand you to say that during your practice animals have groaned under anæsthesia?—The symptoms for repeating a dose of chloroform would be the least indication to kick, just a movement, a slight raising of the leg, or a slight flinching of the open eye, or the slightest noise from the mouth, not connected with respiration.

2026. But an animal when it is in full vigour and suffers pain does groan, does it not?—I think it is not at all difficult to recognize in a horse or an ox the least pain.

2027. (*Mr. Hutton.*) Have you made any experiments of that kind on frogs?—I have very seldom anæsthetised them with chloroform. I have given them ether frequently, and found that they have remained under it a very considerable time.

2028. But not chloroform?—I have not given them chloroform.

The witness withdrew.

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FREDERICK WILLIAM PAVY, M.D., F.R.S., and PHILLIP HENRY PYE-SMITH, B.A., M.D., called in and examined.

2029. (*Chairman to Dr. Pavy.*) You are physician at Guy's Hospital and lecturer on physiology?—Yes.

2030. In your capacity of physician and physiologist at Guy's Hospital you have considerable experience, no doubt, on the subject which is referred to this Commission?—I have.

2031. Are you of opinion that the practice of experimenting on living animals is one which subjects living animals in this country to great suffering?—I may say to an almost insignificant amount of suffering. Experiments need not be performed to submit animals to any appreciable amount of suffering, that is to say, the generality of experiments.

2032. You mean owing to the application of anæsthetics?—Yes.

2033. And I understand you to say, that so far as Guy's Hospital is concerned, anæsthetics are always employed?—I have lectured for 20 years on physiology at Guy's Hospital; I am for the 20th year, I would say, lecturing on the subject now; and I believe I was the first person in London to commence using experiments in illustration of physiological lectures, and I have invariably employed anæsthetics for the purpose.

2034. (*Lord Winmarleigh.*) From the first?—From the first. I feel perfectly satisfied that anything else would not be tolerated for a moment. No class of students, if I know anything about medical students, would tolerate the introduction of a living animal into the theatre, and a vivisection performed on that animal whilst the animal was not under the influence of some anæsthetic.

2035. (*Chairman.*) What you say with knowledge as regards Guy's Hospital, you say from general information and belief as regards the practice in this country generally?—I do. I do not mean to say that if only a few students were together when such experiments were practised, the same objection would always be raised by them as would be raised in the lecture theatre at Guy's, for instance, when a number of students were gathered together; for amongst a large number there would be sure to be found some who would object.

2036. And with regard to the general sentiment of physiologists and surgeons in this country, do you think you may venture to say, with regard to them, what you have said positively with regard to your own practice at Guy's?—I thoroughly believe so.

2037. Is it your opinion that the practice of experimenting on living animals has, in the last few years, been very considerably increased in this country?—I think not. There is a popular idea that it has been increased, which I think arises from this cause. A little time ago the College of Surgeons required a course of practical physiology; they wished the subject to be taught more practically than it had hitherto been, and required attendance on this course of practical physiology. I think that at the time the regulation was made, the authorities of the College of Surgeons scarcely understood what this course of practical physiology was to include; it was left to the lecturers. We tried to get information from them, but all we could hear from them was that they required that practical instruction should be given. This practical instruction now resolves itself essentially into practical histology, as I think my colleague, Dr. Pye-Smith, who superintends that course, will tell you, that is to say, work with the microscope, the microscopic examination of the tissues. The different organs and tissues are examined manipulatively; each student works with his own microscope; and my colleague superintends the course.

2038. (*To Dr. Pye-Smith.*) You are assistant-physician at Guy's Hospital and lecturer on physiology, together with Dr. Pavy?—I am.

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2039. You have paid attention to the subject which has been referred to this Commission?—Yes.

2040. Are you of opinion that for purposes of medical discovery and science, it is possible to do altogether without experiments upon living animals, or that such experiments are necessary?—If by "medical study and science" you include physiology in the full sense, undoubtedly it is impossible that they can be done without.

2041. Are you acquainted with what is done in this country on that matter?—I believe I am.

2042. It is proposed as you know that we should report whether legislative interference of any kind is necessary for the satisfaction of the public mind, and for the prevention of the infliction of unnecessary suffering on animals. Are you prepared to offer any opinion to the Commission on that subject?—With regard to what may be necessary for quieting the public mind, I do not feel able to form an opinion. With regard to the other question, I should have thought that until abuses were proved, legislation was unnecessary. I am not aware of any necessity for it.

2043. You are not aware of any abuses?—No, I think I may say so certainly in this country.

2044. You are familiar with what is known, through published medical journals, of what is taking place in other countries?—Yes.

2045. Would you extend to all countries the opinion that there are no abuses for which correction would be desirable?—No, I do not feel able to do that.

2046. May I take it then as your general opinion, that it is not desirable that the practice in this country should become such as those journals would represent it to be in some other countries?—Yes, I think you may.

2047. And that if there were any tendency observable towards such a state of things in this country, it might modify your opinion as to whether any legislative interference was necessary?—Certainly.

2048. Reverting then to the practice in this country, is it your opinion, that a sense of humanity governs the physiologists and surgeons of this country in the practice to which they have recourse?—Yes, as far as my own experience goes, undoubtedly.

2049. In the first place, are anæsthetics used whenever it is possible to use anæsthetics?—I believe so. I always do so myself.

2050. Is the proportion of experiments in which anæsthetics can be used a very large proportion?—There is only a very small proportion of the experiments in which it is impossible to use anæsthetics.

2051. Then in the very large proportion of experiments, anæsthetics are used?—Certainly.

2052. And in those cases I suppose the anæsthesia is complete?—Certainly; it is badly done if it is not.

2053. So that in reality in those cases the animal is not subjected to anything that can be called torture?—I ought to explain perhaps that there is a certain set of experiments to which that would hardly apply; I refer to experiments in which it is desirable to ascertain the action of some drug, or the effect of some operation. The administration of the drug, or the performance of the operation, can take place under anæsthetics, and will cause no pain whatever, but the experiment is not then complete, and of course it is necessary that the animal should live after the recovery from anæsthesia for a certain time to see the effects of the experiments. During that time there may be a certain amount, not often I should think of pain, but of discomfort.

2054. Then we may take it generally that in the larger number of experiments complete anæsthesia is produced?—Certainly.

2055. Of the other experiments, in a considerable proportion the most painful part is done under anæsthesia?—Certainly.

2056. Then as to the small number of experiments that remain, are they very painful?—I confess I have very little experience in that. I have never performed an experiment on an animal, nor have I seen one performed, without anæsthetics.

2057. As a physiologist, is it your opinion that there is any large proportion which ought to be performed at all, and where the removal of pain by anæsthetics is impossible?—Certainly not.

2058. Is it within your knowledge that any incompetent persons perform experiments of this kind?—No, I have never known such an instance.

2059. Then you do not think that legislation is necessary for the purpose of preventing the practice falling into the hands of persons who ought not to be allowed to perform experiments?—I think not.

2060. (*To Dr. Pavy.*) Referring only to published medical reports with reference to what takes place in other countries, are you of opinion that there is nothing done in any other country which, if it were done in this country would be considered to call for legislative interference?—I think that more is done in other countries than is done here.

2061. More you believe to be done in other countries than would be allowed by the sentiment either of the medical profession or of the pupils in this country?—Yes.

2062. And which, if it were done in this country, would give the British public a right to consider whether legislative interference was not necessary?—Certainly. In support of what I mentioned just now, I should like to state that at the commencement of my course I am almost obliged to give a little apology for saying that the course will be an experimental one. I see upon the faces of the students sitting before me a feeling which leads me to consider it necessary to make some explanation, and to tell them at once that no experiment will be introduced which will wound the feelings of the most sensitive amongst them. I made use of that remark only a few lectures ago, and it is what I am constantly in the habit of doing, and I have found it necessary from what I have seen in my audience.

2063. With that feeling, I need scarcely ask you, both you and your professional brethren entirely sympathize?—Yes.

2064. Now are you at all aware or disposed to believe that the practice of experimenting on living animals falls sometimes into incompetent hands?—I have no knowledge of it at all.

2065. You do not believe it?—I do not believe it.

2066. You do not believe that that kind of suffering is inflicted upon animals in this country which would be inflicted if either bungling operators were to undertake it, or if the number of victims were increased by unnecessary and ill-considered experiments being performed?—I am not aware of anything of the kind. I do not know of experiments being undertaken by medical students.

2067. You do not believe that students in their private rooms do undertake experiments?—I do not believe it.

2068. If it should be established that such practices were introducing themselves into this country, would you think them worthy of legislative repression?—I think so, certainly.

2069. You have seen, I daresay, the various proposals that have been made for legislative repression?—Yes.

2070. Particularly two bills that were introduced into the two Houses of Parliament last session?—I took particular notice of the bill which was introduced by Dr. Playfair, and I felt that if that bill were introduced, it would alter nothing of what is done now.

2071. Meaning by that, if I rightly understand you, not that the bill would under all circumstances and in all countries have been inoperative, but that nothing in violation of it is now done in this country?—There is nothing, as far as my experience goes, in violation of it done in London.

2072. By the limitation "in London," do you mean that you are not acquainted so well with what goes on in other places, or that you purposely reserve the

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remark exclusively to London ?—I reserve my remark to London, because my experience does not extend beyond London.

2073. You are not prepared therefore to give an opinion either way ?—No. I do not know what is done beyond London, but I do not believe that more is done in this country beyond London than is done in London.

2074. This at least may be said, that Dr. Playfair's bill would not be any restraint or impediment upon the legitimate practice of experimenting upon living animals, as you understand it ?—I consider that it would not.

2075. (*To Dr. Pye-Smith.*) You have heard Dr. Pavy's evidence; will you tell me whether you agree in it ?—Yes. And I may perhaps add one point on which I can supplement it. I know personally the gentlemen in charge of all the laboratories in England out of London; they are very few; namely, in Edinburgh, Manchester, and Cambridge, and I doubt if there is another. At Cambridge I worked myself, and I know what is done there, and it is exactly what is done in Dr. Pavy's laboratory. In Edinburgh and Manchester I have not been in the laboratories, but I know the officers personally, and from conversation and knowledge of their character, I am quite sure they would agree absolutely with what Dr. Pavy has said on this point.

2076. (*To Dr. Pavy.*) Have you any knowledge of what goes on in any private laboratories ?—I do not think there is anything going on in strictly private laboratories; I have no knowledge, I have not heard of it.

2077. (*Mr. Forster.*) Do you know of any private laboratories ?—I do not.

2078. (*Chairman to Dr. Pye-Smith.*) Do you know of any ?—No.

2079. (*Lord Winmarleigh to Dr. Pavy.*) I understand from your evidence, that you think that any legislative interference would be injudicious. You stated that you did not believe that there was any practical grievance connected with the subject of our inquiry that required legislative interference ?—It would pacify the public feeling if the bill of Dr. Playfair were passed; and I think that what the bill would enact would be nothing more than what is just. It would not alter my own course.

2080. You have of course either read or had some knowledge of the handbook for a physiological laboratory ?—Yes, I am quite familiar with it.

2081. To persons unacquainted with the details, there appear from that book to have been experiments made which must have given very great pain to animals, from the very description given of them ?—It is an unfortunate way of expressing it. The book has been written, and the experiments have been described just as though persons were to perform them, but that is not done.

2082. But I have got one experiment before me now where it appears to have been done ?—I mean not done in the schools.

2083. Then do you think that there are no other places where these experiments are made ?—Those experiments are principally described from continental sources. The persons who have written in that work have derived their information especially from continental sources.

2084. (*Sir J. B. Karslake.*) Do you think that there has been any tendency to increase the amount of vivisection in this country in consequence of the publication of that book ?—I think not.

2085. Although it points out how these things may be done, medical students and others are not in the habit, you think, of ascertaining whether they can do the things pointed out there ?—I do not think it is a book consulted generally by medical students; it is more a book for the advanced, for professors.

2086. I understand it to be your opinion, after considerable practical experience and knowledge on the subject, that medical students and others do not perform these operations and these vivisections in their own private rooms ?—Yes, certainly.

2087. And if there is any public opinion that that is the case, in your opinion that public opinion is ill founded ?—Unquestionably.

2088. How many courses of lectures do you deliver in the year ?—One course in the winter session of six months.

2089. What are the chief animals which you choose for the purposes of the experiments ?—Dogs, rabbits, guinea pigs; I suppose, if you are to include it amongst experiments, one might say also a dormouse, which is used just to show hibernation, but on which no vivisection is performed.

2090. I meant the animals on which vivisection is performed ?—Frogs, in addition to those already named; those are the chief animals.

2091. In every case are anæsthetics applied ?—For lecture purposes, except in the case of frogs, which are usually rendered insensible by pithing.

2092. So that no student sees an animal experimented on except under anæsthetics ?—No, not if there is a cutting operation performed upon it.

2093. In what cases do students see animals upon which anæthetics have not been used, and which are brought into the lecture room ?—I may give an illustration of what I have done already this year. To show the changing composition of the blood, I introduced into the stomach of an animal a liquid consisting of sugar and yellow prussiate of potash. This was effected by passing a flexible tube down through the mouth and the gullet into the stomach, and then it was injected by means of a syringe. No inconvenience at all was occasioned. The urine was afterwards collected by pressure over the bladder, and the rabbit, after this had been performed, was introduced before the students just to show what had been done.

2094. Was this rabbit afterwards killed or did it live ?—It lived; it had no pain or any inconvenience.

2095. It suffered no pain or inconvenience ?—No pain or inconvenience; the rabbit is alive and well now. It was not a cutting operation; no knife whatever was used.

2096. Can you give any notion of the number of animals that are used in the course of the series of lectures ?—I should think perhaps in the course of the session, say 20 dogs and 8 or 10 rabbits.

2097. And to those animals that you have mentioned the knife is actually applied ?—Under the influence of chloroform or some other anæsthetic, because we sometimes use puff-ball where we do not want to weaken the action of the heart.

2098. As far as your observation has gone, in every case has the anæsthesia been complete during the operation ?—Quite complete.

2099. There can be no doubt about that in your own judgment ?—No; and I would say further that the operation is very much facilitated by the anæsthetic being complete. If we had not anæsthetics to use we could not get on with our operative experiments as we now can. It is of the greatest assistance; apart from any question of humanity in the matter, the anæsthetic affords the greatest assistance to physiologists. The animal under anæsthesia lies perfectly quiet, and the experiment can be carried out without any interference whatever; whereas if the anæsthesia were not complete, the experiment in many instances would be interfered with.

2100. Do you find it at times necessary to repeat the administration of chloroform during the operation ?—We go on with the chloroform; the experiment may require that the animal should be kept under the influence of chloroform, for instance, for two or three hours.

2101. I think we heard from a witness that there are some particular regulations or rules as to the introduction of animals into the hospitals. Is that so to your knowledge ?—I am not aware of any.

2102. That there are certain persons who supply dogs and rabbits for vivisection, and there are rules

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issued by the authorities as to the mode in which these animals are to be introduced ?—No, not at Guy's.

2103. (*Lord Winmarleigh.*) How do you get them ?—The assistant gets them, he buys them; he goes over to Leadenhall Market and purchases our rabbits there and our dogs too.

2104. (*Mr. Forster.*) Without any concealment at all of the object ?—Certainly, they are brought in during the day.

2105. (*Chairman.*) I understand that you have nothing to conceal at all ?—Nothing at all.

2106. And desire no concealment ?—In the lecture room I may say that I have always lectured under the feeling that at any time a member of the Society for the Prevention of Cruelty to Animals might be present.

2107. I think we have been told by the secretary of that society that you offered to give them every facility of knowing everything that was done ?—I did, and said that they might attend the lectures when they liked. They visited the laboratories and had explained to them what was done.

2108. (*Mr. Huxley.*) I think I understood you to state that during the 20 years that you have been engaged in teaching physiology, you have always made use of these experiments as illustrations of your lectures ?—Yes.

2109. I judge from that, that you must very early have been convinced that the teaching of physiology without such practical illustrations is very maimed and imperfect ?—I felt so convinced of it when I came over from France, that I at once commenced to make the course an experimental course. I felt that instruction could not be perfect without it.

2110. I apprehend that you have gone through the ordinary training of a medical student, and you have doubtless paid great attention to chemistry ?—Yes.

2111. In going through that instruction, you must have gone through a course of practical chemistry, no doubt ?—Yes.

2112. And I presume that you must have derived the benefit from that study which everybody has done who studies chemistry, that is to say, you must be aware that your knowledge of chemistry is on a totally different footing when you have gone over the facts yourself from what it was before you knew them otherwise than by books ?—That is exactly what I have often said, that I considered that physiology could no more be taught without experimental illustrations than chemistry could.

2113. It is somewhat difficult to convey to persons who have not actually been engaged in the study of science, the intensity and the reality of the difference between knowing anything of your own knowledge because you have seen it, and knowing it from being told ?—When it is seen, it is so impressed upon the mind, that persons have a good foundation to work upon; without that foundation, they would often be at a loss just at the time when the knowledge was required.

2114. It has been replied to that argument that a surgical student is not allowed to operate on living bodies; but I take it that you would agree with me that the difference between a limb in a state of life and one in a state of death, so far as it is the subject of operation, is a difference of a totally different character from that which exists between a dead organ and a living organ in action ?—Yes.

2115. Whereas a man by studying anatomy may have a perfect conception of all the parts of a limb brought to him to operate upon, no amount of the study of anatomy can give him a notion of the action of the living parts ?—I agree in that.

2116. Then I should particularly like to have your opinion on one other point as a practical experimenter, and I ask the question without the slightest desire in any way to diminish the horror felt at the unnecessary infliction of pain, but rather with the desire of removing what is, to a certain extent, a prejudice. The question I put is this: Is it not within your experience that operations which, when described, sound exceedingly horrible, may, in reality, be performed with the infliction of what is really a small amount of pain. For example, I will take a particular case. It sounds a very horrible thing to say that a vessel of a live dog should be opened, and that a tube should be passed down it, and a certain quantity of blood taken therefrom; that sounds very horrible, but is it not a matter of fact that a dog will suffer that operation with less apparent manifestation of pain than he would exhibit if you pinched his tail pretty hard ?—Unquestionably, because the parts internally are not sensitive. And I would say further that I have frequently been unable to recognize the fact by the description. The description which has been given of an experiment has been so horrible, and likewise, I may say, so exaggerated, that I have not been able to associate the real fact with the description.

2117. There is another point which I should like to put to you. We have been told here that a vast number of what were called wanton and useless experiments are made by scientific investigators, experiments for no purpose, or no assignable purpose. Now you are familiar, I dare say, with scientific investigation, and the sort of pressure upon men's time which it involves. Are you aware that anything of that kind ever takes place ?—I am not.

2118. Do you think it likely that under the conditions of scientific life it should take place ?—I do not. Would you allow me to add that a great deal of the experimental part of our course consists of chemical and physical experiments. We illustrate the phenomena of life by chemical and physical experiments. A number of these experiments are introduced in relation to the circulation, respiration, and digestion; we imitate as far as we can the processes of life, and we consider that as belonging to the experimental part of the course.

2119. You are the author of a valuable work upon dietetics, I think ?—I have written a book upon dietetics.

2120. Have you been aided in the views you there express by your experiments upon animals ?—I think materially.

2121. (*To Dr. Pye-Smith.*) May I ask if you concur in the answers which Dr. Pavy has given to my questions ?—Yes.

2122. (*Mr. Forster to Dr. Pavy.*) You stated, in answer to Professor Huxley, that you agreed with him that the difference was very great in the advantage of teaching, which enabled a student to see an experiment from merely describing it to him. I, not knowing anything about medicine, should be much obliged to you if you could describe to me in what way there is a very great advantage to the medical student in his future profession from having had that kind of experimental teaching ?—The practice of medicine, one may say, is founded upon physiology. Unless we know the functions of the organs of the body in a state of health, we cannot be expected to realise what may be the condition in a state of disease. And I consider that by giving an experimental course, the knowledge of the functions of life is made more perfect than it otherwise could be.

2123. Could you illustrate that by any practical case which would show in what way a physician or a surgeon would be better able to practice the healing art by having seen an experiment ?—That may be taken generally and specially. I would say generally that our knowledge of disease must be based upon knowledge in a state of health. For instance, take the spinal cord; the symptoms which are produced by disease of the spinal cord are manifested at a distance from it. Now how can a person appreciate and give a proper interpretation to these symptoms which are manifested at a distance unless he knows what must be the functions of the spinal cord in a healthy state ? Before you can know anything of diseased

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action, you must be acquainted with what is healthy action.

2124. But would he not obtain that knowledge by walking the hospitals, and seeing the effect of the disease upon the spinal cord?—But supposing he did simply that, he would profit by the information which had been gained by physiology. The teaching is dependent upon the knowledge which has been acquired, and this knowledge has been acquired through experimental physiology.

2125. Can you give me a case in which a man could be called in by a patient, and in which from the fact that he has not had that knowledge, he would be less likely to do the patient good?—If he has simply got his knowledge from books or from lectures, the knowledge would not be impressed upon his mind in the same manner as if he got it from seeing the experiment performed. A man called in to treat a patient in the one case would have his knowledge at his command, in the other case it is possible the knowledge which he had acquired at lectures and likewise by reading would have vanished.

2126. Then, in fact, it comes to this, that you believe that in all cases the knowledge gained by sight is very much stronger than the knowledge gained by reading or by hearing?—I think the combination of the senses is needed. They hear, they read, and they see the experiment itself; and going through those channels, the information is much more likely to be indelibly impressed upon the mind.

2127. (*Lord Winmarleigh.*) May I ask you whether all the experiments that you have made at Guy's Hospital have had relation to benefits to be derived by the human frame from them?—I have always had that in view.

2128. Never performed such experiments simply from purely physiological objects?—It is physiology which is at the foundation of the treatment of disease.

2129. (*Mr. Forster.*) Do you lay this great stress upon the student's seeing the process upon this ground, that by seeing it, the knowledge that he gains by reading and hearing is very much deepened and confirmed, that is, the sensation upon him is much greater; or do you also think that he does, in seeing the experiments, get some information that he would not otherwise obtain by hearing and reading?—I do unquestionably. I think he is placed in an altered position. I think that by seeing an experiment performed, he is placed altogether in an altered position from what he would be in if he had not seen that experiment performed. He has got a foundation or substratum, as it were, which he could not otherwise have obtained.

2130. (*Mr. Huxley.*) Let me suggest another illustration of your view in the teaching of medicine. I presume that there is no teaching of medicine recognized unless there is a hospital attached?—No.

2131. That is to say, nobody would for an instant suppose that medicine could be taught to any useful purpose unless the student, not only had the opportunity of hearing disease described, but could go and see it in the hospital himself. That is a parallel case, is it not?—It is.

2132. (*Mr. Hutton.*) Of course you do not mean to deny that many good physiologists have been educated without this kind of education?—I should look upon them as physiologists occupying the same position as a physician who had not had practical experience.

2133. The argument is one of degree is it not? In point of fact your argument goes to this, does it not, That you ought to see some sort of experiment on living human beings in order to get still greater knowledge?—Certainly not.

2134. You would not deny, would you, that the knowledge would be larger which would be got by seeing the experiments on human beings?—I do not think it would.

2135. Surely the physiology is not the same of human beings and of animals?—There is no essential difference in fundamental points.

2136. The points at which the physiology of man branches off must be very numerous, must they not?—If you take the great functions, circulation, respiration, and digestion, those functions are carried on in the same way in the lower animals as they are in the human subject.

2137. And you think that no fresh instruction would be derived from seeing the same kind of experiments on men in any case?—Certainly that is my opinion; it would be quite superfluous, not at all required. As regards mental action there would be a difference certainly; but as regards the physical functions of life, they are performed in the same way in the lower animals as they are in the human subject.

2138. (*Mr. Huxley.*) That is to say, supposing we leave the phenomena of consciousness aside altogether, you could demonstrate all the truths laid down in a handbook of physiology upon a dog, could you not?—Quite so.

2139. (*Mr. Hutton*). The physiology of the brain is not the same, is it, in the dog and the human being?—As regards the elementary functions it is; the higher functions are different.

2140. Did the description which you gave just now of your experiments apply simply to the lecture room or to the laboratory also?—I perform two sets of experiments, those which are directed towards instruction, and those which are directed towards research.

2141. Did what you said just now refer to the experiments directed towards research?—The experiments directed towards instruction are invariably performed, whenever there is any cutting operation concerned, under the influence of an anæsthetic, and the experiments directed towards research are also almost invariably performed under the same state. I will not say that there may not have been an exception, because in some instances I have thought that the chloroform may have interfered with the conditions to be observed, and in order to see whether that has been the case or not, the experiment has been performed without the chloroform.

2142. But it is usually employed in your laboratory as much almost as in the lecture room, is it?—We never think of experimenting upon an animal without the chloroform; it must be something very special indeed to lead us to do so.

2143. Have you ever made experiments on the effect of strychnine on animals?—Yes.

2144. And you have proved, have you not, that death under strychnine generally takes place from the ceasing of respiration?—It does.

2145. In that case were you able to use chloroform?—Yes.

2146. The chloroform did not in that case interfere with the success of the experiment?—No. There is an experiment which I perform at lecture in which the animal is placed under the influence of chloroform, and some strychnine is introduced into the circulation whilst the animal is under the influence of the chloroform; and whilst the animal is still under the influence of the chloroform it dies immediately.

2147. But if you keep up artificial respiration it does not die?—But that is not done.

2148. But you have done such experiments yourself in the laboratory?—Yes, I think I have. I was performing some experiments some years ago upon woorali and strychnine.

2149. And under those circumstances the animal would suffer, would it not, because you could not keep up the anæsthesia?—I think the animal under the influence of the poison would not be in a position to feel pain in the same way as if the poison had not been used.

2150. You said that that handbook of physiology which has been referred to was used for accomplished physiologists. Now the very first line I read in it, "This book is intended for beginners in physiological work." Does not that imply that the editors

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did not take the same view as you take of it?—Unquestionably; but I do not believe that the book is used by any beginners; I do not see how it can be used by beginners.

2151. It was supposed by the editors that it would be used by beginners?—But persons are not always right in their suppositions.

2152. You say, as I understand you, that the classes of experiments recommended in that book are almost always painless. Will you kindly look at that contained at page 403, on recurrent sensibility, and on that at page 409, where the frog is gradually raised to 30 or 40 degrees centigrade in water; must not both of those experiments be very painful?—I did not say that they were painless experiments. The first of those experiments, to begin with, is performed under the influence of chloroform.

2153. But you will see that the roots are stimulated, and that the experiment depends upon the pain in the after part of it?—Yes; but all I can say is that that experiment would not be performed at Guy's.

2154. But still the effect of that handbook would be to suggest to a physiological lecturer that such an experiment ought to be performed?—But I am not responsible for the suggestions which are made in that handbook.

2155. But I understood you to say that you did not believe the method of vivisectional experiments to be extending in this country, or to be likely to extend. Surely this is a symptom that it is extending, and likely to extend?—Nor do I think that it is being extended by this book which has been written.

2156. Do you not suppose that all those experiments had been tried by Dr. Burdon Sanderson or the other editors of that book before they published it, and that they usually practice them in their laboratories?—Very likely the majority have been tried, but I should say not all; and, as I mentioned just now, the greater portion of the evidence given in that book is from continental sources.

2157. (*Chairman.*) The experiment to which your attention has been particularly directed is one which you would not think it proper to introduce at Guy's?—I feel sure that the students would not tolerate it.

2158. (*Mr. Hutton.*) Do you mean that you would not introduce either the one or the other. Those two are only specimens of a very large number of experiments, and I wanted to know whether you thought that the effect of that handbook would not be to recommend such experiments to our English lecturers. One of these experiments is on two frogs; a healthy frog which makes an effort to escape; and another frog which, as I suppose, is pithed?—Yes, it is pithed.

2159. But I suppose the healthy frog suffers very much the experience of being put into boiling water?—I should think not. The temperature is gradually raised, and before the frog could experience the effect which would be experienced by being put into boiling water, the sensibility would become altogether destroyed.

2160. I suppose the frog would feel very much what we should feel if we were to be put into water and the temperature gradually raised to the boiling point?—I think we should not feel any pain. I think that when the temperature got to 110° or 112° the condition of the body would be so altered that pain would not be experienced. I should explain that there is a difference between being in water at 110° and in the atmosphere at 110°. The water would have the effect of raising the temperature of the body, and when the temperature of the body reaches about 10 to 12 degrees above the normal point death ensues; so that death would ensue long before the animal could be subjected to the influence of the boiling water.

2161. (*Mr. Forster.*) Would it not be the case that either you or I would suffer very much more pain if we were suddenly to put our bodies into boiling water than we should if we put them into cold water, and then the temperature was gradually raised?—I do not think that in the latter case pain would be experienced; death would be produced by the destruction of the vital capacity of the tissues long before the boiling point was attained.

2162. (*Mr. Hutton.*) The evidence is that the frog made great efforts to escape; which I suppose means that the frog suffered a good deal?—I do not know that you must interpret it in that way. There may be a physical action on the muscles causing those efforts.

2163. All I wish to obtain from you is that you do not think the experiments in this handbook are experiments which you would think it desirable should be adopted in the physiological schools of this country?—Certainly, I would say so.

2164. (*Lord Winmarleigh.*) Have you gone through the whole book?—The greater portion of it; but taking the book altogether, I think there are a great many experiments in it not adapted for repeating.

2165. (*Mr. Huxley.*) You mean by that not adapted for physiological laboratories meant for teaching ordinary medical students?—Yes.

2166. But if they were physiological laboratories for the education of physiological students proper, would you see any objection to the experiments?—They should be prepared with due care and caution.

2167. (*Mr. Hutton.*) But even the most painful of those experiments, those purely demonstrative, you would think would be right for the purely physiological laboratories, would you?—Where a person was prosecuting physiology so that he might make some advance which might be of benefit to mankind, I should consider that he would be justified in resorting to measures which would not be justifiable in the case of ordinary students.

2168. But as to both of those experiments to which I have been calling your attention, they are not experiments essential for research, but experiments desirable for the purpose of teaching students in the one case the action of the nerves in relation to what is called recurrent sensibility, and in the other case the reflex-action of the frog's nerves; they are not experiments in the course of new research, but experiments for the purposes of teaching?—Yes; but because they are recommended in that handbook it does not follow that we should repeat them.

2169. I only want to get your repudiation of them as experiments for the purpose of teaching?—I should not consider myself bound to do anything but what I considered conscientiously right.

2170. (*To Dr. Pye-Smith.*) Do you agree with Dr. Pavy's answers on this subject?—Yes, as far as I follow your questions.

2171. Do you think that for the purposes of teaching, these painful experiments are desirable, even with a class of advanced physiologists?—I should say that is a question which must be left to the judgment and the conscience of each teacher.

2172. In fact you would not disallow them?—I would not disallow them by law so long as I had confidence in the teacher.

2173. (*Chairman.*) But we have understood from you that in other countries things do go on which you do not approve?—I have reason to believe so.

2174. And you have reason to believe that this handbook before us is borrowed chiefly from continental sources?—Yes.

2175. And you do not associate your reputation with these experiments which are recommended in this handbook, but on the contrary you withhold that association very clearly, on the ground that they are not suited, at least for the purposes of Guy's?—I should draw a very great distinction between the amount of physiology which it is necessary to teach those who intend to practise medicine, and the amount which it is necessary to teach those who are themselves to teach and to carry on the study of physiology. This handbook is intended for both, but much more for the latter class, and for the latter class, while I would not bind myself to every individual sentence there, I confess that I think there is nothing there that is not perfectly justifiable for that purpose.

2176. You say that you would trust the conscience

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of the person who is to perform the experiment, but suppose there are persons whose consciences are not trustworthy, would you think it unreasonable that there should be some security that improper things of this sort were not done?—I would venture to suggest that that is a question for experience. Where any wrong or abuse is shown, I think every physiologist would wish that it should be stopped, if necessary, by law, where public opinion among physiologists is not sufficient, which it is at present. I know personally each of the gentlemen who wrote this book, and I should feel at least as much confidence in their judgment and their humanity as I should in my own.

2177. (*Lord Winmarleigh to Dr. Pavy.*) You said that there are some cases in which it is impossible to make these experiments under chloroform, but I think you said they were few?—Very few indeed.

2178. Would it be possible to draw a distinct line between the cases in which chloroform cannot be used and those in which it can be used?—That must be left to the physiologists; there can be no distinct line drawn, I think.

2179. Supposing, for instance, it was proposed to legitimatise experiments under chloroform, there is no possibility of making an exception as to certain cases which cannot be performed under chloroform?—I consider that no line can be drawn, simply because you cannot foresee the kind of experiments which are likely to be required. As knowledge advances we experiment in a new line, and you cannot foresee what line of experiment might be required; so that by legislation I do not understand that any hard line could be drawn.

2180. Speaking of what has prevailed up to the present time, what are the particular cases in which you cannot use chloroform or anæsthetics, and which you allow to inflict considerable pain upon the animal operated upon?—There are none at the present time, but I was referring to what I had done, for instance, in experiments relating to the blood. I had reason to think that chloroform might have interfered with the experimental result which I obtained, and I felt it necessary to perform the experiment in the absence of chloroform, so as to see whether the experiment had been vitiated by the chloroform or not.

2181. But I think you stated that that could be done without pain to the animal?—Chloroform may be administered first, and the cutting operation performed, and the animal allowed to come round from the influence of the chloroform, and the blood subsequently collected and examined.

2182. Can all the experiments with reference to the liver and so on, all without exception, be performed under anæsthetics?—I do not remember one that cannot at the present time.

2183. Can the experiments relating to the nerves connected with the brain, and so on?—With regard to the nerves, with some of those experiments it would be necessary that the animal should not be under the influence of chloroform.

2184. And would those be very painful?—I do not consider that any material amount of pain need be inflicted, because all that would be required would be to just see the result, and then no unnecessary prolongation of suffering need be produced.

2185. (*Chairman.*) I understood you to say that a bill framed generally upon the basis of Dr. Playfair's bill would not be restrictive of anything which is now done by physiologists in this country?—I felt, on looking through that bill, that it would not restrict the course which I was myself pursuing.

2186. (*To Dr. Pye-Smith.*) Is that your opinion also?—Yes. I think it might possibly to some extent throw difficulties in the way, but not seriously. But it would, I think, throw a kind of stigma over physiological experiments.

2187. Then it is merely a sentiment, that it would involve what you consider to be an undeserved stigma upon the profession in this country?—Yes.

2188. But it would not you think, interfere with anything which those whom you are acquainted with, or whom you believe to be the principal physiologists in this country, really practice?—No.

2189. It would therefore be operative, if at all, against other persons outside those whom you have in your contemplation?—Yes.

2190. Then if it should so happen that there was sufficient reason to think that there either are now or may hereafter be persons of a different description from those whom you are acquainted with, you would not see any very serious difficulty in the operation of the bill?—Certainly not.

2191. (*Mr. Forster to Dr. Pavy.*) I understand by your answers, that you would see no objection to the prevention of all experiments for merely demonstrative or teaching purposes in which anæsthetics are not used?—Anæsthetics used at the commencement or during the operative part of the experiment.

2192. Anæsthetics used in the same manner as they are in your experiments at Guy's Hospital?—Yes.

2193. (*Mr. Erichsen.*) You said that the experiment on recurrent sensibility would not be tolerated by the students at Guy's Hospital?—Yes.

2194. I suppose we may take it that you mean that the students at Guy's Hospital are a fair average sample of the students at all well conducted schools of medicine in this country?—I think so.

2195. And if a large body of gentlemen like the students at Guy's Hospital would not tolerate that experiment, it is also to be supposed that large bodies of students in other schools would have the same repugnance to it which would be manifested at your school?—I think so.

2196. And consequently that in all probability no teacher of physiology would demonstrate such an experiment as that to his class?—Not to his class.

2197. You have been in the habit of demonstrating more largely I think than most teachers of physiology, for a longer period, at all events for the last 20 years, before very large classes, and having been a teacher for so lengthened a period as that, you must have had an opportunity of seeing and judging of the progress of the men that you have taught in after life?—Yes.

2198. Have you found that the practice of attending demonstrations of physiological experiments or in physiological laboratories where experiments are made, tends to harden the heart or to alter the character injuriously of those who study in that way?—I do not think so.

2199. You do not think so from your experience during that lengthened period, and having been able to trace the progress of these men after their establishment in practice?—I think certainly not.

2200. Besides those experiments which are demonstrative experiments which you have been in the habit of performing, there are a series of observations which may be submitted with great advantage to students, I presume, in your opinion, which are not exactly experiments, but by which certain actions going on in the living body are visible and can be shown; such, for instance, as the action of the heart in an animal which is pithed, but in which artificial respiration keeps up that action?—Yes.

2201. Now do you think it could be possible for anybody who has not seen the contractions of the heart and of the different chambers of the heart under those circumstances to form a correct estimate really of the action of a living heart?—I think not. As I mentioned in answer to a question previously, I think that a person who has seen these things is placed in a different position from a person who has not seen them.

2202. And that the physician who has seen such a thing as that would be better capable in his practice of judging of the morbid action of a living heart, from the fact of his having seen the healthy action in a living animal?—I consider unquestionably so.

2203. Then again with regard to such an observation, which may be made in the same way, of the

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peristaltic action of the intestines, in examining that, do you think that a surgeon who has seen that movement would be able to form a better conception of such a disease as intus-susception than a man who has never seen it?—I think that seeing the peristaltic action of the intestines gives an idea which would be of incalculable service to a surgeon after.

2204. (*Lord Winmarleigh.*) He would not derive the same knowledge from theory, you think?—I do not see how he can have a correct conception without seeing it.

2205. You have never found any practical difficulty in keeping animals under anæsthesia sufficiently long for all purposes of demonstration as well as of experiments out of the class room?—Not the slightest. An animal may be easily kept under the influence of chloroform for three hours. I have done this myself within the last two months.

The witnesses withdrew.

Adjourned to Saturday next at 2 o'clock.

Saturday, 23rd October 1875.

PRESENT:

THE RIGHT HON. VISCOUNT CARDWELL, IN THE CHAIR.

The Right Hon. LORD WINMARLEIGH.
The Right Hon. W. E. FORSTER, M.P.
Sir J. B. KARSLAKE, M.P.
THOMAS HENRY HUXLEY, Esq.
JOHN ERIC ERICHSEN, Esq.
RICHARD HOLT HUTTON, Esq.

N. BAKER, Esq., Secretary.

Mr. J. BURDON-SANDERSON, M.D., F.R.S., and Mr. MICHAEL FOSTER, M.D., F.R.S., called in and examined.

Mr. J. Burdon-Sanderson, M.D., F.R.S., and Mr. M. Foster, M.D., F.R.S.

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2206. (*Chairman to Dr. Burdon-Sanderson.*) You are Professor of Human Physiology in University College, London?—Yes.

2207. But you represent the London University in your professional capacity, do you not?—I am the Superintendent of the Brown Institution, which is immediately under the London University.

2208. (*To Dr. Foster.*) You are Prælector of Physiology in Trinity College, Cambridge?—Yes.

2209. (*To Dr. Burdon-Sanderson.*) You are the author of one part of the "Handbook for the Physiological Laboratory?"—I am.

2210. (*To Dr. Foster.*) And you are the author of another part?—Yes.

2211. (*To Dr. Burdon-Sanderson.*) We were told the other day that this book had been very much founded upon experiments conducted in foreign countries; is that so?—The whole subject of physiology is founded to the extent of about perhaps nine parts out of ten upon experiments conducted in foreign countries, because physiology has been very much neglected in England during the past 20 years.

2212. Then you consider, do you, that physiology is now taking what may be considered rather a new start in England?—I hardly think that. It is to a certain extent taking a new start, but it has not yet taken any very decidedly new start. The science has progressed very considerably, as I have already said, in all countries during the last 25 years; and all that has been done lately is to organize schools of physiology in a more complete manner than they were organized before.

2213. In this country you mean?—In this country.

2214. This book is prepared with a view to a more general study of physiology in this country than we have had heretofore, is that so?—I should rather answer that question thus: it has been prepared for the purpose of enabling those who are engaged in the experimental study of physiology to do it in the most effectual way.

2215. In short, to give what I said before, a new start to a science which you said has hitherto been very much neglected in this country?—Exactly.

2216. You are acquainted with the practice of the foreign schools upon this subject, so far at least as it may be known from the published medical journals?—I am.

2217. We have understood from many very eminent witnesses who have been before us, that there are many things done on the continent in regard to this subject which would not be approved if they were introduced here either by the medical teachers or by the medical pupils, is that your opinion?—It is rather a dangerous question to answer. Investigations in physiology are conducted abroad by men of very great eminence; at no previous period in the history of the science have so many great men lived as at the present moment; and to speak of such men in terms of criticism would be perhaps rather bold, but there are certainly things done in connection with teaching abroad, and even in connection with research, which ought not to be done on humanitarian grounds, if I may be excused for using the word.

2218. If therefore the Legislature and the British public should think it desirable to prevent the possible introduction of similar practices into this country, you would not be prepared to dissent from that desire on their part?—It would depend entirely upon the way in which it was done.

2219. I am assuming that it was done in a reasonable and sensible manner; but I am now speaking of the object to be attained, you would not dissent from the object which the Legislature in the supposed case would have in view?—Certainly not.

2220. I think you were acquainted with Dr. Playfair's bill at the time that he introduced it into the House of Commons?—I was, and took an active part in regard to it. I had some conferences with Dr. Playfair on the subject, and with other gentlemen; and in the main I approved of it, and thought it was a suitable measure.

2221. Now in the general sentiment which has been expressed here by so many of the most eminent medical men, that the giving pain to animals is a great evil, and should, as far as possible, be avoided, I am sure I may take you as entirely agreeing?—Entirely.

2222. So that if an experiment could be tried with complete anæsthesia, and complete anæsthesia were not employed, you would condemn it very much?—I should condemn the non-employment of anæsthesia.

2223. If an experiment was in your opinion absolutely necessary for the attaining of some point of physiological knowledge, and it was a painful experiment, an unnecessary repetition of that would be equally foreign to any approval of yours, I presume?—I should be very cautious in finding fault with anybody for repeating an experiment; because all facts in science require repeating in order to make them certain; and of course it is extremely difficult to judge how often an observation must be repeated. It must be repeated a sufficient number of times for us to be sure that no error exists, because if a fact goes forth which

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has not been made sufficiently sure by exactness of observation, without referring to the scientific evils which would arise therefrom, it would lead to the making of additional experiments for the purpose of making it certain.

2224. That you consider is frequently a very difficult point to determine ?—A very difficult point.

2225. One which ought to be determined, if at all, by the most competent and most scientific persons ?—I am strongly of that opinion.

2226. And ought therefore not to be left to the judgment of every person who might imagine himself to be a competent person, when perhaps nobody else would be likely to be of the same opinion ?—No doubt.

2227. As far as your knowledge and information go, the practice in this country is different from that of which we have spoken as known to prevail in some other countries, is it not ?—The sentiment is quite different among physiological workers, and consequently the practice is different.

2228. The sentiment in this country has been thus in your opinion quite correctly described to us as being a sentiment that pain is a great evil, that it should be avoided where possible, and, where it is not possible to avoid it, should be minimized ?—Certainly.

2229. And that the utmost care should be taken to accomplish that in every experiment that is tried ?—Certainly.

2230. We were told by the teachers of one of the most eminent schools in London, that in all demonstrations at their school complete anæsthesia is resorted to. Is it your belief that that is the case generally in all the schools in this country ?—I can only say what my own practice is. I always have been in the habit at the beginning of my course, long before this present agitation commenced, of explaining to students that in all experiments which were used for demonstrational purposes, the animals are anæsthetised.

2231. So that an entire absence of pain may be predicated so far as demonstration to pupils is concerned ?—Certainly, in my school it is so. There is one observation I should like to make, namely, that outside of the ordinary teaching of physiology comes a subject which belongs to physiology, but would not be ordinarily included in it, namely, the subject of pharmacology, including toxicology. In this case if anything is to be shown to students (and I think it is most desirable that the actions of some of the most important poisons should be shown), the experiments would necessarily be attended with pain. It does not come within my scope to make such experiments before students, or to show them ; but I should not think that a teacher of pharmacology was not justified in exhibiting the effects of some of the most important poisons, although that might be attended with pain.

2232. And although the action of the poison was perfectly well known, and therefore no new scientific information would be obtained by that particular experiment ?—I mean that. I refer to certain poisons of which the action is extremely characteristic, which kill the animal used, but cannot be said to kill the animal without pain ; you cannot call it a case of euthanasia ; it is a case in which death does not take place in so short a way as it might be produced ; but it does not strictly come within the definition given of experiments made to illustrate the subject of physiology.

2233. Would that remark apply to cases such as have been mentioned here, of animals having been starved to death, or nearly to death, in order to show pupils that a very small injury will terminate life in an animal so reduced ?—As to such an experiment as that it would be of course very objectionable to make it for demonstrational purposes, and it would be altogether uninstructive. I was alluding for example to such a poison as strychnia, as to the action of which no one can form a vivid conception unless he has seen an animal under its influence.

2234. The point which I want to have as clearly as we can get it on the notes is, whether the experiments in toxicology, which you think ought not be excluded from experiments of mere demonstration to pupils, are of a very severe and protracted nature, or whether they are of a less severe and protracted nature ?—They are all of them very short indeed as regards the time during which the suffering is inflicted.

2235. So that what you contemplate as regards the demonstration to pupils is, as regards the use of the knife, a complete absence of pain ?—Yes.

2236. And as regards the use of poisons, only a comparatively slight suffering of pain ?—Yes.

2237. And in very rare cases ?—In very rare cases. There again I refer to the purpose of teaching.

2238. I am asking you now with regard to poisons entirely in regard to demonstration to pupils ?—Yes, I understand the question in that way.

2239. Now in the book to which we have already referred, I observe in the preface that it is said to be a book "intended for beginners ;" but I do not understand that you mean that pupils, or any but advanced students, are to practice any of these experiments ?—The book is intended as a guide for persons who are engaging in scientific investigation. It is not intended at all for students in the ordinary sense of the word, as meaning students of medicine. It is intended for students of physiology, and not for students of medicine. I may illustrate that by saying that at the beginning of each of my courses of physiology, when, as is usually the case, one points out to students the books that they ought to purchase, I always tell them, "You know the handbook for the physiological laboratory, but that is not a book that I recommend "you to buy, because that is not intended for the ordinary student to provide himself with; that was "written for the purpose of helping persons who are "engaged in physiological study, and want to make "either physiological, pharmacological, or toxicological "investigations, requiring a knowledge of what has "been done before, and the way in which it has "been done."

2240. So that to interpret the words "intended for beginners" as meaning that anybody who goes as a medical student to one of the great hospitals is to read this handbook, and apply it for himself, would be entirely to misapprehend what I understand you to say is your meaning ?—Entirely, it means beginners in research. It would be correct if you were to introduce the two words which I have now given after the word "beginners," namely, "in research."

2241. The word "beginners" then means, as I gather, people who have already attained very considerable surgical skill ?—Not surgical skill, because surgery has no necessary relation to physiology, but considerable knowledge in physiology.

2242. By surgical skill I mean that if they are to apply the methods suggested in this book, they should be people competent to apply them with the utmost skill that anybody could be expected to possess in such an occupation ?—Quite so. And I think this gives me an opportunity of saying that the state of things which we should like to see established with reference to physiological research is such as would unquestionably discourage the making of experiments by anyone, excepting by persons trained in what I should call a school of physiology ; and by a school of physiology I do not mean a system of lectures, but a place where research is carried on under the supervision of skilled persons, such persons being assisted by persons less skilled, who would in that way of course have the opportunity of acquiring the skill of which you speak.

2243. I want for the present to ascertain whether any precautions which may be indicated in the book for the purpose of either preventing or diminishing suffering would be in the hands of beginners, who might be bunglers, or would be in the hands of persons whose previous teaching had made them competent to use such precautions with the greatest proficiency ?—It is our intention that the experiments referred to in the book should only be made by com-

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petent persons, and that the accounts of them should be used to assist persons who are competent, and who have made themselves competent in the best possible way, to make experiments.

2244. Now I should like to ask you a few questions about some of the experiments in the book, in order that you may illustrate more fully what you have already said. Now I have before me the experiment on page 235, No. 47, that is called "Experiments "relating to the influence of the cerebro-spinal "nervous centres of the vascular system," and the first heading is "Destruction of the nervous centres"; and it begins—"Two frogs are slightly curarised." Is curari an anæsthetic?—The action of curari is now under investigation. It is a very complicated question indeed to enter upon, and it would take a long time to discuss. There is every reason to believe, as regards the frog, and the animals belonging to that group, that we have got pretty exact knowledge with reference to it. As regards the higher animals we have always treated it as if it were not an anæsthetic; that is to say, we have always considered it necessary (in experiments of research I am talking about now) when any painful operation had to be done, to use anæsthetics with a view to such painful operation.

2245. And in that case you would not consider curari an anæsthetic?—Certainly not, so far as regards the higher animals, so far as we know. I mean we act as if we did not consider it so.

2246. Do you draw a distinction between the frog and the higher animals, and hold that curari is an anæsthetic in regard to the frog?—So far as the thing has been investigated, everything tends to show that it is.

2247. Are you acquainted with the recorded opinion of Claude Bernard upon the subject of whether curari is an anæsthetic or not?—Claude Bernard was the first who investigated the subject of curari.

2248. And we have been given to understand that it is on record as his decision on the subject, that curari is not an anæsthetic; and I had not heard before that there is a distinction between the frog and the higher animals in this respect. Do you hold that it is established that there is that distinction?—It is not at all established that there is a distinction in that matter between the frog and the higher animals, because we do not know the action of curari on the higher animals, but we do know it in a great measure with reference to the frog.

2249. But we have been given to understand by some of the evidence before us that curari operates upon the motor nerves and does not operate upon the nerves of sensation. Do you hold that to be the right opinion?—There is a very marked difference between the action of curari on the motor and the sensory nerves.

2250. So that on the sensory nerves it is not at any rate ascertained that it operates so as to produce anæsthesia?—With reference to the frog we have not the slightest evidence that the frog feels any pain under the influence of curari.

2251. But I understand you to say with regard to the higher animals that you have so little faith in curari that you always think it necessary to use some other anæsthetic?—We do so. I am speaking of my own practice, what I should do myself, and what I have done.

2252. But I am speaking of this handbook which is intended, not for your own practice, but for the guidance of those who are to profit by it, and I have adverted to this particular experiment which I presume I may take to be a very painful experiment if it were performed without any anæsthetic?—I think with regard to the frog, that a frog under the influence of curari is to the best of our knowledge in a condition in which the consciousness of pain is extremely small, that is all I can say about it.

2253. But this is in its nature, is it not, a painful experiment, supposing there to be no anæsthetic used?—It cannot be a very painful experiment because the injury done is relatively slight. This is a case in which the heart of a frog is exposed, and that is the only operation that is performed in that particular case.

2254. Take the next one, No. 48, at page 237. "All "being now ready the integument is opened along the "middle line of the back of the neck and the occipital "bone perforated in the middle line with a fine awl, "close to its posterior margin. The frog is then "laid, back downwards, on the board, in such a posi"tion that one of the needles enters the cranium "through the hole in the occipital bone, the other "the spinal canal. The web is then laid on a plate "of glass, which covers the notch, and secured if "necessary by fine pins. Finally the heart is exposed "as before," and so on. Now when you speak of a painful experiment would you include that in the category of painful experiments?—The source of pain, if anything, would be the introduction of the pins which are mentioned here, because those pass through the integument of the frog; but of course I need not say that the amount of pain produced would be extremely small.

2255. In the whole experiment do you mean?—Extremely small, supposing the animal were not curarised I mean.

2256. Then whatever the pain may be in such an experiment as that you think curari is a sufficient anæsthetic to employ?—Certainly, it would be most absurd to use any other, absolutely absurd (I can speak of that afterwards), bearing in mind that these are experiments of research.

2257. (*Lord Winmarleigh.*) With a particular object?—With a particular object.

2258. (*Chairman.*) I am now speaking entirely of whether it is very painful, and whether curari is an anæsthetic, not with respect to the importance of the object; and I understand you to say with regard to that experiment that it is not very painful, and that curari is so sufficient an anæsthetic, that it would be absurd to employ any other?—I used that expression "absurd to employ any other" because any other anæsthetic which you could employ would not be beneficial to the animal itself, even looking at it in that point of view.

2259. Now on page 269, Experiment No. 74, is this: "A frog having been slightly curarised or "rendered motionless by section of the medulla, is "fixed in the prone position. The sternum is then "divided in the middle line, and the two halves of "the wall of the cesht drawn to either side, so as to "expose the pericardium and lungs, while a stout "glass rod is passed down the æsophagus." Do you consider that a painful experiment?—I may remark with reference to this experiment, that if the experiment is properly done, it is one which can be done without pain at all; because it is one which can be done after the destruction of the brain. I am sorry to see that that is not so distinctly stated in the book as it might have been.

2260. Supposing the brain to have been destroyed, would it then be necessary to curarise the frog for this purpose?—Not necessarily to show the facts which are referred to there.

2261. So that it is rather an oversight to say that the frog may be "slightly curarised;" it should have been said that the sensation of the frog should have been destroyed?—I think so.

2262. Now on page 278 I find this: "In a "curarised rabbit, in which artificial respiration is "maintained in the usual way, an incision is made in "the middle line, extending from the upper third of "the sternum to the upper end of the trachea. The "external jugular vein of one side is then brought "into view, tied in two places, and divided between "the ligatures," and so on. Now is that in its nature a painful experiment?—There is no doubt that it is a painful experiment.

2263. Now you seemed to suggest to us a little while ago that a frog differed from what we call the higher animals in respect of the operation of curari. How does a rabbit stand in regard to the operation of

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curari ?—The observation that I made just now applies to it ; we do not know what the action of curari on the higher animals is at all as regards the relation between sensibility and voluntary motion ; but we treat the case as if curari did not make the animal insensible. For all such operations as these, of which you have read the description, one would use another anæsthetic ; and we have at hand an anæsthetic for that purpose, namely, chloral in the case of a rabbit.

2264. (*Lord Winmarleigh.*) Not in the case of a frog ?—Not in the case of a frog.

2265. (*Chairman.*) It says here "in a curarised rabbit." Do I rightly understand that that ought to be "in an anæsthetised or narcotised rabbit"?—May I make a general observation in reference to this book, namely, that we had not in view the criticisms of people who did not belong to our craft in writing it, and that we did not guard against all possible misunderstandings of that sort. It is generally understood that we use anæsthetics whenever we possibly can, and consequently that is a thing taken for granted. That ought to have been stated much more distinctly at the beginning in a general way ; but it was not stated for the reason I have given.

2266. Then I may assume that in any future communications with the "beginners," for whom the book is intended, greater pains will be taken to make them distinctly understand how animals may be saved from suffering than have been taken in this book ?—Yes ; I am quite willing to say that.

2267. Sir John Karslake suggests to me this question : In the case of a rabbit, would not it have done quite well to say "in an anæsthetised or narcotised rabbit," or whatever the proper phrase is, instead of "a curarised rabbit"?—It would not have done, because the object in this particular experiment was to cut off one set of functions, namely, the functions of voluntary motion ; that was absolutely necessary for the purpose of the experiment. You cannot possibly make any experiment bearing upon the condition of pressure in the circulation in which you have the interference of muscular action, for instance, the rhythmical motions of respiration, inasmuch as each muscular act of respiration modifies the mechanism of circulation.

2268. I understand you that the special action of curari is to prohibit the action of the motor nerves ?—Yes ; the purpose being to simplify the conditions of the experiment by shutting off the greatest function, namely, the function of the muscular system.

2269. Then for this experiment it is essential that curari should be used ?—I must just refer to the precise nature of the experiment to point out why. This is one of the experiments in which curari must be used for the reason I have stated, namely, that the whole thing depends upon the conditions of circulation, and that the conditions of circulation are modified by the presence of muscular motions of any kind, and particularly those of respiration.

2270. Then for this experiment it is essential to use curari ?—It is essential to use curari, whether you use another anæsthetic or not.

2271. Then if it were properly explained to those who are to use the book, it would be understood that besides an anæsthetic, which would completely destroy the sense of pain, curari should be given to destroy the action of the motor nerves ?—Precisely.

2272. So that what is wanted is that the pupil should be told that the animal here is to be anæsthetised ; and you think that it was the intention of the book that that should be understood by everybody ?—By everybody doing any such experiments, certainly.

2273. Though I think you now see that it would be desirable that that explanation should be made a little more clearly in future ?—Yes.

2274. Now on page 308, at No. 101, I find this : "Death after section of both vagi. Rabbits in which "both vagi have been divided commonly die before "the end of the first day. Dogs live longer ; often "two or three days." Now is the section of the vagi a painful experiment ?—Yes.

2275. (*Lord Winmarleigh.*) What are the vagi ?—The vagi are two nerves which supply the organs of respiration and those of disgestion, and in fact preside over the most important functions of the internal organs.

2276. (*Chairman.*) Then, that experiment No. 101 is a painful experiment ?—It is a painful experiment, and one which would not be done excepting purely for the purpose of research.

2277. Then after the animal has been anæsthetised for the purpose of performing the section, there still remains to rabbits a portion of a day, and to dogs a much longer portion, often two or three days of a very painful state of existence ?—Yes. But may I draw attention to the fact that no one is recommended to do this experiment ; it is not mentioned as an experiment to be done by anybody. The paragraph is headed "Death after section of both vagi," and then an account is given of the phenomena which have been observed to result in different animals from that lesion.

2278. Then your opinion is that it is an experiment which ought not to be resorted to ?—Excepting for the purpose of investigation. It is an experiment which has yielded results of the very greatest importance, and consequently when it has been done previously, it has been done with the best justification.

2279. The result of that experiment has been so clearly established already that it is stated in this book with perfect precision, is it not ?—It is stated with a certain precision ; but it cannot be said that the result of it is fully known already, because there are many points connected with it which are not as yet known, and I cannot say that it is not even probable that it may be performed again, but no one would perform it for the purpose of seeing only the facts which are already recorded and already known. If it was done at all, it would be with reference to some of the facts which are not known.

2280. There is no very clear distinction drawn in the book, is there, as to whether it is an experiment that a beginner might very properly perform for himself or not ?—Well, I cannot say that that is distinctly stated ; for the reason I have already given, that I do not think that any well educated man would think of doing this experiment, excepting for the purpose of investigation, and certainly not for the purpose of merely seeing the facts which have been already ascertained. But let me observe that the amount of pain produced by that experiment is not in the least greater than the pain which occurs in an ordinary case of pneumonia, an ordinary case of inflammation of the lungs. The only serious painful result of the injury is that inflammation of the organs is produced ; and of course we know that that occurs over and over again to animals, and that they do not suffer any great amount of pain. You asked me at the beginning whether it was a painful experiment, and I said at once that it was a painful experiment, and one in which the inconvenience and suffering was prolonged, because in fact you produce in the lungs of the animal an illness of which it eventually dies. I wish to repeat that there are many things stated in this book which are not intended for repetition by anyone.

2281. (*Sir J. B. Karslake.*) Is there anything in the context to show for what purpose it will be necessary, if at all, to repeat that experiment ?—You will find stated in the text what are the physiological facts which have been derived from this experiment ; there is one of them, for example, which comes across me, which is stated here, but which is disputed. With reference to that fact, it being disputed, it is possible, or probable even, that it would be necessary to repeat that experiment again. The facts which are stated as results here are those facts which arise out of the experiment. There are some however which have been questioned recently, and with reference to them it is quite possible that it may be necessary to repeat the experiment.

2282. (*Chairman.*) The book would naturally lead, would it not, a large number of independent inquirers in different parts of the country to repeat for that

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purpose an experiment which was admitted to be painful, and certainly causing death in the rabbit in one day, and in the dog in two or three days, and in which anæsthesia could not be employed?—I cannot agree to that for this reason, that the question arises quite independently of the book. The book affords the best and most efficient way of answering it, and that is all that the book has to do with it.

2283. Now on page 320, No. 110, I find "Asphyxia by slow suffocation." Is it desirable that asphyxia by slow suffocation should be resorted to by independent inquirers generally?—Well the subject of asphyxia is one of course of fundamental importance; it has been investigated by experiments lasting over a considerable period from time to time. Now in consequence of the introduction of better methods of research, such experiments as have been made in the past would not be made again. I may allude, for example, to the experiments made by a committee of the Medical and Chirurgical Society, of which I was a member some 14 or 16 years ago. Those experiments were a sufficient number, but they led, as usually happens when committees assemble to make experiments, to very unsatisfactory results. Since that time we have come, by the use of the best methods, to understand the subject of asphyxia very completely. Of course it is not desirable to make experiments with a view of making more certain things which are already certain; but the only way in which one could bring together the various elementary facts which relate to the subject was by giving an account of it in this way, at all events it is the most ready way of doing it. I may answer the question by saying that it would be decidedly improper to repeat painful experiments already made in this or in any other case, for the purpose of determining a fact which had already been determined.

2284. I have taken those instances, rather selecting them as they occurred to me for the purpose of illustrating the view which you have already expressed, and I think you have every disposition to recognize the impression on my part as being well founded, that it would be better that the public should have what are your real views more clearly brought before them than they are already in the preface and other portions of that book; is not that so?—Quite so.

2285. Then coming back from instances to your opinions, you think that there is in this country a tone of sentiment towards the lower animals which would generally prevent wanton or unnecessary pain being inflicted upon them for scientific purposes?—Certainly.

2286. That for the purposes of demonstration to pupils, your own practice and the practice of Guy's Hospital, and so far as your knowledge goes the practice of the other principal schools, is to inflict no pain at all in experiments that are for the mere purpose of demonstration to pupils?—To the best of my knowledge, with the exception of Guy's Hospital and my own school, there is no school in London that teaches practical physiology by experiments.

2287. Then I may take it that in your opinion I have correctly stated what you believe to be the tone of feeling in this country?—Yes.

2288. (*Mr. Huxley.*) Is there no teaching of physiology at King's College?—I ought to have mentioned King's College; but the position at King's College has lately been vacant, and it was on that account that I omitted to think of it.

2289. (*Chairman.*) Then we may say that the only necessity for inflicting pain upon animals at all is where original research is the object?—Yes.

2290. That where original research is the object, it should be practised not by persons inexperienced and incompetent, but on the contrary by persons of the highest experience and competency?—Yes.

2291. That it is a legitimate object for the public and the Legislature to have in view, to secure, if necessary, that such shall be in future the practice in this country?—Yes.

2292. And to take care that some of the practices, which we have public evidence for knowing are resorted to in other countries, shall not be introduced into this country?—Yes.

2293. And for that purpose you are willing, speaking generally, to coincide in the proposals made by Dr. Playfair to the House of Commons?—Yes.

2294. Though, as I understand it, you reserve liberty to comment upon portions of Dr. Playfair's proposals, if you think fit?—Yes.

2295. But that in the main, to the general outline of Dr. Playfair's bill you may be taken to be a consenting party?—Yes.

2296. Now would you allow me to ask you in what respects may the study of physiology be expected to be useful for the advancement of practical medicine?—The utility of physiological study, it seems to me, ought to be judged of, not so much by its direct applicability to disease, as by the certainty which exists in our minds that eventually it will be the guide of practice in medicine; and I wish to emphasize this first statement particularly, not because I am insensible to the fact that there are a great many direct applications of physiology, but because I think that what ought to encourage us above all is that certainty of which I have spoken. And I wish to illustrate that by referring to some of the common diseases, and to some common remedies; for the reason why medicine fails is, that we do not know the nature of the diseases, and that we do not know the mode of action of the remedies which we use in many cases. A most common acute disease is inflammation. With reference to inflammation we have been working at the subject of the mechanism of the disease, for a long time, but during the last eight years very signal progress has been made, in the explanation of its leading phenomena. The most prominent phenomena of inflammation, the so called cardinal ones, are pain and heat, and redness and swelling; and with reference to the last two, we have been until lately unable to give any account of the mechanism of them; and even as regards heat, we have considered heat as a characteristic of inflammation, but until lately we have not possessed an exact measurement of the temperature which actually exists in inflamed parts, or understood the laws which govern the temperature. Then as regards redness we have known about the general fact that inflamed parts were red, but until the last few years we have not been able to explain the nature of the process by which, as soon as a part is irritated, it becomes red, that is, by which it becomes congested with blood. And then again going on to the other point, the swelling, which is another cardinal symptom of inflammation, it is only very lately that we have learned to know what the meaning of that swelling is at all—what it depends upon. We not only know now that it depends upon the passing out of the constituents of the blood through the blood vessels, but we know exactly the mechanism of that process, and have gained the whole of that knowledge entirely by experiments.

2297. (*Lord Winmarleigh.*) By experiments on animals?—By experiments on animals.

2298. (*Sir J. B. Karslake.*) Might we add the word "painful"?—Painful experiments. With regard to experiments on inflammation, even the most simple ones cannot be done without the production of a certain amount of pain; because pain is one of the phenomena of inflammation.

2299. (*Mr. Forster.*) But does that imply pain without anæsthesia that the experiments cannot be performed under anæthesia?—With anæthesia, as regards many of the observations perhaps; but still without anæthesia as regards the process itself. You cannot produce an inflammation in an animal and maintain a state of anæthesia during the whole of the process; it is quite impossible. Then take another disease, namely, the disease tuberculosis. You have here a disease which destroys an immense number of lives every week. With regard to the nature of tuberculosis, until within the last six years we have really known absolutely nothing of the mechanism of the process; I mean the physiological changes which take place in

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connection with it. For, as regards this disease and the other which I have mentioned, we have known what you may call the natural history of the disease familiarly for long; we have known, I mean, the aspects which it presents to the clinical observer, to the observer at the bed side; but what we have not known has been the intimate nature of the changes of which the disease really consists. As regards tuberculosis, the leading discovery with reference to its nature was made some half a dozen years ago, by a French observer, M. Villemin, who, although he misunderstood his results more or less, made observations which have led to investigations which have been carried out in all the seats of scientific activity, and which have led to an approach towards a really complete knowledge of the mechanism of the process of tuberculosis. That result is due not exclusively to experiments on animals made with reference to the question, but it was by experiments that we were first led to our fundamental knowledge on the subject. I mean we were enabled to understand the mechanism of the process by being able to produce it in animals. I think it does not need explanation that it is a proof that we have the key to the production of a process when we can reproduce it—when we can bring about the conditions under which it originates It must be understood that this progress was also helped forward by anatomical investigations, particularly with reference to the anatomy of the lymphatic system. As regards this branch of anatomy, the anatomy of one of the great systems of which the body consists, an equally marked progress has taken place during the last few years; and that investigation, although it was anatomical, has necessarily involved making experiments upon animals, because in order to make out the structure of the lymphatic system it has been necessary to produce what we ordinarily understand by disease, that is to say, abnormal conditions of it, in order to be able to trace out its structure; so that in both directions, both the anatomical, and the directly experimental, it has been necessary to put animals under special conditions, under conditions unnatural and disgreeable to themselves, in order to bring out the facts in question. I said before that a very large number of persons died of tuberculosis. Of phthisis we know that about 10 per cent. die of the total number of deaths; but besides these there are certainly more than that number that die of other tuberculous diseases; so that every week in London you have upwards of 200 people in a thousand who die of those diseases. With respect to inflammatory diseases I cannot state positively, but of the remainder the majority is certainly made up of them. There are many other common diseases of which we do not know the mechanism at all, although, as I said before, we know the natural history very well. For example, cancer, we cannot give any such answer with reference to cancer as we can give with reference to tuberculosis. Then again, acute rheumatism is a good example of a disease which we know very intimately, but we do not know its mechanism a bit. We do not know why it follows its course, or why it affects the organs it does affect. It also is a disease which is fatal to a great many lives, and if we had the same information there that we have with reference to the others that I have mentioned, we should be in the way of doing a great deal more good. Then I want to speak of a few remedies. First, I will speak of the influence of cold. Cold is used as a remedy against fever. I do not wish to speak of cold as a surgical remedy, but as a remedy against fever. Long ago, it was the custom to subject people to cold effusion, or to the cold bath, in the treatment of fever, and then that practice went out of use altogether. Now again it is coming into use, but not on the ground that a change of opinion has taken place, but because by means of investigation, by means of experimental investigation too, we know what is the mode of action of cold, and we know what is its relation to the condition which it is intended to cure, namely, fever. We know that in fever there are certain conditions in which the application of cold is injurious, and other conditions in which it is not only beneficial but the only thing which can save life. We have instances, of which several have occurred in London in the last few years, in which persons affected with acute rheumatism have passed into a peculiar stage of fever, which we now know how to recognize, in which the application of cold is the one and only thing which can save life, and by the application of cold life has been saved. This leads me to speak of its influence on fever generally. In fever we have two sets of symptoms; certain sets of symptoms which are dependent upon the influence of the high temperature of the body upon the body itself, and others which belong to the general state of the system; and what we know is, that with regard to those symptoms, those dangers in fever which are dependent upon the unnatural heat of the body, we can do good by bringing down the temperature by external means, but that as regards those dangers which arise quite independently of the heat of the body we cannot do so. Then I would like to speak of another remedy, namely, that of bleeding. It is very well known to everyone, I believe, that bleeding is a remedy which after having been used a great deal has been thrown aside, and again used. For example, at the end of the last century people were afraid of bleeding. It is difficult to say why, but it was so. During the period of the war, and immediately after the peace, there was a tremendous reaction in favour of bleeding, which reached an acme of extraordinary intensity somewhere about 1820, between 1820 and 1830. During that time the number of lives that must have been sacrificed to bleeding is altogether beyond calculation, and it must be remembered that these lives were so sacrificed simply for want of knowledge. Then came influenza and cholera, and they demonstrated clearly that bleeding was injurious by direct experience; notwithstanding which, bleeding, though less used, still continued to be used till about 25 years ago. Now, we know that bleeding is injurious, not because experience teaches us that it is so, for we know that experience seemed to teach our forefathers just exactly the opposite, consequently experience is not a safe guide in a question of that sort, but because we know precisely what bleeding does to the animal organism. We know also a good deal about the mechanism of fever; we know, for example, as regards the state of the blood, that the very same condition which is produced in the blood by bleeding is also produced by fever itself, and, consequently, that by bleeding a person with fever we are just helping to do the mischief which the disease is doing. Under those circumstances the question arises, is it conceivable that we should now fall back again into such a state of things as existed in 1820, that we should again resume bleeding, and begin to treat people as they did at that time? There is no reason why we should not, excepting knowledge. The fact that we know what bleeding does, and the fact that we know what fever is, are the only safe-guards against our falling back into the practices which our forefathers followed. That I think is a good illustration, so far as it goes. Then I would refer to the subject of food. In the treatment of all acute diseases we now attach great importance to food as a means of treating disease; and there again there has been a very great difference of practice. Some of us may remember the time when the starvation practice existed to a very much greater extent at all events than it does now. I will take one item of food, that of gelatine. Formerly it used to be held, that all gelatinous stuffs were the very best things you could give to invalids. People used to have hartshorn and isinglass and all those things, which were considered as among the most important *subsidia* of the sick room. That passed away, and the time came when all these things were condemned as useless. Now we are in a different position. The physiological action of gelatine has lately

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been most carefully investigated; these investigations have been carried on at the expense of the Bavarian Government, on a scale of sufficient magnitude, and now we have a mass of the most complete information with reference to the action of gelatine, and many other kinds of food, which places us in this position; on the one hand, we could not possibly come to a state of things in which we should attach such value as our forefathers did to gelatine; on the other, we could not possibly go back to the rejection of gelatine as an article of diet which succeeded that state of things. By science we are put in a position to form an exact and certain opinion as to the value of that particular article of food, and from that position no waves of medical opinion could possibly drive us. Then let me pass to another remedy, namely, alcohol. Alcohol is another agent which is chiefly used as a means of treating fever; and now we know a great deal with reference to it, as to how it acts. The first thing is this, that in fever the main condition, as I mentioned before, is high temperature; I mean that the temperature of the body, instead of being at the normal temperature, about 98°, rises to a temperature of 105° or 104°, and so on. What we know about it is, that in fever alcohol has the effect of bringing down the temperature to the normal state, but that upon the healthy person it exercises no such influence; if you give a healthy person alcohol, his temperature remains exactly the same as it was before (I mean giving alcohol in what we may call beneficial ordinary doses); but if you give the same doses, or suitable doses to a fever patient, you have a special effect produced, namely, the reduction of the temperature towards the normal. All this information about alcohol again has been obtained by experiments on animals, seconded, I must explain, by careful systematised clinical observations on patients; but in all cases of this kind the proper way to begin is by seeing what happens on animals first, and then of course you make the results of the experiments practically valuable by weaving them in with the results of experience.

I wish further to point out, with reference to some of the diseases which I have mentioned, particularly tuberculosis, the most important application which will be made of the knowledge we are gaining will be rather in prevention than in treatment, because our more careful investigations of the processes of tuberculosis only lead us to maintain the opinion, even more decidedly than before, that the process, when it has once begun, is an incurable process; but the application which will be made of our knowledge, as soon as it is complete, and is being made of it now as far as it is complete, will be rather to enable us to know how we may prevent tuberculous diseases. As regards the causes of tuberculosis, what has come out so clearly, both from ordinary experience and from careful investigation is that there are two sets of causes; that we have on the one hand constitutional predisposition, and on the other the exciting causes of the disease, and our investigations with reference to animals will, we hope, enable us to understand the nature and mode of working of both of these sets of causes. First, as regards the predisposition, what we have learned is that certain species of animals are susceptible, others are insusceptible. We know physiologically what are the peculiarities of these animals, what are the physiological differences, if I may so express myself, and this is guiding us to a knowledge of that in which susceptibility really consists; I mean of the anatomical and chemical differences between susceptible and insusceptible organisms. I merely mention this as one line of inquiry. Then as regards exciting causes, we have come to the same kind of knowledge of the exciting causes of tuberculosis. We know, for example, that there is nothing specific in the disease, but that certain external agents tend to originate the process and to keep it going; and our object is to make our knowledge of the nature of those external agents more and more precise. I will endeavour to illustrate that statement by referring to the way in which we should set to work to find out the influence of climate; I mean, for example, the reason why persons are affected by tuberculosis in London, and not affected by it in certain other particular districts. The way that one would attempt to find that out would be this, by subjecting animals to the same conditions which we already know to be productive of tuberculosis, on the one hand in London, and on the other in the other climates in question. This is a kind of investigation which has been already commenced, and there is every reason to hope that it will give us important information. If we learn, for example, that an animal which has been kept in a certain climate, where the air is absolutely pure, if I may so express myself, is under certain conditions not made tuberculous at all, but under the same conditions in London is made tuberculous, we certainly gain very definite information as to the influence of those two atmospheres in the causation of the disease. Of course I am anticipating in giving these examples. I do not mean to say that these questions have been answered, but I am endeavouring to make it clear how they are likely to be answered. The same thing applies to another subject, namely, that of the secondary diseases, which are produced in consequence of injuries, the so called traumatic diseases. We know that a person receives a gunshot wound, or a smash of some kind or other, and that the result of that is that there is local inflammation produced; but, that inflammation may follow a good course, or it may follow an unfavourable course, and give rise to a number of so called secondary diseases, a general involving of the whole system, or the production of secondary inflammations in other parts of the body, such as those which are called pyaemic, and are apt to follow serious injuries, particularly injuries which are attended with great destruction of tissue. All that subject is at the present time receiving an attention in all parts of the world where pathological investigation is going on, which has never been given to it before; and in great measure the reason of this is, that during the war a number of men versed in research were placed with the troops, and that in consequence new facts of a very remarkable kind were observed. These facts have led to further investigations, which have been, in a great measure, experimental; and thus an immense activity of research has been thrown on to this particular subject. I mean the mode in which the secondary results, so tremendously fatal as they are in their character, are produced in consequence of great injuries. That I think is another example showing that we are finding out by experiment in what way such states of things are produced, and how we may confidently hope to be able to prevent them.

2300. (*Chairman.*) For these discoveries of which you have been speaking, has physiology been dependent for its progress upon experiments made upon living animals?—I think I have nearly answered that question already, as far as the various subjects which have been mentioned are concerned.

2301. Under what conditions can experimental investigations be most advantageously made?—With reference to that I should like to point out the conditions which actually exist in physiological investigation just now, and which we hope will exist in future, although that point has been already partly brought out. Until recently investigation in this country, at all events, has been carried out in not a very systematic way. No doubt, as was said at the beginning, an effort is being made to organize research as much as possible. In Germany, as I think I have already stated, that has been the case for some time past, that is to say, that there are a good many persons in Germany whose lives are devoted to science, who hold responsible positions as University teachers, and who are engaged in physiological work and physiological research. In England the professed physiologist has been until recently unrepresented, that is to say, the physiological work has been done by persons engaged in other pursuits at the same time.

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Even the late Professor of Physiology in Edinburgh, who was the only professed physiologist in Great Britain until a few years ago, was also a medical practitioner, a consulting physician; that is not now the case, in Edinburgh; for the gentleman who now holds that position is a professed physiologist, who occupies his whole time in that way. At the present time there are three positions in England which are occupied by men who are professed physiologists. There is Dr. Foster's position at Cambridge, mine at University College, and the Brackenbury Professorship at Owen's College, Manchester. I do not know what the conditions are on which all of these appointments are held; but with reference to my own appointment I may as well say that my professorship is held under the condition of devoting myself to original research. The words of my appointment are these: "On condition of devoting to original "research, either in connection with his professorship, "or in any other work of a kindred nature, which "shall be essentially auxiliary to such research, all the "time that can be spared from the work of the "lecture room." In this way there is assigned to anyone holding my position a function which is separate from the function of ordinary teaching.

2302. Is that intended to exclude you from practice?—Yes, it is intended to exclude me from practice. What I want to draw attention to is the organization that this involves, and what that organization ought to consist in. What it ought to consist in is, in the establishment in connection with every school of science, and particularly of chemistry and physics, of a school of physiology, that is to say, chemistry and physics applied to the explanation of living phenomena. It is desirable that such schools should be connected with the systematic teaching of physiology to medical students; but that teaching is not what I mean by a school of physiology. I mean by a school of physiology, not the teaching of students by lectures, but the association of workers under the direction of a head, such persons not being students in the ordinary sense, but men who are themselves devoted to science, either for their whole lives, or for a certain definite period, men who are intending earnestly to engage in research for a certain time. It is in this way, I think, that physiology ought be studied, and the more it is studied in this way the better. In such schools of physiology it is clear that the work which will be done will be in a great measure experimental, because we cannot make progress without experiments of one sort or another, painful or otherwise. The question arises what the bearing of that will be upon the matter which we have now before us, namely, the question whether abuses are likely to arise in connection with such schools, or whether on the other hand it is not the case that the existence of such organized schools will tend to check and prevent abuses. I say that abuses are not likely to arise in connection with any such institution, because, in the first place, it is managed by a person who is publicly responsible, and, secondly, because the physiological school is a part of an institution which is governed always, as in the case of King's College, or in the case of University College, or Owen's College, Manchester, by a committee of men who are not in the least likely to tolerate anything which is objectionable in the conduct of such an institution; and I may say in passing that one practically finds that in every such body of men you will always find some men who have strong humanitarian feelings, so that there are always men who are, so to speak, on the watch against any abuses which are likely to arise in connection with such institutions. It is obvious that that method is the best for science. It is perfectly clear that for the sake of science it is most desirable that the whole thing should be methodized, and that all researches, if possible, should be made in this methodized way, and under the superintendence of persons who are themselves skilled, and with the assistance of others who are also skilled. But in England I do not think that we can put a stop to individual action. I cannot conceive that it is possible that we should not have men who are working in science quite independently. We know that in England individual action in all things is the national tendency, and it is just the same in science as in other things. A great deal of scientific work is necessarily done by persons who choose to work by themselves and who do not want direction, and do not wish for direction; and we have plenty of examples to show that that sort of individual work is very often productive of extremely valuable results. Of course, the objection to that is this, that there is not the same guarantee for verification; for everything that is done in an institution for research is verified, not only by the supervision which it undergoes, but by the presence of other persons who are engaged in other branches of research, and whose testimony is of great value.

2303. I think you have said that there is now an effort made to organize physiological inquiry much more systematically than it has ever heretofore been organized in this country?—Yes.

2304. That the most efficient mode of promoting such inquiry would be by institutions of a public character, like your own institution and Guy's Hospital and that of Trinity College, Cambridge, and so on; and that the publicity which would be connected with such institutions would have a very good effect for science, inasmuch as it secures verification of the experiments which might be doubtful if they were in the hands only of private persons?—Yes.

2305. That implies, does it not, that in its organization the influence of publicity will, to a great extent, prevail?—The influence of organization, I should rather say.

2306. So that anything that would happen in such institutions will of necessity be known to the gentlemen who conduct those institutions?—Yes.

2307. And being known to so large a number of persons, we may take it that in truth public opinion will come to bear upon them?—Yes.

2308. And that that public opinion will, in your judgment, be a great security against those abuses which are the particular subject of our inquiry?—Yes.

2309. You think, however, as I understand you, that in this country you cannot altogether repress individual inquiry, and that something must be left to individuals?—Yes.

2310. But you think also that those individuals ought to be subject, I presume, to the same general limitations that the institutions would be subject to, namely, that public opinion should influence and direct their proceedings, in reference both to science and to humanity?—Yes.

2311. Therefore that some legislative provisions, if they extended to these institutions, might reasonably also extend to any private inquiries?—I do not know whether the question suggests what I should like to say, but I should like to make this statement with reference to legislation in general. That so far as the institutions to which I have referred are concerned legislation is certainly not necessary, because there is no influence which legislation could exercise of a beneficial character upon those institutions, which is not already exercised by the organization under which they are placed. I refer to the two facts that they are under the direction of committees, or bodies equivalent to committees, containing men of position in society; and on the other hand that they are under the immediate direction of educated and responsible men.

2312. So that without thinking that for those institutions legislation is necessary, you think that such legislation as you were yourself a party to, proposed by Dr. Playfair, would not in any way interfere with the efficiency of those institutions?—I would answer that question, subject to the comments which I should like to make upon his bill, taking that as a sort of example of what I suppose legislation might be expected to be.

2313. As I understood you before, reserving to

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yourself to make some observations upon the details of the bill, to the general scope of it; you were an assenting party?—Yes.

2314. Now perhaps if you wish to make any particular remarks on the bill, you will be good enough to do so?—There is one point I should begin with, namely, the expression in clause 2, "new scientific discovery." That was not in the sketch of the bill that I saw, and that does not admit of any definition at all to us. A "new scientific discovery" has no definite meaning in relation to research. It is the object of research, not merely to make new discoveries, but to establish the truth, and to develop discoveries which are already made; and therefore what is really required there is something of this sort: "Any person for the purpose of investigation," or something equivalent to investigation, or "scientific investigation," it does not matter what expression is used, but it must be something to which one can attach a *bonâ fide* meaning; I mean one that one has not to take a great deal of trouble to assign a meaning to. The question what a new discovery is or what it is not is a question which might be discussed endlessly. One man might say, "This was anticipated a hundred years ago," and you would find just as many people who would say "It was not known at all; we were absolutely ignorant of it before;" because one would assume a mere vague expression of a thing to be an adequate statement of it, whereas the other would expect an exact statement of it and in this way there would necessarily be a constant conflict of opinion between people who spoke exactly and people who spoke vaguely. I do not see how you can possibly get over that, except by using some expression which would imply the *bonâ fide* intention of the investigator. Any investigation which is done *bonâ fide* for a scientific purpose, for the purpose of investigation, would be a proper investigation to make; and any investigation which was made without purpose would be, of course, not an investigation at all, and certainly not a scientific investigation.

2315. A short time ago, in reference to one of the instances which I proposed to you from your book, you said that to repeat a painful experiment which had already proved that which was the object of it, for no new object, but merely for a repetition of the same truth, would be improper?—Yes.

2316. I do not understand you to depart at all from that opinion, but merely to say that you think these words in the bill before you are too narrow, and would tie down an experimenter more than would be necessary to carry out the intention of the person who drafted the bill?—Yes.

2317. (*To Dr. Foster.*) You have had the advantage of hearing Dr. Sanderson's evidence. I would ask you if you have in the main agreed with it?—I have in the main; probably some of the answers I should have given would not have been exactly those which Dr. Sanderson has given.

2318. Does it occur to you to specify any particular instances?—No, I think I could not do that.

2319. Then I may take you as in the main agreeing in the evidence which has been given by Dr. Sanderson?—Yes. Perhaps one point in which I would not go quite so far as Dr. Sanderson would be in the matter of the necessity for legislation.

2320. Would you be so good as to state what is the point of difference which you wish to bring out?—Simply that I, so far as my experience goes in this country, think that there have been no abuses in physiological inquiry, and I think that that humane sentiment to which Dr. Sanderson referred, might be of itself trusted entirely to prevent any such abuses taking place.

2321. But supposing that in the minds of the most competent persons, and the most elevated persons, and of those with whom you generally are most acquainted, there is that humane sentiment, would you entertain an objection to taking measures which might prevent physiological experiments falling into the hands of other people of less exalted sentiments?—Certainly not. That was not the point on which I differed from Dr. Sanderson, but on the other point; I have no objection to legislative interference. I only disagree with him so far as I understood him to say that he thought it was desirable or necessary.

2322. Let me ask you some questions as to one or two of the experiments in this handbook which are given under your name. I see on page 395 you begin by saying:—"Introduce beneath the skin of the "back of a strong frog a drop or two of a solution of "urari?"—Yes.

2323. Do you entertain the opinion that urari is an anæsthetic in regard to frogs?—What I would say is this: that there can be be no doubt that urari suspends the functions of the motor nerve trunks; there also can be no doubt that it does not affect to anything like the same extent the sensory nerve trunks; those two things are quite certain. Bernard and others with him have from that and other facts about urari drawn the conclusion that the central nervous system, as distinct from the sensory nerve trunks themselves, was not affected at all by the poison. It would be quite possible of course for the poison to affect the sensory nerve trunks and not affect the central nervous system; and on the other hand, for it to effect the central nervous system and not the sensory nerve trunks. From my observations on urari poisoning in the course of some two or three years, I had always been led to doubt very much the immunity of the central nervous system from the action of urari, while admitting, of course, that the sensory nerve trunks were not affected by it; and most physiologists, I think, will agree that by large doses of urari the central nervous system is certainly affected, that is to say, that although action may take place in the sensory nerve trunks, that sensation for instance might begin there, it would not be developed as a conscious sensation in the central nervous system; and from some experiments which were made by Mr. Yule of Magdalen College, Oxford, comparing two frogs, one under urari and the other not under urari, there seemed to me to be very strong evidence indeed that urari does, even in moderate doses, prevent voluntary movement or volition; and if it prevents volition, it seems to me that the arguments are extremely strong in favour of the view of its doing away with consciousness. Now those experiments can be carried on on in a frog; and similar experiments on other invertebrata have been carried on by Dr. Steiner, of Halle, all pointing to this conclusion, that urari does act in some of these animals primarily upon the central nervous system in destroying volition, and with that, one infers, taking away consciousness. As far as I know the experiments have not been carried on in the case of warm-blooded, quickly breathing animals, and at present one is uncertain as to whether the action of urari has the same effect upon them as it has upon frogs, because the method of observation which is employed upon frogs cannot be employed upon the warm-blooded, quickly breathing animals.

2324. I infer from what you say, that in your opinion, urari does destroy consciousness in a frog?—Yes.

2325. You differing in that particular from Claude Bernard and from the physiologists of the continent in general?—Yes.

2326. And you have set forth the experiment mentioned in this book under that opinion?—I beg your pardon, No. This book was written before I arrived at that conclusion. You ask me my opinion at present. Those experiments of which I have been speaking are recent. When this book was written I simply was in doubt about the action of urari upon the central nervous system, and was not prepared to give, what I think I may now say I can give, tolerably conclusive evidence of the view that urari does destroy the central nervous system.

2327. Then this book was written under the impression that urari did not?—This book was written

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under the suspicion, under the belief from several facts, that urari did destroy the central nervous system, but not with what I believe to be the tolerable certainty which I have at present.

2328. Then the book was written under a doubtful impression on your part whether a frog into which urari had been infused was conscious of pain or not?—Yes.

2329. And when you said that they should get "a strong frog" and introduce a drop or two of a solution of urari beneath his skin, you were very doubtful whether he would be conscious of pain or not?—Yes.

2330. And you took a strong frog, I suppose, because you thought he would be better able to bear pain?—No, but because he would better manifest the phenomena of the poison.

2331. At any rate it was only a doubtful opinion of yours that he would be relieved from pain?—Yes.

2332. Now these experiments upon frogs are very painful ones in their nature, are they not?—Some of them; not all of them necessarily. Might I just make a preliminary statement, before you ask me any other question, as to my purpose or what I had in view in writing these directions? If you turn to the preface on page 341,—I mean to the introductory remarks,—you will see that I say "Such observations "and experiments as the student may be reasonably "expected to perform for himself under due super-"vision." My idea was that these directions would be used in a physiological laboratory, under the direction of a competent supervisor, and the object of the form in which I put the observations was to enable the student to go on with the operation for a certain time, while the supervisor passed from him to another student, to facilitate the performance of the operations under the demonstrator. I have therefore taken no special care to point out exactly what operation should be made by this or that class of students. One would have students of various classes; some whom you would not desire at all to repeat any of these experiments; some to whom you would entrust a certain number; others to whom you would entrust a larger number, and in whose case the supervision need not be so exact and so complete as in the case of others. Consequently I was not so careful as otherwise I might have been. I was not particular. I did not take particular care to insist in every case upon the introduction of anæsthetics. I put them in several cases, but I was not careful to put them in all. Now in this particular case my own practice, I not feeling certain about its action on the central nervous system of a frog, was to introduce with urari a certain quantity of morphia, which would not materially affect the general result of the experiment, but would make it, one would then feel satisfied, certain that the animal was suffering no pain whatever.

2333. Would you allow me to ask you, do not you think it would have been better, with regard to the persons for whom this book is intended, and who are described in the general preface as "beginners," to specify a little more completely that degree of anæsthesia which you yourself thought it right in your own practice to produce?—Yes. I think, supposing that I had to write the work over again, I should be more careful.

2334. And that it would be desirable that, if possible, the communication of your present opinion should be circulated as generally as the book itself is circulated?—Perhaps it would be.

2335. And that would apply also to the other experiments which are upon pages 396 and 397?—Yes. May I say that, for instance, the experiment on page 396, I think, is not so painful as one might perhaps be led, on first reading it, to imagine. The operation itself is conducted under chloroform. After the operation has been performed, the amount of paid felt by the animal while the urari is acting is not very great, because it shows no signs of pain before the urari is administered.

2336. I do not at this moment see in this experiment any reference to chloroform; I was speaking of Observation 3?—I thought you were speaking of Observation 2 on page 396.

2337. I asked about Observation 3 and Observation 4. You have already stated, you know, and it will go to the public, that you yourself did use what you believe to be complete anæsthesia, and that if you had to write the book over again you would state to others what you do yourself?—Yes.

2338. I ask you whether that does not apply also to pages 396 and 397?—Observation 2 also comes into page 396, and there chloroform is suggested; and in my own mind I concluded that the chloroform would be given in the perfectly corresponding Observations 3 and 4. I thought it was unnecessary in Observations 3 and 4 to state that chloroform might be given, because I had done so in Observation 2.

2339. (*Mr. Erichsen.*) You wrote the book very much as one writes a book on operative surgery, where it is supposed that anæsthetics are to be given, and yet anæsthetics are never mentioned?—Yes, I wrote a great deal in that way. I wrote first without putting in the anæsthetics, and then afterwards I put in the anæsthetics. If I am to explain how the matter came about in these cases of omission, I may say that first of all I remember writing several of these observations without stating that chloroform was to be given, because I took it for granted that chloroform would be given. I did not go into that detail in the experiments; I put the anæsthetics in in some, and omitted them in these two.

2340. (*Chairman.*) Then may I take it generally, without going into individual cases, that you entirely agree with what we have had stated by other teachers, that for purposes of demonstration complete anæsthesia may be secured?—Where painful operations are performed.

2341. That where an operation, in itself painful, is resorted to, complete anæsthesia may be secured?—That is in cases of demonstration. I make it a practice in cases of demonstration to give such demonstrations only as those in which anæsthesia can be applied, where distinct pain comes into the operation.

2342. And you agree in all that has been said so far as this, that it is so in your school, and ought to be so?—Yes.

2343. And do you agree in the other observation, that—speaking now not of toxicology, the introduction of disease into the frame of an animal, but speaking of surgical operations—the number of those which need be painful, and without anæsthesia, is comparatively very small?—Yes.

2344. That in others the pain may be and ought to be very much diminished?—Yes.

2345. That is to say, the most painful parts of the operation being performed under chloroform?—Yes.

2346. And that as far as you know and believe the sentiment, both of the teachers and also of the pupils, in this country would be and is in favour of the utmost tenderness in dealing with animals made use of for the purpose of experiments?—As far as I know.

2347. You thoroughly believe that?—As far as I know.

2348. You object to legislation, not on the ground of any mischief that reasonable legislation would introduce, but, as I understood you, only on the ground that in your belief it is in this country unnecessary; is that so?—Yes. There would be some inconveniences attaching to legislation, but I think also that there would be even for physiology some advantages, and that the one may be counterbalanced against the other. The difficulties, if I may say so, would apply with reference to the private individuals; those, I think, are small in number, and probably decreasing. I think that the nature of physiological experiments is such that the best of them, and probably a large number of them, can only be conducted in a physiological laboratory. I do not place any great stress upon that objection to legislation, but I say on the whole that I object to it chiefly on the ground that it is not needed.

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2349. But supposing it to be needed for private persons, and not to be prejudicial in the case of physiological laboratories, you would not I suppose see any very great objection to it?—I think it would be an objection if private individuals were prevented from following physiological experimentation, but I say that I do not lay great stress upon that because they are few, and probably will become fewer and fewer year by year.

2350. The restrictions which you yourself place, and which all those with whom you are acquainted place upon the practice in these public institutions ought, I presume, to be placed by private individuals upon their own practice?—Yes.

2351. And if there were reason to suppose that there were cases in which they were not so placed by them, the public sentiment might be allowed to express itself in regard to them in the form of some provision which would enforce those restrictions, might it not?—Yes.

2352. (*To Dr. Burdon-Sanderson.*) Have you anything else to say upon the bill introduced by Dr. Playfair?—First as regards the principle,—and what I mean by the principle of the bill is the principle of a licence,—I cannot see any objection to the principle of a licence, because I know very well what the working of licences is in other matters for which people are licensed, and I know that there is nothing in a licence which is oppressive, there is nothing objectionable in having to obtain a licence for any lawful purpose, and no particular inconvenience attending it, unless it were made inconvenient by the conditions; so that there is nothing objectionable in the principle of a licence to do this act more than to do anything else. Then I should like to say something about a point which is not referred to in this bill, namely, the question of inspection. There is no provision for inspection in this bill, and I have very carefully thought over that question, and the way in which any inspection could be carried out. I cannot see that any inspection is possible. There is no example of a similar inspection of any other kind that I am aware of, and I do not see how the functions of an inspector could be usefully performed; and I am quite certain that it would lead to bad results if there were any appointment of inspectors, because of course in any institution (I am speaking first, of the effect of inspection upon institutions) no inspection would give any guarantee whatever as to what happened when the inspector was not present; and the very fact of inspection would lead to a spirit of opposition on the part of subordinates and servants, and in that way a tendency to concealment would spring up, which would certainly be injurious to the interests of humanity. Then as regards the keeping of a list of the animals, that I believe has been proposed. As regards that again it would be impossible to carry it out with any good effect or result. In the first place, because the animals that happen to be there may be intended to be used not for experimental but for anatomical purposes; and then secondly, with respect to some of the animals, as regards frogs for instance, it would be altogether futile to attempt to keep a record of them, those animals I mean that are used in much larger numbers than others, and which are used partly for anatomical purposes and partly for experimental purposes; you could not possibly pretend to keep anything like an exact register, and an imperfect register would be merely a temptation to carelessness and inaccuracy. So that I really do not see that any good could be got by that mode of exercising supervision.

2353. We were told yesterday by some eminent teachers from Guy's Hospital, that everything at Guy's was open to the Society for the Prevention of Cruelty to Animals if they liked to come and see, and that there were no secrects of any sort?—Yes.

2354. I understand you to have the same opinion as was expressed by them on this point?—Just the same. I may mention with reference to my own demonstrations, that whenever demonstrations are done, the time for doing them is written up on the black board; so that if any gentlemen should wish to see them, they might come. It may be perfectly well known to everybody, the board hangs in the corridor, and it can be known that something is going to be done relating to a certain particular subject.

2355. (*Lord Winmarleigh.*) Are the public admitted generally?—Any person, I believe, can walk into University College; certainly anybody could go in there without any difficulty.

2356. (*Chairman.*) May I understand you then to mean that, agreeing in the principle laid down by the gentlemen from Guy's, that perfect publicity should be allowed in all particulars, your objection is to the particular method of securing publicity which has been proposed by some people, namely, an inspector?—Yes.

2357. (*Mr. Hutton.*) May I ask if what you stated just now about the notice on the blackboard applies to the laboratory, or only to the large classes?—That applies only to the fixed lessons. But I may mention that these lessons, when an experiment is made of the kind I was describing before with anæsthetics, are not given with the general systematic course. It is a thing which is arranged beforehand, and the number coming to such a lesson would be relatively small.

2358. (*Chairman.*) But I understood the evidence that we had before to go to this, that there was nothing whatever done within the four walls of the place that was not perfectly open for the public to know, if they wished to know it?—If they wish to know it, certainly. I do not, of course, mean to say that we expect to be intruded upon at work.

2359. (*Lord Winmarleigh to Dr. Foster.*) In talking of the private practice of experiments, you say that the private practice has hitherto been small, and in all probability will become still smaller, have you any idea what the amount of private practice is at the present time?—I think I only know one person who is not connected with a laboratory who is making actual experiments—One person in England, I mean, who is making experiments not in a laboratory.

2360. (*Chairman.*) Who is that?—That is Mr. George Henry Lewes.

2361. (*Lord Winmarleigh.*) And you do not think that in any of the great towns of England at the present moment any private experiments are being made by the medical profession or other parties?—Although there are not regularly organized laboratories in the large provincial towns they have professors of physiology there, who probably have small laboratories.

2362. I mean outside of the great medical schools?—I do not know of anybody, and I have no reason to think that there are any number of experiments being carried on by such persons.

2363. You do not think that even in those medical schools not connected with London these experiments are made without the proper superintendence, and a proper restriction upon cruelty?—I can only say that I have not heard of anything which would lead me to think that such did take place.

2364. (*To Dr. Burdon-Sanderson.*) I will ask you a question with reference to this matter. I see that you are the editor of the book before us. Have you any knowledge of the circulation of the book?—The number of copies that have been sold now I am afraid I cannot give any very exact information about.

2365. But is it a large number?—At present I do not think it is. I do not think that it is selling now to any great extent. It is entirely in the hands of the publisher, and no second edition has appeared.

2366. You have no control over it?—No control over it.

2367. You are aware, I daresay, that a great deal of the feeling which exists now in the public mind upon the subject has been caused by this book?—I am very sorry to learn it. I have only learned it very recently.

2368. It has been admitted by yourself and by Dr. Foster that certain mistakes have been made in the publication which might lead to an erroneous opinion on the part of the public. Could you suggest

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to the Commission the best mode of correcting that, because I think you admit there is no doubt that an erroneous impression has existed in the public mind from a little want of sufficient explanation in the book as to the mode in which the experiments can be made?—Some step that the Commission could take, or that could be taken by others, do you mean?

2369. Whether it could be taken by yourself or by the publisher, or in any way, to correct the very erroneous impression which you maintain exists in the public mind as to the practices in the great schools of physiology?—A plan has occurred to me, which in fact I ought to have carried out perhaps before, namely, that we might prefix to this book the resolutions which are embodied in a Report of a Committee of the British Association which I was a member of, and took an active part in, with reference to this very question. If it was thought that that would accomplish what is required, it would be very easy, of course, to circulate pretty widely a statement of that kind, and also to have a copy of that report appended to the book, or placed before the title page, or something of that sort.

2370. Would you, as the editor of the book, feel any objection to our making a pretty strong expression on that point, supposing the Commission thought fit, that it was an error in the circulation of the book?—Certainly not, if the statement did not go beyond what we have admitted. If it could be made in any way that would express what I have said on the subject, of course I should not have the slightest objection to its being made as public as possible; I should be glad that it should be made public.

2371. I am sorry, after the very interesting description you have given us, to bring you back to any of the experiments; but there are two to which I would call your attention. One is at page 174, an experiment by Dr. Durante. It is to this effect: "In a " rabbit two small incisions are made across the " course of the external jugular vein, one near the " clavicle, the other near the origin of the vessel, " great care being taken not to go down deeper than " is necessary in order to see the vessel through the " fascia. A small needle is then passed under the vein " near the proximal incision, in a direction at right " angles to that of its axis, and corresponding to that " of the incision, but deeper. A second needle is then " laid in the course of the incision, and drawn tightly " towards the first by a ligature at either end, by " which means the blood current is entirely arrested, " while the coats of the vein are absolutely protected " from injury." Then it goes on to say, "After the lapse of a couple of days," and so on. Now in that instance is it possible that that animal having gone through what is evidently a very painful operation, could be kept for a couple of days without suffering extreme agony?—It certainly is perfectly possible. Although that manipulation seems to be a very severe one, it really was not a very severe experiment, because the irritation which is produced by passing a fine needle behind a vessel, if it is done with any moderate amount of skill, and then enclosing the vessel between the needle and the ligature, as directed here, is a method which is used in surgery, and is certainly not attended with any great amount of pain. In fact the presence of a needle in the living tissues we know very well does not produce much irritation. But, however, that is not the only answer that I would make. I would go on to say that this is purely an experiment of research, and that one would never dream of repeating it, excepting for the purpose of proving the fact. It affords the clearest, so to speak, and sharpest and best proof of the particular fact which it was made to illustrate that really can be given. And the history of that paragraph is this, that this experiment was made by Dr. Durante, in my own laboratory, and in that way it happened to come into the statement of the experimental basis of the conclusion arrived at with reference to the cause of the coagulation of the blood.

2372. I have gone through these cases, but I have not observed that any explanation, such as you have now given, is included in the book itself?—I am afraid not. The point to be remembered about that is this, that in the book there are two sorts of experiments given. There are a certain number of experiments which are suitable for repetition, because they are experiments which might be made under anæsthetics. There are other experiments which may be considered merely basis experiments, that is to say, experiments by which certain important truths are, or have been, simply demonstrated in the sharpest and best way. Various people have investigated this question, for example, of the influence of the living vein upon the blood in the prevention of coagulation. It was required to put down an experimental statement of the facts upon which our conclusions with reference to that subject are founded. To serve for that purpose this, which was the best form of experiment, as it appeared to me, which had been made, was related as an expression of the experimental basis for the statement with reference to the action of the vessel in preventing coagulation. There clearly ought to be in the book a distinction between experiments which owe their value to being records of facts upon which a truth is based, and on the other hand experiments which are intended to be used as means of demonstration, so to speak, or as patterns for researchers; because supposing that a man is investigating not this question but some allied question, in which a similar method would be applicable, he would naturally take a description of an experiment like this, not as his guide, but simply as a point of departure, so to speak; he would modify the method to suit the particular question which had to be investigated.

2373. (*Mr. Huxley.*) Would you be so kind as to say how long after an operation of that kind the rabbit took its food freely, and whether during the two or three days afterwards it showed the slightest sign of objection to its food, or any sign of illness?—In this case certainly not; the rabbit is not in the slightest degree affected by the introduction of a needle under a vein.

2374. (*Lord Winmarleigh.*) That for the number of days alluded to in this experiment he did not show any signs of having suffered any pain?—Certainly not.

2375. Was the rabbit killed after the experiment?—Necessarily; because the point was to see what the structural changes were in the vein.

2376. (*Mr. Forster.*) It would not be a fair impression then to suppose that the animal died in consequence of the experiment?—No; it would have lived to this day so far as the experiment is concerned.

2377. (*Lord Winmarleigh.*) At page 176 this experiment is mentioned as having been made by Mr. Schäfer:—"The following experiment devised by Mr. " Schäfer which has been repeated a great number " of times in the laboratory of University College, " proves this much more conclusively and satisfactorily. " A glass tube, three or four inches long, is drawn " out at one end into an arterial cannula, of the usual " form and of suitable size. A frog having been " secured in the usual way in the prone position, the " heart is exposed, and the right aorta ligatured. A " clip is then placed on the left aorta at its origin from " the bulb. The cannula is then inserted and secured " in the left aorta, and the tube supported vertically " by a suitable holder. This done, and the clip " having been removed, the blood is allowed to flow " into the tube." It then goes on to say this, "It " rises to a height which varies according to the vigour " of the animal, and the quantity of blood which its " vascular system contains, the blood column oscil- " lating with the contractions of the heart." Now if this is done under anæthesia, does a vigorous animal demonstrate more clearly than a weak animal when under anæthesia any experiment?—With regard to this experiment, the only difference between the result in a vigorous animal and in an other animal would be that the column would be higher; the result

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would not be modified in its character in the least. But the moment that the thing has been done, there is no reason for the animals continuing to live at all, and that is the actual case in this experiment. It is a very instructive experiment, and one which I do every year. It is one which must be attended with extremely small suffering to the frog; the destruction of the nervous system can be done intercurrently in the experiment.

2378. You think that even without anæsthesia this experiment can be done without any great suffering to the animal?—Yes, I used a strong expression earlier in my evidence about using chloroform with frogs, namely this, that it would be "absurd" in many cases; and so it would, because the difficulty of getting a frog under chloroform is so considerable, and the period which it takes to kill a frog in fact in the way which is described there, though it is somewhat elaborate, is still very short; because the whole operation consists in introducing into the principal artery of the body a tube into which the whole quantity of the blood of the animal flows. Of course as soon as that is done the thing is over, and the whole of that process lasts a very short time.

2379. (*Mr. Forster.*) When you say a very short time, how long a time would you be in killing the frog?—I suppose that the whole thing would be over in the course of about ten beats of the heart. The whole quantity of blood or the greater part of it in the circulation would in that time have passed out of the heart into the tube. Of course the preliminary part takes a certain time, namely, the securing the artery and introducing the tube; but taking it altogether, the time is really very short; and besides I must clearly explain that I entertain a most distinct conviction that the exposure of the heart of a frog is not attended with any, excepting a very slight amount of suffering; and I am quite willing to enter into that point if it is wished.

2380. (*Lord Winmarleigh.*) Upon that subject of the suffering of a frog, may I ask what are the chief reasons which have induced the professors of physiology to suppose that a frog does not suffer pain under curari; whereas it has been decided that it is possible that larger animals may suffer?—In the first place those experiments which Professor Foster referred to made by Dr. Steiner; and secondly, these investigations of Mr. Yule's; and thirdly, some others which have been made by Dr. Brunton and others.

2381. What are the symptoms which induce you to believe that in the larger animals curari does not affect the sensory nerves, because I think you said in the larger animals it was supposed not to affect the sensory nerves?—That is quite correct; we know that both in the frog and in the larger animals the sensory nerves are exempt from curari in ordinary doses; but the evidence that we have, as Professor Foster explained, goes to show that the central organ itself is acted upon by curari in the frog, because we are able to show that, for example, if you exclude a certain portion of the muscular system from the action of the curari, the animal does not use those muscles any more than it does the rest; showing that it is not as you might suppose that it shows no evidence of pain, because it is not capable of using its muscles, but because it has no desire to use them.

2382. Generally speaking does a frog demonstrate the same pain in common operations without anæsthesia that a larger animal does?—I am particularly glad that you have asked me that question, because it is one that I wish to answer. The evidence of pain on the part of the frog is excessively small. The mode of expression of pain which a frog has is entirely what it does with its muscles; I mean the motions of its limbs and the motions which it makes for the purpose of getting away; in fact, the only characteristic sign of pain or discomfort that you have in the frog is that it jumps off. With respect to that we have exactly the same evidence of pain in a frog which has been deprived of its hemispheres as we have in the other frogs; because the very same excitation of the skin, a pinch, for example, of the skin, will produce exactly the same reaction in the frog which has been deprived of its hemispheres as in the normal frog. It cannot express its feelings in any other way, and it expresses them just as perfectly when it has been deprived of its hemispheres as it does with its hemispheres. Then the objection may be made, "Oh, but it is possible "that the frog feels without its hemispheres, that it "has other organs by which it feels." The answer to that is, the only reason that you have for supposing that the frog feels at all is that we feel under the same circumstances. We know that if our hemispheres are taken away we feel nothing; and we know with regard to other animals of which the expressions of pain are distinct, that they also, in the absence of their hemispheres, feel nothing. Consequently the very same ground which we have for thinking that the frog does feel applies to the other question, whether or not its feeling is dependent upon its hemispheres. If it feels at all its feeling must be dependent upon the entirety of its hemispheres. If we prove that it affords the same signs of pain when it is without its hemispheres that it does with its hemispheres, we prove that those signs are worth nothing as a sign of pain.

2383. If we are deprived of our hemispheres we die?—We die; but the frog does not; it goes on living. You can go on feeding it (it has not sense to feed itself), and it is just as healthy as another frog.

2384. Does not that show that it is not an argument one way or the other?—Quite so; I say that the evidence is negative as regards the frog.

2385. And you cannot produce any evidence to show the reality of the frog not suffering?—Neither way; we cannot prove it either way. Of course, we believe that as the frog has a nervous system which is very much below ours in activity, and as particularly its organs of sensation are very much smaller relatively than ours, we may take it for granted that in proportion it feels less; but we also take it for granted that it does feel to a certain extent.

2386. May I ask you, do you in your school make experiments frequently upon the larger animals?—For scientific purposes you mean, I suppose. We use very much fewer animals than people imagine; we use comparatively few.

2387. What are the animals besides frogs, dogs, and rabbits that you experiment upon the most? Of the larger animals, which do you perform your experiments upon?—I may say none almost. A cat might be substituted for a dog for certain purposes.

2388. Do you believe that experiments are made in any of the other schools upon any other animals, frequently I mean, than those you have mentioned, dogs, rabbits, and frogs?—I think cats are used occasionally when they are suitable for the purpose. There is a set of experiments I should rather like to mention made upon cats, namely, those made by Dr. Legg. I am not in the habit of reading the papers, but I believe that those experiments have been very severely commented upon. With reference to those experiments, the object for which they were made was an extremely definite one and a very important one. The experiments in question were with relation to the effect of high temperature (I think those were the ones most objected to), on the tissues of the body, that is to say, the changes which certain organs, and particularly the liver, undergo under the influence of high temperatures. I have got here the paper, and if you look at it, you will find that none of the temperatures were higher than are sometimes met with in fever; and consequently it was no very extraordinary suffering to submit the animal to a temperature of that kind, of course that being the temperature which was required for the purpose.

2389. I will not go farther upon that. You stated I think that it was impossible to carry out inspection effectually. Do you see any reason why the heads of the different schools of physiology should not be themselves made equally responsible as the man who might be appointed inspector?—I do not see any

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objection to that, it might be implied in the conditions of the licence.

2390. You yourself, for example, might be made responsible for the conduct of the operations in the school over which you so ably preside?—One would not like to be made liable to penalties for any impropriety of conduct on the part of others in experiments they might make. All one can say is, "I will do the best I can to prevent anybody doing what is wrong;" but to make me responsible for an act, and that a penal one, done by another would be going too far I think.

2391. You are acting at the present moment, I presume, under a licence?—A licence to teach, but not to conduct investigations.

2392. But you see no objection, you say, to any licence being issued to the head of a school, a professor who should teach physiology under certain responsibilities?—Provided that they were not penal responsibilities. I mean to say that I could not go beyond, in the way of responsibility, saying "I will "do my best to prevent anything being done that is "wrong."

2393. (*Sir J. B. Karslake to Dr. Foster.*) I understand you to say that in your judgment there ought to be no licensing either of laboratories or of private individuals?—I said that I thought it was not necessary. I did not go so far as to say there ought not to be any licensing; I said that I did not see the necessity for it.

2394. Now supposing the necessity either exists at the present time, or should exist hereafter, have you at all determined in your own mind the sort of license which should be given to that individual?—No, I have not; I feel a great difficulty on that question.

2395. Have you at all determined in your own mind whether it would be expedient that a private individual who might be perhaps a man of great eminence, engaged in physiological research, should be bound to state what experiments he proposed to make, and what the object and result of those experiments were, before he was allowed to perform them?—I think he could hardly do more than indicate the *line* of his research. Because directly you begin an inquiry you begin in one line—you begin, I mean, with one set of experiments; and at once something occurs which takes you off in a completely different direction. No one can possibly declare completely beforehand what are the experiments that he wishes to make; and if he began an inquiry, having made the experimetns which he said he was about to make, and in the middle of those experiments a diverging line occurred to him, and he then had to wait till he had a further license to go off on that said line, physiological investigation would be impossible.

2396. If any license were to be granted at all, should you propose that it should be a general license to practice vivisection with a view to science?—I think it must be a general license; I do not see the possibility of defining any special license.

2397. (*Mr. Hutton.*) You say that you do not see the slightest necessity at present for any kind of inspection or restraint, on the ground, as I understand you, that there are so very few individual inquirers who engage in these studies; or at least partly on that ground?—Partly on that ground. Yes

2398. Do not you suppose that there are a very much larger number of individual inquirers of that kind on the continent of Europe than there are in England, that in France and Germany, at least, there are very many more individual inquirers of this kind?—You mean isolated inquirers not connected with laboratories or schools, as I understand you?

2399. Yes?—I have very little practical acquaintance with the continent except by means of periodicals; and of course it is not always stated where the inquiry is carried on. My idea from conversation, and so on, is that in Germany the number of individual inquirers is excessively small. I do not know of any results which have come out in that country during the last few years which have not been brought out in some physiological laboratory attached to some university or school.

2400. And do you think that the same is true of France?—I think so, because all the work which I have seen in French for the last 10 years has come entirely from the laboratories of Paris or Lyons.

2401. I understood you to say that inquiries in regular physiological schools abroad are conducted with much less regard for animal pain than they are in this country?—I beg your pardon, I made no observation at all about the conduct abroad.

2402. May I ask you whether you suppose they are so conducted?—I really do not know. I am entirely a home-bred physiologist and know very little practically of experiments abroad, except what I gather from periodicals.

2403. You know Béclard's book, and Claude Bernard's?—Yes; but I cannot speak on the practices of foreign physiologists with authority; I simply repeat what I hear; I have no personal knowledge.

2404. Have you no knowledge of Schiff's Inquiries?—I have knowledge of Schiff's Inquiries, but no special knowledge of the way in which he behaves and carries on his work in the laboratory. I cannot speak with any authority on that.

2405. With regard to the nature of the experiments which you have read about, would you not say that a great many of them were needless, and I will not say wanton, but experiments which you would be sorry to conduct yourself?—It is very difficult to say when an experiment is needless. One feels a difficulty in sitting in judgment upon one's fellow men in that way. It is quite possible. One knows that there have been needless experiments performed.

2406. Just to fix our ideas, take the case of excising the kidneys altogether from animals, and studying the results when the kidneys are excised, which is quoted here from Béclard's book. Should you think that an experiment which is not likely to be useful to scientific research?—I should think it most distinctly likely to produce useful scientific results as part and parcel of an investigation. I do not think there is any inquiry more promising than that of determining the place of production of urea, and in this inquiry the extirpation of the kidney is one important operation.

2407. Do you know Gavarret's Book on Heat produced by Living Creatures?—I know it cursorily; I looked at it some years ago.

2408. Do you suppose that experiments on baking animals to death are of the kind which would be conducted in English laboratories?—I should not like to say anything about it, unless I knew all the details of the experiment. Put in that form, I should say that "baking an animal to death" would not be useful.

2409. They were simply experiments to ascertain at what temperature the animal would survive?—It must depend so much upon what investigation the experiment was a part of, whether it was justifiable or not.

2410. Then your general impression is that a great many experiments conducted abroad are a class of experiments that you would be sorry to conduct yourself?—No, I would not go so far as that. So far as I know (and I only give this from hearsay, I speak this not as a physiologist, but simply as a person who has had things said to him,) the physiologists abroad are not so tender as I should be myself, or as are the English physiologists. I have no practical acquaintance with the physiological laboratories abroad in their working condition.

2411. I may take it that it is the intention, is it not, of this book, and of what I should call your school of thought, to introduce the general system of study abroad very much more generally into England than it has hitherto been introduced?—I think our study is a study entirely of our own; I mean, I think we teach physiology in a way that it is not taught on the continent at all.

2412. The experimental method is derived very much from the continent, is it not?—The experimental method is coeval with physiology.

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2413. Quite so; but I understood you to say that hitherto it has been exceedingly little pursued in England, and not nearly enough?—I beg your pardon. I have made no statement in reference to that. I can only agree with what Dr. Sanderson has said, that, say, some 20 years ago, there was very little physiology carried on in England, and a great deal abroad. Experimental physiology and vivisection had been carried on in England before that time; and at one time, as for instance, if you take from the year 1810 to 1820, when Sir Charles Bell, and afterwards when Marshall Hall was working, probably nearly as much was being done in England then as on the continent at that particular time. Since then much more has been done on the continent and less in England.

2414. Still your object is to extend the experimental method in England, is it not?—My object is undoubtedly to advance physiological science, for which the experimental method is one good means.

2415. When your object has been attained, do you not think it is very possible that restraints may be desirable that are not now desirable simply because that method is not now much pursued?—I do not see any reason to think that, because the use of the method would be increased, therefore the good feeling which characterizes the Englishmen who have taken part in it would be different when they were many from what it is when they are few.

2416. Then you think there have been no abuses of any kind in the English schools?—I do not think there have been any.

2417. Now I understood you to say that for the purposes of demonstration you thought all experiments ought to be under anæsthetics?—All experiments causing pain.

2418. Did you mean by demonstration, demonstration in the larger classes of medicine, or demonstration in the laboratory?—I meant both.

2419. Are there not experiments in your own portion of this handbook which are, strictly speaking, demonstrative experiments, and which cannot be conducted under anæsthetics by any possibility? I will take two. One is an experiment which we were told by Professor Rolleston that you had never performed yourself, partly from horror of the pain,—that on recurrent sensibility. I do not know whether he was correct in that statement?—Quite so; I have not performed it, and have never seen it done; and I certainly should not offer that as a demonstration to students.

2420. Is it not the tendency of this book to suggest that as an experiment which the physiological teacher should show his students?—No, I think not; and if you read that chapter you will see that there is a very great difference in the manner in which that experiment is introduced as compared with others. In the majority of cases I have used the imperative; I have said "repeat" "devise," and "place," but on the question of recurrent sensibility I simply say, "This "is never witnessed in the frog. It can only be "shown in the higher animals," and I go on to describe a method which I took from Bernard, never having conducted it or seen it performed myself; and I put it in there simply because it does render complete that little section of physiology.

2421. Then the other experiment which I referred to (of course I am not sufficient physiologist to know how many there may be of that kind) was the experiment on raising the temperature of a healthy frog till it dies?—I beg your pardon, the healthy frog jumps out.

2422. No. I think that the description is that you put a net over to prevent it jumping out; that it is the object of this experiment, to raise the temperature of the water containing a frog secured by net-work?—Directly the frog becomes uneasy, one stops the experiment.

2423. But you have given no explanation of that at all in the book?—Perhaps I ought to have done so. I think you will say that there is nothing in it to lead you to suppose that you go on until the frog is dead. That is perfectly unnecessary. Directly the frog begins to move you have got the result of your experiment; you need not go on any further. The point is that the one frog is motionless, and that the other frog begins to move at a certain temperature. I put a gauze over it, so that you may have two or three repeated movements, that is all.

2424. I am very glad indeed to hear that explanation, because I think everybody would be mistaken as to the meaning of that part of the book?—Of course one has a great deal to think of in writing these directions, and in avoiding one difficulty one often falls into another.

The witnesses withdrew.

Adjourned to Monday next, at two o'clock.

Monday, 25th October 1875.

PRESENT:

THE RIGHT HON. VISCOUNT CARDWELL, IN THE CHAIR.

The Right Hon. Lord WINMARLEIGH.
The Right Hon. W. E. FORSTER, M.P.
Sir J. B. KARSLAKE, M.P.
THOMAS HENRY HUXLEY, Esq.
JOHN ERIC ERICHSEN, Esq.
RICHARD HOLT HUTTON, Esq.

N. BAKER, Esq., Secretary.

Dr. JOHN ANTHONY, M.D., called in and examined.

Dr. J. Anthony, M.D.

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2425. (*Chairman.*) I think you were formerly a pupil of Sir Charles Bell?—I was during the period of his connexion with the Middlesex Hospital, to which I was attached.

2426. Were you selected by him among his pupils to fulfil the duties of dissector?—I was.

2427. Will you have the kindness to explain to the Commission what those duties were?—Sir Charles Bell's lectures being principally on the nervous system, or some of the functions arising from the distribution of the nerves, it was necessary that the dissections should be of a particularly neat and clean character. I was selected from 70 pupils for the purpose of performing those dissections, and having everything necessary in readiness for Sir Charles to lecture.

2428. At that time anæsthetics were not introduced?—No.

2429. Any experiment therefore which was in itself of a painful character must necessarily have inflicted its full amount of pain upon the animal experimented upon?—Quite so.

2430. Was Sir Charles Bell particularly careful in that respect?—I never saw him operate upon living animals; I prepared the subjects for his lectures on the nervous system; but at that time Sir Charles was not in the habit of giving lectures on vivisections; it was on demonstrations of his discoveries as to where the nerves were supplied in the dead subject.

2431. The cases in which you acted as dissector were not on the living animals?—Not on the living animals.

2432. Does it fall within your knowledge to speak at all as to Sir Charles' treatment of living animals?—I never saw it.

2433. Have you any means of saying whether he was very tender?—I can only speak from hearsay, from the statements of those who knew him well, that he was most kind and most feeling.

2434. That is to say, I presume, that he limited the infliction of pain within the narrowest limits possible?—So I understood.

2435. You were afterwards, I think, for a long time a student of physiology abroad?—At Paris.

2436. Speaking generally, have you any remarks to make about what you considered to be the tone of feeling on this subject there?—Among English or French do you mean?

2437. I mean generally in the lectures that you saw there, whoever were the people?—The English part of the class partook with myself of a degree of indignation at the cruelties which we saw manifested in the demonstration of the experiments on living creatures.

2438. How long ago was this?—In 1838.

2439. Looking to the published medical journals of the period, are you able to lay before the Commissioners from them, instances of what you consider to have been objectionable practices on this subject?—I have every reason to think that I can do so. My memory serves me, that the text books of the period were rather proud of giving the detail of these experiments; and therefore I think I can give a good résumé of such experiments.*

2440. So that if a certain time be allowed you, you think that you can substantiate from the published medical journals the statement which you have made generally about the state of things when you were at Paris?—Yes.

2441. Have you considered the subject which has been referred to us?—Very carefully.

2442. Do you consider that there is at this time a disposition to extend in this country the practice of vivisection?—I have every reason to think so.

2443. Do you think that it is desirable to take any measures on the part of the legislature here, to limit the treatment to which living animals may be exposed in this country?—That is a difficult question. Perhaps it would simplify matters if I were to read a short paper on which I have put down what I think would be advisable, and then your Lordship could put any question touching why I think so.

2444. If you please will you do that?—I think in the first place it is desirable that for the advantage of human beings, observations of the vital processes involving experiments on living creatures throughout the lower range of the animal kingdom, should be permitted. I take that as a postulate. Secondly, that such permission should be given in the form of a license to such persons only as have already shown great aptitude for the study of physiology, or the cognate sciences. Thirdly, that stringent regulations should be attached to such licenses, involving the giving of every facility for the inspection of the experiments by proper officials. Fourthly, that nothing in the nature of a public demonstration of vivisectional experiments of any kind should be permitted, such exhibition being perfectly useless on scientific grounds, very demoralising in its tendency, and only serving to minister to a morbid curiosity. I shall be very happy to say why I have formed such an opinion, as differing from the idea which I know now obtains with regard to the institution of laboratories.

2445. On the first point do you consider that observations of the vital processes, in involving experiments on living creatures, are absolutely necessary to the successful prosecution of physiology?—I think so. I think it is impossible that you could judge in the least, from the dead appearance of a nerve, what its functions would be. In fact when observations were made from those sources, persons arrived at the conclusion that all nerves were alike; a nerve was a nerve and very frequently for the relief of ticdoloreux, which is an exceedingly painful affection, before the days of Sir Charles Bell a section was made of a large nerve, which was observed to ramify over the face, and to the astonishment of the operator that did not relieve the pain the least in the world, but only took away the power of movement. In other words we know now that it was the nerve of motion. Nerves appear to be small thin white cords, some slightly redder than others; but we know now from experiments performed on living creatures that of these nerves, some are nerves of motion, some of sensation, and some of what we should call co-ordination. Others are nerves of special functions, just as the heart would beat day and night for three score years and ten; but if you stick it full of pins the individual's heart would not be conscious of it; the nerves that supply that, are nerves of special functions. If I take my microscope I merely recognise the nerves as very diminutive tubes filled with dead matter, looking almost like the coarse meat of sausages; and that is all we know about them in a dead form.

2446. You mention that to illustrate your first position, that it is desirable or necessary that observations of the vital processes, involving experiments on living creatures, should be permitted?—Yes; I named the lower range of the animal kingdom, that is to come as near our species as moral and religious grounds would permit.

2447. Now your next position is that such permission should be given in the form of a license to such persons only, as have already shown great aptitude for the study of physiology or the cognate sciences. You think that the persons qualified to turn such experiments to any real scientific use are a special class of persons?—Quite so; there would be no difficulty in identifying such persons in the profession, and I will name why I prefer the form of a license to giving any permission generally to attend at a laboratory. It would be probable for any man who was worth his salt, as a discoverer to be jealous; he would not allow for a single moment any person who might have access to that laboratory to witness what he was doing. If he were himself conscious of having the power of experimenting and so carrying it to great results, he would be most careful that those about him should not have an idea of what he was doing. Discoverers are jealous. Whatever discovery has taken place, it has never taken place by three or four men coming together and talking over something that has been done. It has been done in the quiet of a man's own atelier or work-room. I think the astronomer is best alone in his observatory, and that the microscopist best alone with his instrument. If three or four are gathered together, there is no work, it is mere talk, and there is no discovery.

2448. Then in your next proposition you say, that stringent regulations should be attached to such licenses. Would you kindly indicate to the Commission what is the nature of the regulations that you would propose?—I think that there, the greatest pains should be taken that the cruel element should be kept down as much as possible. What I saw in Paris (I do not know whether this is evidence or not) pointed to this, that very frequently men who are in the habit of making those experiments, at all events the French, are very careless of what becomes of the animal, when it has served its purpose. The brain is exposed, portions of it are cut, or pinched, or torn, and then the animal having served its purpose, is thrown on to the floor, to creep into the corner and die. I would give the license under such restrictions that everything approaching to cruelty should be prevented, and so that when it was possible, anasthetics should be given, either chloroform or ether, or the protoxide of azote, to negative the sensation when the experiment permitted of it. In other words, I would make it as merciful as I could.

2449. The object, as I understand, of the regulations

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* Appendix III., § 3.

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would be to diminish the suffering of the animal; but are you prepared to give in detail any specification of regulations which, in your opinion, it would be judicious to attach to such licenses?—I have never considered that subject; I have merely proposed it in general terms. There would be no difficulty in filling up a sketch picture afterwards with the necessary detail.

2450. Fourthly you say, that nothing in the nature of a public demonstration of vivisectional experiments of any kind should be permitted, supposing that the creatures that are the subject of those experiments are so treated with a complete anœsthetic, as to be entirely insensible to pain, would you still adhere to that?—I would still adhere to that, I have thought that over very carefully; and I believe once you admit the principle that it may be allowed under certain circumstances, you would introduce the thin end of the wedge, it would be a matter of discussion about what was and what was not a legitimate object for exhibition. I believe that the more you keep the scenic element away the better. All that is done that is valuable to science is done in the quiet of a man's laboratory or room, as it may be, and then he communicates it to the body of the profession or physiologists in a proper manner, and it is so accepted. If that statement should be doubted, the experiment can be repeated by others, whose names would also carry weight, and then it comes before the profession in a proper manner. The reason of my stating that I would keep this matter as much as I possibly could from the public, is the existence of morbid curiosity. There is a morbid curiosity which is known to medical men well with reference to operations of all kinds. There are a certain number of persons who are very fond of coming to see the different operations at the hospitals. I look upon that, and particularly upon the desire of seeing these experiments on animals, as something very very morbid indeed. I may say I never gained one single fact by seeing these cruel experiments which I have seen in Paris. I know nothing more from them than I could have read in the current journals, or in the works of physiologists.

2451. We were told the other day by lecturers of great eminence, that in the first place demonstrations are never exhibited in this country except under complete anœsthesia, and secondly that the tone of feeling among the professors, and among the pupils also, there being two separate statements, the one regarding the professors, and the other regarding the pupils, was such that it would not be tolerated for a moment?—I am very glad to hear that.

2452. Now supposing that to be the tone of feeling and the anæsthesia to be complete, would you be afraid that a demoralization would follow if that practice were permitted to continue?—I think it a bad practice; I think it would be sensational instead of educational.

2453. Are there any other suggestions which you wish to offer to the commission?—There is one point that you have not referred to, on which I would say a word, namely, that it should be one of the conditions of the licenses that the atelier or workshop or place should be open at all times, that every facility should be given for inspection by proper officials, I mean by that, inspection by those persons who would not care in the least what the functions of the nerves or other parts of the body might be, but would simply take care that the thing should be carried on in conformity with regulations laid down. There would be no difficulty I think in carrying that out, and no jealousy caused by it.

2454. But supposing one of these jealous discoverers of whom you have spoken to be engaged in a series of complicated experiments, do you think it would be possible to have an inspector put over him, so that the two objects could be combined, the efficiency of the inspection, and the successful prosecution of the experiment?—I think so; because the inspector having access at all times to the place where these experiments were being carried on, if a man of intelligence, would have no difficulty in arriving at a conclusion whether the license was acting in conformity with the regulations. Public opinion would soon bring that right. I merely wish to prove that instead of (as has been proposed I believe), having ateliers or galleries to which the public should be admitted; that is taking it that publicity would be the best safeguard against cruelty, I think that this arrangement would entail practically, that no man who was making a series of careful experiments, would care to go to a place and make those experiments with a parcel of people chattering in a gallery round him, or persons looking on.

2455. But on the other hand you are not afraid that with such a proposal as your own, one of two things might happen, either that the inspection would be perfunctory, and the real severity inflicted at same times when the inspector was not likely to come, or that if it were really efficient, the jealousy of the experiments would operate as a difficulty in the way of accomplishing the scientific results desired?—I see where the difficulty would lie, and I guard against it, so far as I possibly can. In the first instance, I would not grant a license to any man except he was, in the general opinion, I may say, of the members of the profession, a fit and proper person to carry on those experiments; and then I think that if you must have safeguards, considering that the man if he is to be licensed, has got work to do, that work would be better done, and the cruelty would be guarded against better, by inspection by a proper official, who might come at any period during the experiments, than it would be by having simply a number of the public present in a gallery who knew nothing of the regulations and nothing of the experiment, excepting that pain was inflicted. Pain there must be in the majority of experiments where the animal cannot be subjected to an anæsthetic agent.

2456. Are you of opinion that in the whole number of necessary or desirable experiments by far the greater portion can be performed under anæsthetics?—Many can be; but nothing, I believe, with regard to the experiments on the nerves of the body. If you wish to arrive at a satisfactory conclusion the less elements you have which would confuse you the better. If the animal is placed under the influence of an anæsthetic, which acts directly through the nerves, it is impossible to arrive at a satisfactory conclusion as to what, in a normal condition, that nerve is when in the fulfilment of its full functions.

2457. But we have had a good deal of evidence to lead us to believe, that of the whole number of experiments on living animals, which are to be considered as either necessary or desirable, a very large proportion may be performed under complete anæsthesia?—I think it may be so; I have no reason to suppose the contrary. If, for example, a man wishes to investigate inflammation in any particular part, he can see the effect of the inflammation just as well whether the animal is conscious that he is cutting that part or not, so far as the blood is concerned; but with regard to all the experiments on the nervous system, I think it is impossible to arrive at a satisfactory conclusion when the animal is not conscious of pain.

2458. Then we have understood also that of the experiments where there must necessarily, if they are performed at all, be a consciousness of pain, it often happens that the larger portion of the pain may be entirely removed by anæsthetics, and that the smaller portion only will remain in which the animal must necessarily be conscious. Do you agree with that?—I think it may be taken to be so.

2459. Then upon the whole, you are of opinion, that the pain to which animals ought to be subjected for the prosecution of science is limited to a very small portion of the whole number of experiments?—So far as we have gone hitherto; but I think we are merely on the threshold. I believe that much more will be done in this part of the 19th century than ever has been done in the investigation of what are termed vital functions. May I venture one observation on that? It is this, that it is impossible to deny the evidence that the animals of the lower range do not feel pain as we

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feel pain. I should be sorry to give the impression abroad any sort of bias on my part towards experimenting on animals; for I could not do it. My reason tells me, however, that experimenting on these inferior creatures of a lower order, is the only way we have of arriving at satisfactory conclusions in many very important problems; but I do not think that the cab horse, for instance, when it is whipped, feels exactly as we feel. It is not the case, as Shakespeare says, that "The beetle that we tread upon feels a pang "as great as when a giant dies;" we know that is not correct now. The lower you go down in the scale of animated nature, the less you find of what we call nervous sensation. The lower animals have the nerves of special functions, for the beating of the heart and the inflation of the lungs for example; but if these creatures receive a cut or a blow, I do not think that they would feel it as the higher animal would do.

2460. (*Lord Winmarleigh.*) With regard to that last answer, what are the proofs of that which have been made public?—I do not know whether you are aware that a Dr. Badham, who went into that subject, wrote a small book on it; he was an Oxford man and a travelling professor, and he made it his particular business to collect evidence, and himself to go into experiments on that subject; and he went as high up as he possibly could to get evidence; but he also went low down among the articulata. Certain animals, you know, have the power of throwing their own limbs off, as crabs and lobsters. Experiments of that nature have been made with flies; the fly is so fond of sugar and water that you can cut the extremities of a fly's legs off and the creature will go on sucking although the extremities of all its legs are cut off. This fact has been shown in museums again and again. Taking the predations beetles, the carnivora, a pin has been stuck through them and they have been thought to be dead and have revived, and have drawn the pin away, and walked off to a neighbour, and eaten it.

2461. Is that a proof that none of the inferior animals suffer pain in the same way as the higher ones?—My point is that none of the inferior animals suffer in the same degree what we should call the sensation of pain. The nervous system in them is a system of what we should call the nerves of organic life to carry on the breathing and locomotion; and I presume that if an animal finds anything annoys it, it has modes of expressing its disapprobation. But mammals have the same nerves of organic life, with a brain superadded; and hence their sensation of pain is very different.

2462. Have those proofs been so far established that that they are recognised by physiologists as indisputable?—I think I may say so. I do not think that what I am now telling you is mere matter of opinion, it is looked upon as matter of fact.

2463. At the same time is it not the fact that animals, when they are subjected to certain modes of punishment, do exhibit a sense of very great torture; I will take for instance a dog?—That is a mammal. I was speaking now principally of the lower animals; when you get down to the articulata, the coleoptera, and the neuroptera, in fact insects in the lower range of animals.

2464. The horse of which you were speaking just now, is a mammal also?—Yes, but it belongs to a lower species than we do. We belong to the very highest species in the range, and I believe that by taking a descending scale, the lower you go down, starting from the highest mammal down to the lowest, you will find a gradual absence of what we should call ordinary nervous sensation.

2465. But seeing that a horse under certain operations groans in a dreadful manner, is not that a proof that it is suffering pain?—I would not for a single moment say that the animal does not suffer, and suffer acutely. I am afraid that what I am now saying may open possibly a door for those who would wish to carry on experiments, cruelty or no cruelty, but what I say is that those lower orders of the creation that are still furnished with a large number of nerves, are furnished principally with the nerves for carrying on organic life. You may pull their limbs off, and they seem to manifest no particular disapprobation. It must be familiar to all that if you put your finger on a daddy long legs, as it is called, the animal will fly off, leaving one of its limbs behind, but he does not seem to suffer from that.

2466. But are there not parts of the daddy long legs that are affected with great pain?—I think not. I have no reason to think that there is any particular pain that can be felt by it. We have reason to think that with regard to the creatures that seem to be required in very enormous number for food for others, it is a merciful arrangement that they should not feel the pain; that they live on in happiness till the day of their destiny comes.

2467. Have you ever paid attention to the comparison between the discoveries made by the French physiologists, and those made by the English physiologists?—Yes, I may say I have.

2468. Which should you say have have contributed most to the service of physiology, the French or the English?—I say that nothing that the French have ever done bears, for a single moment, any comparison with the discoveries of Sir Charles Bell. I happened to be a pupil at a period when Sir Charles Bell and Herbert Mayo were the English physiologists, and Magendie was their rival in France.

2469. So that you think that the English system, where there is less cruelty, has been equally beneficial to physiology with the French?—I think infinitely superior.

2470. You stated that you thought no person should have a license to practice experiments in physiology unless he was known to have qualifications for it?—My reason for stating that is that it would take out to a certain extent the element of mere curiosity.

2471. But supposing that the student of physiology was not of the medical profession, what means would he have of showing his qualifications?—I think he would be certainly known to some persons of note as a physiologist, or he would place himself very rapidly in connexion with some one of the kind. If you were accustomed either to the use of the microscope or to experiments in any of the cognate sciences you would know that the first thing a man does when he gets such proclivities is to place himself in communication, through some mutual friend, with men high in the profession who have also followed the subject out. He must first find out what others have done, and get some one to put him in the way. If he has got a *furore* for carrying on experiments, advice will be given to him as to the best mode of conducting them; that is, providing it is not mere curiosity, something merely in the amateur way.

2472. Some authority, the Secretary of State, or some other public authority, must be charged with the duty of giving such licenses?—Yes.

2473. How would you enable him to test the qualification of the person wanting a license; for instance, the Secretary of State?—Supposing we take any medical school, King's College, or University College, among the number of 60 or 70 pupils, it would be very soon known to the professors that there was one particular young man who had shown a particular talent for the investigation of these subjects; he is constantly about and asking questions; he shows by his questions that he has got something more in his brain than the ordinary run of students; there would be no difficulty then in the professors joining to recommend to the Secretary of State that such a young man should be under certain regulations, given a licence to carry out the experiments which he evidently was fond of.

2474. What is the nature of the restrictions that you would insert into a licence granted by the Secretary of State, or others?—I have not considered that sufficiently, and it would be a matter for very grave consideration. You should surround the licence with such restrictions as would make the element of cruelty (which must subsist to a certain extent) as small as possible. I can see no difficulty in imagining that a licence would be granted with certain restrictions, not too many. I would make a general blocking out of my sketch before I filled in the details.

2475. In fact you think that there would be no difficulty about it?—I think there would be no difficulty.

2476. (*Mr. Forster.*) You draw a line as regards pain between man and other mammals. Do you think that there is much difference amongst these other mammals as to their sensibility to pain?—It would only be a matter of opinion. I should not be able to prove the difference between the man and the animal, they come so near together.

2477. My question was rather as to the difference amongst other mammals?—We only get it from analogy. As you go lower down from the very highest organization of the nervous system, pain ceases to be felt in the lower as it would be felt in the higher. It is a matter of degree; I think it is in proportion; with a brain weighing 3 lbs. to 4 lbs. the individual would feel pain very differently from what he would if it weighed only a few ounces.

2478. Would not dogs be as likely to suffer pain as any other animals?—Of course if you take the canine race, the dog, the cat, or any animals of that developmant of brain, I think they would all suffer alike; but I do not think that they would suffer as men suffer.

2479. But would they not be as likely to suffer pain as any animals, except man?—Yes.

2480. Why is it that dogs and cats are more taken than other animals for experiments?—They are more easily got at, and are cheaper.

2481. Is there no reason connected with the experiment itself which makes it desirable to get them for the purpose?—No, excepting that they are as high up in the mammals as could be got at a cheap rate. A sheep would be a very dear animal to perform an experiment upon. I believe Sir Charles Bell's experiments were made chiefly on a donkey.

2482. But I suppose there are some experiments which would be useless; unless they were made on animals as nearly approaching man as possible?—Yes; I think you may say it would certainly be unsatisfactory if it were brought before the profession that certain functions were judged of by a function in an inferior organization.

2483. Now I suppose you would consider that it is probable that a frog suffers less pain than a dog?—Certainly.

2484. Why is it that in some experiments that they take frogs, and in others dogs?—Because in the frog the nervous system, such as it is, is very large in proportion to the rest; and also Galvani in his experiments, having accidentally observed in touching the nerve of a frog, prepared for culinary purposes, with metals producing a galvanic current, that the limb was violently agitated, it was found that the nerves were very susceptible; but it does not follow that because the nerves were susceptible to movement, sensation should be equal. It was a great discovery of Sir Charles Bell, that the nerves of the body arising from the spine were half of them merely nerves of sensation, and the other half of them nerves of motion. The motion might be vigorous though the sensation leading to that motion might not be what we should call our sensation, a sensation of pain,—will you allow me to give as an illustration of what I have been saying about the difference of the nerves, this fact? I was saying that the nerves of motion did not depend upon sensation. Now, when a horse goes to the knackers of course he is killed, and they cut his head off. Before the knacker however begins to remove the skin of that horse, he passes a a long rod down the spinal marrow. Why? Because if he begins to skin that horse without doing that, although we cannot believe that there is any sensation, yet for an hour or an hour and a half there would be sufficient irritability in the nerves, for that horse to kick and perhaps break the man's arm, although as I say the head has been cut off for some time.

2485. You stated that you would not allow any vivisectional experiments for the purpose of demonstration. We have had some witnesses who have said that they consider that students either of medicine or surgery cannot properly be taught their profession unless they see such experiments; is that your view?—I do not think so. I think a man can understand that a medicine is a purgative, though he has not seen an operation to prove it.

2486. We had this case put before us, that it is sometimes very desirable for a practitioner to know whether a patient really has had strychnine or not, and that it would be very difficult for him to know what the effects of strychnine was, unless he either saw it in a human being (and he might not have had that opportunity) or had actually seen it in some animal; and that was given to us at some reason for the desirability of seeing an animal under strychnine?—That does not shake my opinion, because the effects of strychnine on the body are such that they are phenomena of the coarsest nature. The particular spasm induced by strychnine is such that it would be impossible for a single moment to mistake those phenomena as phenomena arising from any other other source.

2487. Then as regards merely the acquisition of such knowledge it is asserted that it is necessary for them to see the experiments in order to be good practitioners. We are told that even in such a matter as the circulation of the blood they obtain the knowledge of that by seeing it in a perfectly different way from what they do by hearing of it?—I think that that would hold true of everything throughout life. What you have seen fixes itself better on the memory; but is it necessary to go through the experiment of cutting open the chest of a frog, being at the same time particular not to kill it; and then to turn its lungs out, in order to see the circulation of the blood? Fond as I am of physiology I would not do that for the world.

2488. You have had considerable experience in the medical profession?—I have.

2489. Do you think that a man who had seen the circulation of the blood in a living animal would to any extent worth taking much account of, be likely to make a better surgeon or physician than a man who had not?—Not the least, he could merely see the circulation of the blood in the web of a frog's blood, or in a fish's gill, or in some of the very inferior animals; but we are not at all sure that the circulation of the blood in them is conducted in the same mode that ours is; they do not breathe by lungs but by pouches; a different arrangement altogether.

2490. (*Sir John Karslake.*) From what I gather, you do not practice vivisection yourself at all?—I am continually occupied in dissecting with the microscope.

2491. But not in making experiments on living animals?—No, not on living animals.

2492. Might I ask you this question, have you ever seen a dog, for instance, or any animal, under the influence of strychnine?—Yes. I have poisoned a very favourite dog myself with it.

2493. Intentionally administered you mean?—Yes.

2494. I rather gather also that it has not been any part of your duty to lecture to classes of pupils?—Not on this subject.

2495. Would you think that those who have for many years past had pupils under their charge, would be better able to form an opinion as to the value of demonstration than those who have not?—I can scarcely answer the question, because it is an opinion as to whether another person might be capable of forming an opinion.

2496. Now with reference to the license that you propose, it seems to me to be very much a question of detail, can you give us any detail; have you formed any opinion yourself as to the character of the details connected with such a license?—Yes, I have asked myself the question, what I should require, or what I would do myself. Take a case; supposing I wished to follow out experiments on the nerve which supplies sensation to the face; then I should begin to think among the lower animals, which could be got at at a

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moderate price that I could act upon; then I should begin to think whether I should require any one to assist me, either in the way of holding the animal, or in the way of performing some of the inferior parts of the experiment; then what place would be convenient for performing it; naturally the cries of the animal submitted to experiment would, as it is termed, raise the neighbourhood; you would be obliged to consider whether you had got facilities for the purpose at the place where you lodged or lived, or any outhouse connected with it. That suggested itself to my mind; therefore an individual making such experiments, that is to say, getting a license to make such experiments; would have in his own particular capacity to make a series of arrangements for carrying them out. I would allow him to make those arrangements, and not let the arrangements be made for him in the form of a particular laboratory. You might grant him the use of a room at a particular place, but he should have that room to himself, and not be subjected to be interrupted by any one except the official who came in from time to time to see that matters had been, what is called, decently conducted.

2497. Have you formed any opinion of what would be the function of that person who is to come from time to time, and inspect the operations which are going on?—Yes, I think that an intelligent person who was properly informed, would know it when cruelty was manifested, it would be made afterwards matter of conversation among the professors; I believe that the man would not stand alone who was making these experiments; he would from time to time say that he was getting on well, and hoped to be able in a short time to bring something forward that would be of value. Supposing that the official was a man properly informed I think he would be enabled to judge whether an undue amount of cruelty was being made use of.

2498. Might I ask, is the inspector, according to your view, to be an educated physiologist himself?—No; I think he would naturally be a member of the medical profession, but not connected in any way himself with experimenting. I think a man would be very jealous of any person coming into any room where he was at work on experiments, who might be engaged, or quasi engaged in the same work with himself; he might take the wind out of the sails of the other; see what he was doing, and then go and perhaps obtain priority of publication.

2499. Then he is to be a medical man, but not a skilled physiologist, as I understand you?—Not a skilled physiologist.

2500. As far as I understand it, you go to this extent, that a license is to be granted, on the recommendation of somebody, to carry on an experiment in some particular place, subject to the inspection of a person who is not a skilled physiologist, but who has sufficient skill to know whether that experiment is cruel or not?—Yes.

2501. Are there any other details which you have in your mind?—I think if you had a place furnished with certain appliances, made convenient for a student of physiology, he might be assigned, as the French say, a cabinet, of which the individual should have himself the key, and he should be enabled to ask anyone else to come and assist him in that particular cabinet; but that he should not have to go to a public laboratory to which anyone could have access, and in which every person who wished could see what he was doing from the gallery.

2502. Your view is that the license should exclude everybody from his laboratory except what may be called an assistant?—Yes.

2503. And that no one should be allowed to see the experiment going on except the official?—Yes; and that the official, as people in common language would express it, should be the person to see fair play.

2504. Are there any other details which occur to you in the license which you would propose?—No, I think not.

2505. Have you considered at all what the consequences would be of a person who is an eminent physiologist doing that which the unskilled inspector considers to be cruel?—That is going into a series of possibilities that one has scarcely contemplated. Of course one knows that such things must be, but they are generally provided for by those who are more skilled in seeing how you should provide for them than I am. If it were noised abroad that Mr. A.B. was a cruel wretch, I think there would be no difficulty in bringing him before the tribunal of public opinion.

2506. But supposing the physiologist is a very eminent man, and the inspector a man of no skill at all compared to the eminent physiologist, and he were to say, "I have seen an experiment to-day performed by Dr. so and so which inflicted great pain," do not you think that the profession would give credit to Dr. so and so rather than to the inspector?—Yes, I think if he were a man of weight, he would be able to justify himself.

2507. But you would not grant the license to a man who was not a man of weight, as I understood you?—No, I did not say that. I said a man who showed a peculiar aptitude for the study of physiology. For instance, he has got a degree, we will suppose, but still he is a young physiologist; he has got young blood, and young brains, and he devotes himself to physiology. He might be a cruel creature, and that would soon be found out, and he would be made amenable to public opinion. In fact the very fact of his having done anything cruel would vitiate his license; because I take it that the license would only be granted from year to year. I would make the license annual, or for some definite period.

2508. (*Lord Winmarleigh.*) Would you apply that to the heads of the school?—They would submit to it for the known good that would arise out of it in the other case.

2509. (*Sir John Karslake.*) Do you know of any instances yourself of young men, from mere curiosity, carrying on these experiments?—Yes.

2510. Can you mention them?—No, I am prepared to say that I could mention them, but I should scarcely like to do so.

2511. I want to know whether there are instances under your own knowledge at the present moment of persons, from mere idle curiosity, with no sufficient reason at all, carrying out these experiments?—I am afraid there are.

2512. You cannot give instances?—No.

2513. Where do they exist, in London or the provinces?—In the provinces. Of late years all my experience has been in the provinces. Of course I have juveniles coming forward to ask me sometimes with regard to various points of physiology, and they tell me candidly what they are doing, and I am obliged to shake my head occasionally.

2514. Are these young medical practitioners?—Yes.

2515. Who, in your judgment, are engaged in vivisection without any justifiable object, and merely from curiosity?—Merely from curiosity.

2516. How long has that state of things been in existence, in your opinion?—A number of years.

2517. I ask you that question not as an opinion which you have formed of a thing that may be, but of a thing that exists?—Yes; guinea pigs seem to be the creatures that are the greatest favourites for being operated upon by those who do not wish to go to great expense, and kittens, and cats, and stray dogs, now and then.

2518. In these cases are anæsthetics used in your opinion?—No; or if an anæsthetic is used it is only to watch the effect of an anæsthetic; but an anæsthetic is not used to diminish the pain of the creature; the man using it would be one who wished to try the effect of what that anæsthetic would do.

2519. But you are really of opinion that there are persons who carry on these experiments, and who do not use anæsthetics, unless it is convenient for them to do so?—Yes; I think it is not from want of feeling, but they do not think about it. They want to know

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something, and the creature is utilised for that particular purpose.

2520. And you cannot tell us more specifically where, in your opinion, this state of things exists?—No.

2521. Is it done in private houses?—Yes.

2522. And are several of them present at the same time?—Yes.

2523. And they do not feel among them any shame in performing these operations?—Not the least.

2524. Having stated that, do you say you cannot give us any further clue, so that further inquiries might be made?—I am afraid I must refuse it, on the ground that it is knowledge I have come at, and which was given to me guilelessly without any idea of harm. I could not drag down public opinion on friends or acquaintances who have informed me that they have done certain things of this sort.

2525. But do you think that your shaking your head as you say, has had any effect upon them?—Yes, because I have gone further; I have told them very candidly what I thought about the matter, and I think strongly about it; and I think it has had an effect. When a man gets at all enthusiastic over matters he is very apt to disregard a mere "jo-bation" as he would call it.

2526. As I understand it, you cannot give us any further clue, or any clue at all in fact, as to where these practices exist?—No, merely that I have the consciousness that the thing is done, has been done, and probably will be done.

2527. And that is in the provinces, and not in London? — My knowledge does not extend to London.

2528. (*Lord Winmarleigh.*) Does that which you have been speaking of prevail to any great extent?—No, merely on the part of a few enthusiastic students.

2529. (*Mr. Huxley.*) Will you be so kind as to tell us what is the nature of the experiments to which you refer?—Experiments principally on the subjects connected with parturition, and also with regard to the circulation of the blood, and the effects of inflammation. If you take a frog's lung, and put a drop of acetic acid upon it, or make a scratch with a fine needle, in a few minutes you will find the effect of what would end in inflammation when it occurs in a higher being. It is very interesting and instructive to look at, but it is cruel to the frog.

2530. If then a person were to fasten up a frog's foot in the ordinary way for the purpose of seeing the circulation, which is an operation which gives no pain, and if that person were to put a little drop of acetic acid, or to make a scratch with a pin so as to see those very wondrous phenomena of congestion, (which probably few persons can imagine unless they have seen them,) if, I say, a medical man does that for the purpose of understanding better than he understood before the nature of the process of inflammation, do you consider that a barbarity which should be put down?—Certainly not; I have done it myself often enough.

2531. But that is one of the experiments which you have mentioned. I gather from you now that you do not consider that a case worthy of being put down by legislature?—Not if you confine yourself to merely watching the circulation of the blood in the gills of certain fishes, or in the web of a frog's foot. The frog can be put under chloroform if you like, and the circulation goes on just as well if you subject the frog to chlorofrom, giving him a small portion of the air to breathe. You use chloroform not to remove pain, because I believe the creature does not suffer pain from a mere scratch of a fine needle, but that he should not, as he would naturally and instinctively do, pull his toes away. You do not want to destroy the sensation of pain, but it is simply that then he loses a portion of the irritability, and he does not instinctively draw the part away.

2532. I wish to get back to our previous position. You stated to Sir John Karslake, that within your own knowledge medical practitioners in the country were in the habit of performing experiments which you considered cruel and unnecessary?—Yes, they have come to my knowledge.

2533. You were kind enough, in reply to my question as to the nature of those experiments, to mention the nature of those experiments, and among them you stated that the watching of the development of the inflammation of a frog's foot was one?—That would not be part of the cruelty, the watching of the circulation or the effect of inflammation on a frog's foot. I named the cutting open a frog, and turning the lungs outside, and being very careful at the same time not to kill the animal, and watching the inflammation when the lungs are outside. There was the cruelty. It was not the watching the inflammation, or the scratch with the needle, that produced the inflammation of which I spoke; it was preparatory to that. You are very carefully directed in certain books on these subjects, how you are to cut the creature in such a way as that you will get the lungs entirely external, and not kill the animal.

2534. In such books as I am acquainted with, it is stated that that experiment may be performed after the frog has been pithed or its brain destroyed, and as every experimentor knows, it is more convenient to have that done?—Yes.

2535. In the cases to which you refer was nothing of that kind done?—Nothing of that kind was done.

2536. But such experiments as those which you have mentioned are those to which you have referred as being the experiments which need legislative interference?—They would merely come in as part of what I have heard. Mind I have not seen anything of the sort. I would not see it. No sum would tempt me to go and see these things performed.

2537. Will you be so good as to tell the Commission what is the severest kind of experiment amongst those to which you refer, so that we may have the extremest case clearly before us?—Mind, I can only go upon hearsay. I would name the experiment on the generative parts of guinea pigs, on the fallopian tubes, portions of the uterus impregnated and unimpregnated.

2538. For the purpose of repeating Bischoff's experiment, do you mean?—Yes, probably.

2539. The experiment consists in tying one fallopian tube and leaving the other, does it not?—Yes.

2540. Have you ever seen that experiment done?—Never.

2541. You therefore cannot tell the Commission how long the operation lasts?—No; since my studies in Paris I have most carefully avoided seeing anything of the kind.

2542. You cannot give the Commission any idea of how long the operation lasts, or within what period of time after the operation a guinea pig that has undergone it is in its usual condition?—No, I know nothing of it. If I heard that such a thing was going on in the next room, I should walk out.

2543. You have no personal knowledge of it in fact?—No, but I believe that such things are done, from conversations which I have held.

2544. And the case which you have just put before us is the worst?—I have never heard of anything worse.

2545. (*Sir John Karslake.*) Might that operation be performed under chloroform?—Yes.

2546. (*Mr. Forster.*) Although you have never witnessed the operation, and would refuse to do so, have you an opinion whether it would inflict much pain?—The mere tying of the fallopian tube would probably give no pain. A physiologist would know that the interior of all animals is not gifted with nerves of sensation. It would be in the cutting through of the skin that there would inevitably be pain. There would be the painful part. The painful part would not be in the physiological experiment of trying the fallopian tube; it would be in cutting through the skin to get at the fallopian tube.

2547. (*Mr. Huxley.*) You are doubtless familiar

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with the fact that the operation called "spaying" is largely performed on sows?—Yes, I have seen it.

2548. Would you state whether the experiment which you have just referred to would inflict one whit more pain than that operation of spaying, or as much pain?—No, it would not inflict nearly so much.

2549. So that an operation which is constantly performed on sows, in order to get them to fatten a little faster, is an operation decidedly more painful than that which you say is carried on for scientific purposes?—Yes.

2550. I should like to ask you a few questions about a statement which you have made; not because I differ from you, but for the purpose of bringing out the grounds of your statement. You stated that in your belief the animals of lower organization than ourselves in all probability suffer less pain than we do?—Yes.

2551. I think it might be said that that conviction in the minds of physiologists arises, I may say, from some four grounds; that is to say, that when we suffer pain from any operation or injury, there is in the first place a very large amount of the pain which arises from what I may call the hyper-æsthesia of civilisation?—Yes.

2552. You are doubtless familiar, as every physiologist is, with the fact that one of the most painful events in the life of a woman, parturition, which to civilised women living under ordinary conditions of life involves long hours of agony, is got through by savage women without any inconvenience; that in fact, a savage woman will often get up and carry a burden within an hour after it?—I have seen that myself abroad.

2553. I have asked you whether the conditions of civilisation in the first place do not give rise to a great deal of what we feel of pain; and I understand that that is your opinion about it?—Yes; but supposing you take it not in the adult but in the child, you can have no moral consideration connected with it. I do not know whether you would consider that a series of generations might have acquired a greater amount of delicacy in organisations, so that both adult and child would participate in it.

2554. That it another consideration. Then there is another source of pain which is very important in the case of human beings, viz., anticipation?—Yes.

2555. Then thirdly, there is a very great source of pain following upon injuries in the human subject, and in the civilized human subject especially, which is what you know familiarly enough as constitutional irritation?—Yes.

2556. That, you will bear me out, does not prevail in animals as it does in the human subject?—That is so.

2557. And then there is a fourth element in pain, what we may call necessary pain, that which must under all circumstances follow?—Yes.

2558. Then there comes another great difficulty about judging of pain in animals, and I am particularly anxious that the Commission should hear from you, as one having very strong humanitarian views, what is the true condition of that question. One of the great signs of pain is struggling or crying, is it not?—Yes; and you are aware, better than I am, that as in the case of what happens in the case of the knacker and the horse to which I have alluded, struggling does not always signify pain.

2559. Taking the case you mentioned just now, you have in the horse all the signs that we commonly have of violent pain, when it is as morally certain as anything can be, that the animal is insensible to that pain?—Certainly.

2560. I am particularly glad to get this from you, because only two or three days ago we had brought before us a charge of cruelty against a teacher of having shown that which was cruel to a class of young girls. It was based on this statement, that the teacher had bought in a shop a lobster, which she believed to be dead, and had brought it to this class; and the allegation was that she cut the lobster to pieces, (it turned out when the matter was sifted, that that simply meant that she cut it,) and then, under those circumstances, the lobster moved. Now let me ask you, was that even the shadow of evidence that the lobster had any sensation?—No.

2561. I understood you to say just now, that you thought that Sir Charles Bell's discoveries had been of vastly greater importance than anything that has been done in France?—I think so.

2562. I do not wish to dispute your opinion; but may I ask if you have been familiar with Claude Bernard's researches of late years?—Yes, I know them generally; but I take it that the discovery of Sir Charles Bell with regard to the nerves arising from the spinal cord takes precedence over everything.

2563. Then you spoke of the jealousy of experimenters towards one another. I daresay that there is such a feeling, and no doubt it does obtain to a certain extent, but I do not think that it obtains quite to the extent that you suppose; and my reason for saying so is this; I do not know whether you are aware of the organisation of physiological research in Germany?—I do not know how far I can claim such knowledge; I have lived in Germany some time.

2564. You know Ludwig's name very well, no doubt?—Yes.

2565. You are aware that at Leipzig Ludwig has the finest physiological laboratory in the world?—Yes.

2566. And that for many years past there have been carried out in that laboratory, really under Ludwig's superintendence, a great series of most important researches, which researches are carried on by persons trained by him. Those persons have all the credit of the researches, and Ludwig has never shown the slightest jealousy of those persons?—Would you not rather say that that was the exception rather than the rule?

2567. I think that the same thing is going on in France at the present time. Claude Bernard has had many distinguished pupils; Müller of Berlin had many distinguished pupils; and Pflüger of Bonn has had many distinguished pupils; and the experience of the last 20 years has been that experimenters have tried by all means to get persons to work in their laboratories?—No doubt. But where a man wished to follow out a particular course of experimentation himself, do you not think that he would do that in what the French would call a cabinet particulier?

2568. No doubt if Ludwig wishes to follow out a particular research he does it in his own room, but that does not prevent others from making researches in the laboratory under him, does it?—I would ask whether you think that such discoveries as Sir Charles Bell made would be best got at in a public laboratory?

2569. I will put it in this way; do you see any objection to a public laboratory, supposing that the persons who got there have their own private rooms?—None; because that would bring it under the conditions which I named, viz., that it should be an essentially private room provided with conveniences.

2570. But I apprehend that the proposal made here, that there should be public laboratories at which research should be carried on, means laboratories under something like those conditions?—I thought not. I thought it meant laboratories where there would be a sort of gallery where the public would see what was done, and I say that because that was named to me in correspondence; and it was to point out the objection applying to that that I named the matter.

2571. I observed that when speaking of public surgical operations, you mentioned that there are some persons who go there as persons go to see a hanging, because they like to see it, and no doubt that is very condemnable; but do you think that that fact is any valid reason against permitting public operations?—No. It is like people getting up a party of pleasure to go to a battle for the purpose of seeing it.

2572. We all know that in the theatres attached to surgical hospitals the persons who are in those theatres while operations are going on are serious students of medicine; and I presume, although there may be in a a large number some three or four persons of morbid curiosity, yet you would not for a moment say that that is the case with regard to the vast majority of persons who attend those operations?—No, certainly not.

2573. So that that fact would constitute no argument against permitting public operations?—No; on the contrary, I have recommended certain persons to go and witness these operations; for instance, young clergymen, because I think that they may be placed under conditions in which it will be necessary for them to be able to stand the sight of blood. Books are written and lectures are given to enable a young clergyman to meet the emergency of some particular thing which might happen in his own place where he might be present; and if he was a poor creature who had never seen blood flowing, he might faint as much as the sufferer.

2574. I daresay it is within your experience that to a young student beginning his career there is something inexpressibly shocking in seeing a dead man laid out on a table?—Fearful.

2575. But by degrees a medical man becomes used to it, and undoubtedly does not think so much of it as other people?—One of the arguments against the profession being jurors is that they are too much accustomed to it; that they are hardened.

2576. But although that hardening is very necessary for them, do I understand you to think that it makes the men who are so hardened in relation to that particular matter one whit less humane or less tender-hearted in all other relations?—No, I do not think it does.

2577. You doubtless have a very extensive knowledge of the medical profession, and would you say of the medical profession that they are hard hearted and careless of cruelty?—No, on the contrary every day of the week they show that they have kind feelings by doing kind acts without being paid for them.

2578. Then to apply all that to the subject before us, why is it so certain that persons who pursue physiology in the same sense as a surgeon pursues surgery, must be hard hearted if they are witnessing operations not a whit more shocking than surgeons see in the operating theatre?—Well, I take it that they get the same sort of feeling as the woman is said to have who skins eels—she is so accustomed to it that she cannot realise in the least that the eels feel anything—it is part of her business. And I think that the continual sight of animals being acted upon, particularly if the observer feels any enthusiasm for the pursuit, in a very short time blinds the man's sense of humanity.

2579. I understand you to say by that, that he becomes blunter in regard to the inflictions of that particular kind of pain?—Yes.

2580. And we agreed just now that though the surgeon becomes blunter with regard to the particular kind of pain which he sees in the theatre, yet it makes him none the worse man in other relations?—Yes.

2581. And in the same way a person who is continually making experiments on animals is not so qualmish in regard to that subject as other people are?—He would think very little of cutting off the top of the skull of the next pigeon or kitten he might get, in order to satisfy a simple curiosity, a thing that another man would not do.

2582. Now do you base that conclusion on simple reasoning or on experience; because, if I may venture to say so, it in no way accords with my experience?—I have carefully avoided seeing experiments in vivisection after the awful dose which I had of it in Paris in 1838. The men there seemed to care no more for the pain of the creature being operated upon than if it were so much inorganic matter.

2583. And how far, may I ask, do you think that that arises from the peculiarities of the character of the nation, and how far from their scientific pursuits? How far, in fact, do you think that the carelessness was the result of scientific enquiry, and how far of individual character?—I think that nationality has something to do with it.

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2584. I am anxious, as you had personal knowledge of Sir Charles Bell and his work, to ask you one or two questions on that point. I suppose that although Sir Charles Bell did not actually experiment before his students he used to describe his experiments before the students?—No. I am not conscious that in any of the lectures that I ever attended of Sir Charles Bell he went into any details of how he had done the experiments.

2585. I did not mean to ask whether he went into details; but when he was talking about the functions of the anterior and posterior roots of the nerves, I presume he did not fail to tell the students that his conclusion as to the functions of those roots was based on experiment?—Of course he could scarcely describe them without saying that.

2586. And I presume that he insisted strongly on his experiments as evidence?—Yes.

2587. I ask you the question because the statement has been made before us, or at all events it has been made elsewhere over and over again, that Sir Charles Bell did not think much of the value of his own experiments, and that he thought he could have arrived at the same conclusion without them; is that the result of your experience?—No.

2588. To me it is a very extraordinary statement, and it is to you also, as I understand?—It is a very extraordinary statement to me, and I cannot reconcile it with my own observation.

2589. In point of fact Sir Charles Bell was very proud of his experiments, was he not?—He was.

2590. (*Mr. Hutton.*) You are aware that Sir Charles Bell, in a passage which has been put before this Commission, spoke very strongly indeed of the large number of wanton experiments which were made by physiologists during his time. Are you at all aware to what experiments he alluded, whether to those in this country or in any other country?—I think that he referred to Magendie's experiments. I am not quite sure whether Flourens preceded him.

2591. An opinion has been expressed by one of this Commission, that physiologists are too busy and too fully employed ever to make what could properly be called wanton experiments. Now that, I take it, was not the view of Sir Charles Bell, and I gather from you that it is not your experience of physiologists?—No, I think not. They have an end, and that end I believe to be a good one. It is merely that I regard the end as not justifying the means.

2592. With regard to that experiment of the fallopian tubes, I noticed that Professor Huxley, in putting his question, spoke of that as made for a scientific purpose, but I understood your objection to that kind of experiment to be that it could be made for no scientific purpose, but only for a demonstrative purpose?—No; I must have misunderstood the question if I said so. That was merely taken as one, and many more experiments of that kind would be made by men who were anxious to carry on, say, the experiments of Professor Simpson of Edinburgh and others to a higher degree.

2593. Then it was for a scientific purpose, was it?—Yes.

2594. But you said that you thought they were not made by men adequately educated for the purpose of discovery?—They were made by young men who were not controlled, as I should propose, by men who have shown a particular talent for that investigation. They were probably done for the purpose of gaining a little notoriety.

2955. And without any sufficiently useful result?—Probably repeating experiments, or trying experiments suggested to them by others.

2596. With reference to that hyper-æsthesia of which Professor Huxley has spoken as existing in man, does not it exist in the higher bred lower animals too; for instance, there is almost as much difference in regard to suffering between a high bred dog or horse and a low bred one, as there is between an Englishman and a savage, is there not?—I am inclined to think so; that you have brought both under the

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influence of what you may term civilisation. And I think that the human body is continually undergoing alteration, in fact that its functions are kept up to the highest point, and we have every reason to think that the nervous system will participate. Take a case among the animals; take the pointer or setter; you have elevated its condition of smell, and the development of the small bones of the nose, and the distribution of the membrane, and you have reason to believe that as you have educated that dog you have brought him under the particular conditions of the influence of civilisation, and so his feelings would be modified.

2597. Do you think it would be possible to exclude from the number of living animals liable to experiments animals likely to have that kind of hyperæsthesia; and that if dogs and horses were excluded the suffering would be diminished thereby?—I do not think that, taking the mere genus, it would necessarily follow that all the animals would be of the same excess of sensation. Take the bull-dog; I believe that the bull-dog and the Italian greyhound would be entirely at opposite ends of the scale, though they would be of the same species.

2598. I merely meant that if you were to take two large species and exclude them altogether from experiment, such as the dog and the horse; would not that exclude the animals most liable to extreme suffering; would it not be possible by excluding horses, dogs, and cats generally to diminish very largely the amount of suffering?—I think you might very fairly take the amount of intelligence as almost a measure of the sensation. Provided you found any particular animal, whether high up or low down, very intelligent, you would generally find that the amount of pain it would suffer from an operation would be in proportion to the intelligence. Where instinct obtains to a large extent, there we find, so far as our experiments have gone, an absence of suffering.

2599. I think I understand you to say that if the more intelligent animals were excluded from experiments, it would be also an exclusion of animals who suffer most?—Yes.

2600. (*Chairman.*) And also the animals whose nature approaching most nearly to man, would be the animals on which the experiments are made for illustrating the probable effects on man?—Yes.

2601. (*Mr. Forster.*) With reference to those cases which have come before you of young men who have tried experiments which you thought were unnecessary, as you say carelessly and thoughtlessly, do you or do you not think that if a law was passed making such experiments illegal, that would prevent their doing it?—It would prevent their doing it.

2602. With reference to the definition of the pain of an animal, Professor Huxley drew a distinction between men and animals in this respect, that the animals have much less anticipation of pain; but animals have some anticipation of pain, have they not?—Yes, a dog when once whipped, for instance, has.

2603. Or for instance, a hare hearing a dog bark, would be likely to expect the misery that would follow from being hunted?—There the old question comes up of instinct and reason, which is of course an exceedingly difficult question. I think if you take the definition which I gave, the amount of intelligence, the amount of the reasoning or thinking power, that may be taken very fairly as an indication of the amount of the susceptibility of that animal to pain. There may be other points on which it might depend, but it may be taken as holding good in a general form.

2604. (*Lord Winmarleigh.*) Would a monkey suffer pain more than a dog?—I think a chimpanzee, very high in its organisation, would suffer more than one of the commoner monkeys; and the higher the reasoning power of the creature you may take it that the more delicate it is, and the more susceptible it is of the suffering which we understand by pain.

The witness withdrew.

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Mr. J. Burdon-Sanderson, M.D., recalled, and further examined.

2605. (*Mr. Forster.*) I do not know whether you could give us a little more in detail your idea as to the amount of vivisectional experiments that are now carried on in England?—I should like very much to do so, if I can; and I think there are data for giving that information pretty correctly.

2606. May I ask you to draw a line, if you can do so, between vivisection properly speaking, that is, cutting up animals, and the experiments in giving them diseases?—I will. With respect to the amount of physiological investigation that is going on in England one may come to a very correct conclusion from various sources: First, from considering the number of communications which have been received by the Royal Society on physiological subjects. I have made a list of those communications, and I find altogether, in 20 years, 18 or 19 communications on physiological subjects, some of which would involve vivisectional experiments, many of them, however, not. There are five I think altogether, counting all that would require experiments on animals, and including two where the experiment would only amount to the taking of blood from the animal.

2607. I understand your answer to be that there were only three papers in the last 20 years published in the Philosophical Transactions of the Royal Society, which would involve painful experiments?—Yes. One may take another source of evidence which is equally good; there exists in connexion with the Royal Society, a Government Grant Committee, the grant being an annual one administered by the Royal Society for the advancement of scientific research. I have run through the grants which have been given to different persons on their application for the making of experiments of a physiological kind, and I have got a list of only ten persons who have received grants for physiological investigations at all; and of these, six only are of such a nature as to involve vivisection as it is usually called. There is another grant which exists at present, one dependent on contributions from medical men, administered by a committee of which I am also a member, and which has for its object the making of investigations with reference to the action of remedies. From these three sources I am able to form a very correct opinion as to the number of people who are actually engaged in this country in physiological investigation. About the time that Dr. Sharpey was giving evidence here, we went through the number together, and at that time we calculated that in England and Scotland there were about 13 persons now engaged more or less in physiological investigations in this country. I think I may make it up, by taking a great deal of pains, to 15 or 16. I have got the names here. I should prefer to give the names. (I have not got them in any order). Dr. Yule, Dr. McKendrick, Professor Dewar of Cambridge, Professor Rutherford of Edinburgh, Professor Arthur Gangee of Manchester, Dr. Caton of Liverpool, Dr. Michael Foster of Cambridge, Dr. Pavy of Guy's, Dr. Pye Smith, Dr. Brunton, Dr. Ferrier, Dr. Fraser, Dr. Crichton-Brown, Dr. Klein, and Mr. Garrod. I have put the names of four or five gentlemen who really are not in any sense engaged systematically in physiological investigation.

2608. (*Mr. Hutton.*) Dr. Wickham Legg you have not mentioned?—I ought to have put him in; I forgot him; but the number of investigations in which he has been engaged is small.

2609. (*Mr. Forster.*) This is the number of gentlemen that are engaged to your knowledge in physiological investigation?—Yes.

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2610. (*Lord Winmarleigh.*) As you are giving the whole list, had you not better put in your own name too ?—Yes.

2611. (*Mr. Forster.*) Those gentlemen may, or may not, according to the nature of their investigations, feel that they ought to resort to experiments ; it does not follow that they all will ?—Not that they all will, but that they all have. Every man who makes physiological investigations must more or less use animals ; because clearly physiology is a subject that deals with living animals, and not with dead ones.

2612. Do you think it at all probable that there are other physiological investigators through the country who would not have come into communication with either you or any of those gentlemen whom you have mentioned, so that you could know their names ?—I think it is very improbable.

2613. We have had it stated in evidence by one gentleman that he had reason to believe that a good many young men, young practitioners, are trying experiments simply for the object of finding out whether a thing is or is not as it is stated. Have you ever heard of that ?—I have directed my attention particularly to that question. I have had a great deal to do with students at different periods of my life, and have now a great deal to do with them, and I must say that I have never met with any tendency on the part of students to make experiments. And further I would say that students have told me themselves this, with reference to experiments on animals, that in those schools where a certain number of properly conducted experiments are made under the regulations which I explained at the last sitting, that very fact takes away from them that desire to make experiments themselves which they might otherwise have felt; because they see that making an experiment is a very serious matter ; that it involves a very great deal of exactitude and a great deal of preparation, and that in order to its being done effectually it must be done with the utmost possible care ; and that at once tends to discourage men from making idle investigations of their own.

2614. The evidence we have received is not at all applicable to the students of physiology, but the witness was speaking of young medical practitioners who were making experiments, such, for instance, as tying up the Fallopian tube of an animal in order to see what would happen ?—I do not think that is the case ; in fact I am certain that if it is the case, it must occur very exceptionally, and without any reference whatever to physiological investigation.

2615. (*Mr. Hutton.*) Are you not aware of one case which we are both aware of (I do not wish to mention the name) of a young man whom you would think very incompetent to make those experiments, and who admitted to us that he had a laboratory of his own in which he did operate on living animals ; of course you know to whom I refer, though I do not wish to mention the name, a man who had been studying under Dr. Klein ?—I should imagine that he had done very little in the way of experimentation himself. It was stated I believe, as far as I remember, that he had repeated a certain experiment with respect to which he had elsewhere expressed himself in very condemnatory language. I remember that circumstance ; but whether he made many experiments I do not know.

2616. He stated that he had a laboratory of his own in which he operated with the greatest care to prevent pain, but that he had a regular laboratory of his own ?—He certainly has published nothing ; no fruit has come out of his labours.

2617. I was simply going to ask you whether you do not think that that case is much commoner than you suppose it to be ?—I do not think so. I know one instance of a gentleman who has made a certain number of experiments on animals of an injudicious and useless kind. He is a man who entertains peculiar notions as to the action of certain remedies, and he wished very much to find out something about them. All I can say is that I had a long talk with him and did my best to persuade him to make no such experiments again, and I think that that advice was followed. I do not think that the thing has happened again.

2618. (*Mr. Forster.*) Then with regard to experiments for the purpose of teaching, I think I understood you to say that they were conducted with anæsthetics, with the one exception of the experiment of showing the action of poisons ?—The exception that I wanted to make was not that we physiologists, or rather those who are engaged in teaching physiology, would have to make such experiments, but that those gentlemen who are specially engaged in teaching the two subjects of the action of remedies, (I mean the physiological action of remedies, which is called pharmacology,) and the action of poisons, might find it necessary to exhibit the action of certain bodies, either remedial agents or poisons, without using anæsthetics, but I stated distinctly that so far as the illustration of a course of physiology goes, it is possible to make a sufficient number of experiments completely under anæsthetics, which are sufficient for the illustration of the great facts relating to the leading functions of the body.

2619. Then I think I gather from these two answers together, that any law which stopped teaching experiments, except with anæsthetics, and which allowed licences to be given to physiological investigators of competence to perform experiments, with certain restrictions, provided that those restrictions were reasonable, would not interfere with physiological science in England ?—Yes.

2620. You stated that the toxicological experiments would not give pain for any length of time. I am so ignorant about it that I should like to know what length of time ?—I must take an instance for that. You mean, as I understand you, toxicological experiments for demonstration. It is difficult to limit the time, but it is a time which would be expressed in minutes.

2621. In a strychnine experiment how long would the animal be under the agony of strychnine before its death ?—It would depend entirely upon the dose.

2622. But in order that the experiment should be clearly shown ?—Five minutes.

2623. You have the management of the Brown Institute under the direction of the London University ? Yes.

2624. Are the experiments which you practise done upon animals connected with that institute ?—Not at all ; not in the slightest degree.

2625. Are there any experiments in that institute upon animals ?—The only experiments that have been made there, as has been already explained fully elsewhere, are experiments relating to histology. Certain experiments were made there on inflammation which had to do with investigations in progress, and some of the results of these experiments were shown to certain gentlemen, with one or two exceptions, members of the medical profession, who were Dr. Klein's private pupils. It was understood that these experiments were made privately, that is to say, that the case was that of a number of skilled men working together at an investigation ; but there was some misunderstanding with relation to that question. Consequently it is not possible for me to say that there have been no experiments made there ; but no experiments of a physiological kind have been made in connexion with the business of the institution.

2626. What is the object of the institution then ?—The object of the institution is twofold. It is stated in Mr. Brown's will to be to investigate, and, if possible, to cure the diseases of animals. Up to the present time the institution has consisted of a hospital for the reception of out-patients and in-patients and of a laboratory of research ; and now we are endeavouring to extend it by establishing relations with another society with the view of conducting investigations as to diseases of the animals of the farm, and we hope shortly to be able to extend the usefulness of the institution in that direction. We

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have made extensive investigations at the Brown Institution, many of which have required the use of animals, particularly with reference to tuberculosis and to the question of the nature of fever. We have been lately investigating cancer, and also typhoid fever, but these do not come within the category.

2627. (*Chairman.*) But is cancer a disease of animals?—Cancer is very largely a disease of animals, though it cannot be induced in animals. We have it in many cases in animals.

2628. (*Mr Forster.*) Putting aside for a moment the effect upon human beings, do you or do you not consider that the physiological experiments which have been made upon living animals will tend to diminish the suffering of animals generally?—I can only answer that generally, namely, by saying that an increased knowledge of the nature of disease, and of the action of remedies, will most unquestionably diminish the suffering of animals; and further that the diffusion of knowledge among veterinary practitioners will enormously diminish the suffering of animals, and that this is likely to be the result of the direction of the attention of scientific men to their diseases.

2629. There have been experiments on sheep-pox, have there not?—There have.

2630. Do you know anything about them?—Yes; they have all been done in co-operation with myself.

2631. Do you think that these experiments bring any reason to suppose that we shall be able to diminish sheep-pox?—No; the object of investigating sheep-pox was entirely to acquire a knowledge about small-pox, because the one disease is a picture of the other.

2632. The knowledge that you gain of sheep-pox would not help then in the remedial treatment of that disease itself?—No; the case of sheep-pox is this, that you can get rid of it by inoculation. When sheep-pox prevails the animals are inoculated in large numbers and in that way rendered incapable of sheep-pox, and inoculation can be done in such a way as not to endanger the animal's life.

2633. Then you would not say that the race of dogs for example had gained by the sufferings to which any particular dog has been exposed in these experiments?—No; certainly not, not directly, excepting in the way that I have mentioned just now. Indirectly the increase of knowledge is beneficial to the dog as to man.

2634. (*Sir J. B. Karslake.*) In regard to that work which has been referred to, the handbook which you edited, might I ask what the date of the edition is?—1873.

2635. That is the only edition, is it?—The only edition.

2636. In that work, where you speak of the use of curari, was it assumed at the time that it was an anæsthetic?—No; I think not.

2637. That curari was not assumed to be an anæsthetic; but where it is mentioned it is mentioned as being a substance which would be used for other purposes than as an anæsthetic?—Yes; it is mentioned entirely with a view to its other actions, no doubt.

2638. You told us the other day that that book was written for the benefit of those who had advanced in their studies to a certain extent, and who were commencing physiological research?—Yes.

2639. Now does that book anywhere recommend the carrying out of the experiments which are referred to there as a part of education?—I am not aware that it does.

2640. Of course the book will speak for itself, but I merely want to have it one way or the other, whether the particular experiments which are mentioned in that work are recommended as being experiments which ought to be practised, or are merely statements of experiments that have been tried with particular results?—I think it must be admitted that although with reference to some of the experiments it is distinctly recommended that they should be shown, and with reference to others it is clear that they are not intended to be shown, it is not sufficiently distinctly stated which might be with propriety shown, and which not. In one part of the book the different classes of experiments with reference to that are distinguished by asterisks, but that is not the case as regards the rest of the book.

2641. The intention then is that a certain class of those experiments, notwithstanding that the results which it is desirable to obtain from the experiments have been clearly obtained, should be shown to students?—Yes, certainly.

2642. And in other cases it is not intended that those experiments should be shown to students; but the facts are mentioned in order to give general knowledge and acquaintance with the facts which are there disclosed?—Yes.

2643. Now in the cases where it is not intended that the experiments should be shown to students, but where facts and phenomena have been clearly demonstrated by experiments, in your judgment would the disclosure of those experiments rather tend to prevent or diminish vivisection in those cases than to increase it?—I think it would.

2644. In cases where you have described an experiment and the result is clearly shown, would it generally speaking be taken by physiologists that up to that point, unless there is something to disturb what you have stated there, the fact has been clearly ascertained?—Certainly.

2645. And in those cases you would rather assume that the series of experiments, or the particular experiment, by which that fact has been ascertained would not be resorted to again?—Certainly.

2646. In that sense the disclosure of the experiment would rather tend to diminish vivisection for the purpose of establishing the fact than to increase it?—I am quite certain that explanations of that kind have had that effect.

2647. And in writing books of that description for physiological students and others, is it necessary to describe the experiment and the result in order to give the full information as to whether the experiment leading to the result has been satisfactorily performed or not?—Certainly it is necessary. I would illustrate that by saying that in every lecture on physiology one always states the experiment as well as the result which has been founded upon it; and one always makes the conditions of the experiment as clear as one can to the students without reference to its being shown to them.

2648. Now in certain cases I understand you to say that the experiments have been performed, and it is stated that the experiments have been performed, and that satisfactory results have been ascertained, only up to a certain point?—Yes.

2649. In those cases do you think that the description of the experiment which has been performed and the result which has been clearly ascertained, give a basis and standpoint from which further experiments would be performed?—Yes, I think so.

2650. Without resorting to the old experiment for the purpose of ascertaining what is stated to have been clearly ascertained up to that point?—Certainly.

2651. How long have you been engaged in lecturing?—On physiology since 1866.

2652. How many courses of lectures in the year do you give?—One course.

2653. Consisting of how many lectures?—75 lectures.

2654. In how many of those 75 lectures would it be necessary to introduce experiments on a living animal?—None; I never make any such experiments during my lectures. I never make any experiments on animals in connexion with my systematic course. With reference to those special exceptions which have been mentioned, which relate as I say to the most important functions of the body, all of those experiments are included in perhaps two weeks or three weeks of the so-called practical course. In this case the experiment constitutes the whole of what is done on the occasion. I may as well explain, as plainly as I can, what my practical course consists in. I think

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I shall not be wasting time by doing so. My practical course extends over about 11 weeks. Of those 11 weeks about five weeks are occupied with chemical experimental demonstrations which do not involve any experiments on animals. About half of the remainder, that is to say, about two or two and a half weeks more, is occupied with the discussion of the physics of the nerve and muscle, which also does not involve experiments on animals but on the living tissues of dead animals. The other two weeks are usually devoted to the consideration of the functions of circulation and respiration; and within those we are able to show a certain number of experiments, all of which as I stated before can be performed on animals perfectly anæsthetised, or on animals in which the brain has been destroyed.

2655. Now might I ask you this question: in one of those courses of lectures, what is the number of animals, or about the number of animals, sacrificed?—I may state that we use for these purposes only rabbits and frogs. I do not wish to understate it; certainly it is not more than five or six rabbits.

2656. You use frogs as well you say?—We use frogs.

2657. Anything else besides rabbits and frogs?—No. I have used dogs also anæsthetised, but it is not necessary; one can do without using dogs for the particular experiments to which I refer.

2658. Of course different people may have different opinions on this subject; but in your judgment, as the person responsible for the education of those under your charge, could you, in your opinion, make that plain to them which you desire to communicate to them, without showing those experiments upon animals?—No, I could not. I could make it plain to them, but I should be under great disadvantage.

2659. Do you think your teaching would be as perfect unless you had the opportunity of showing the experiments as it is having the opportunity of showing the experiments on animals?—I think it would be much less perfect.

2660. (*Lord Winmarleigh.*) What is the usual length of your course?—A systematic lecture always lasts an hour, but, as I said, at systematic lectures no experiments are done. A demonstration must always last a longer time. May I ask, am I to limit myself in my answer to those demonstrations in which an experiment is required?

2661. You stated that your course lasted 11 weeks, and I meant to ask you especially the length of those lectures with demonstration?—Usually about half an hour.

2662. (*Mr. Huxley.*) To go back to this question of the handbook for beginners: there is a common impression that the meaning of that is, that any young man who feels inclined to study physiology, whether he has had any previous education or not in that direction, is to take up your book and go and try to do the experiments; was that your intention?—Certainly not.

2663. I apprehend in the first place that it is called a handbook "for the Physiological Laboratory," under which circumstances it is a tolerably clear conclusion that it is meant for the use of persons who are at work in a physiological laboratory?—Yes.

2664. To you and to me the sense of the words "physiological laboratory" is perfectly familiar, and we know that is a place under the direction of a skilled physiologist?—Yes.

2665. So that on the face of it, from the title of the book, it would convey to you and to me a clear impression that it was meant only for those beginners who were commencing that work in a physiological laboratory?—Yes.

2666. Then I ask you whether the book is not something very much more than a handbook for a beginner. For example, I myself am in the habit of using it in this way—that when I want to know what the experimental basis of any physiological doctrine is I turn to your book, and I find there an account of the exact matter of fact upon which the higher conclusions of physiology are based. I presume that use of it is a proper one in my hands?—I think it is.

2667. And one intended by the authors of the book?—Certainly.

2668. So that this book is not only a handbook for the laboratory, but it is meant as a kind of statement of the objective basis of physiological doctrine?—It is.

2669. I think you have already said that, so far as your knowledge of medical students goes, you do not find in them any disposition to do much in the way of experimentation themselves?—I have never observed any.

2670. Perhaps I might say that it is your feeling as a teacher that you would be a little better pleased with them if there were some zeal shown in that direction more than there is?—I should.

2671. So far from there being any need to restrain the average medical student in the direction of doing work, the practical difficulty known to every teacher is to get him to do the work that he ought to do?—Yes.

2672. Now there seems to be a prevalent idea that the kind of physiology dealt with in your book, and in such books as yours, is something new. Your memory does not go back, I suppose, quite so far as mine, but still you would be able to say if, 30 or 35 years ago, there was not a method in great use of exactly the same character; I refer, for example, to Marshall Hall's experiments and Sir Charles Bell's experiments in this country; they were pursued upon just the same method as those recommended in your book?—Exactly the same method.

2673. In fact there was no other way of ascertaining the facts which they ascertained except by experiment?—No.

2674. Now as regards the experiments in elucidation of pathology, my teacher, Mr. Wharton Jones, somewhere about the year 1845, that is 30 years ago, was to my knowledge (I must ask you whether it is to your knowledge also) engaged in experimental inquiries as to the primary phenomena of inflammation?—I know his researches very well indeed.

2675. So that there really is nothing new in these methods?—No.

2676. Nor anything that can be regarded as an innovation upon the established customs of physiologists?—There is no difference in the methods excepting that they are more complete than they were before.

2677. Then there seems to be a very prevalent idea that it is physiology alone which has of late years been developing this practical investigation in teaching. Now, I think you will confirm me by answering in the negative a question that I am about to put to you, namely, whether 40 years ago there was in this country a laboratory of practical chemistry in which students were trained in practical chemistry?—It is not necessary to go so far back. In Edinburgh, when I was a student, which is not so long ago as that, in 1850, there were no means whereby students could work practically in chemistry, in a laboratory in research, even in Edinburgh.

2678. As regards physics you are aware that recently the Duke of Devonshire has established a physical laboratory in Cambridge, and that within the last 10 years a very fine physical laboratory has been established in Oxford; but I think it will be within your knowledge that 20 years ago there was no physical laboratory in this country to which students had access, and in which they were regularly trained?—I think there was not.

2679. So that in point of fact the development of the teaching of physiology in the practical direction is nothing special to physiology itself, but is only part of the great movement which has affected all branches of physical science, and of the conviction at which men of science have gradually arrived, that there is no teaching of physical science worth anything as thorough teaching unless it is accompanied by practical instruction?—Quite so. I should like to add, Mr. J. Burdon-Sanderson, M.D.

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with reference to my opinion, that in the future physiological research will be carried on more in England than it has been carried hitherto, that the ground which I have for thinking so is that the study of physics and chemistry is now pursued under greater advantages than before, and that there are a greater number of young men practically conversant with those subjects on which physiology is based.

2680. Then is there not another consideration which tends very much to lead one to think that in course of time the number of persons who undertake private investigations will be much limited, and that the investigations will, as a matter of course, be confined more or less to physiological laboratories; I mean this consideration—that as research is carried on into the more difficult parts of physiology the investigator requires appliances of greater complexity, which are exceedingly expensive, such as time-registering instruments and the like; and that not only are those very commonly out of the reach of the private investigator as a matter of expense, but that even if he could afford to buy them he would have to build a place adapted for their use?—I am certain that that will operate in that direction.

2681. So that the natural tendency of physiological research is to get itself, so to speak, into laboratories provided with these means and appliances? — Precisely so.

2682. Then again, I think you expressed the opinion just now that the more clearly students are instructed in the experimental method the more they learn of what requires to be done to carry out experiments successfully, and the less likely it is that they will attempt them themselves unless they are properly instructed?—I think so.

2683. That is to say, any man of decent intelligence will understand that it is a perfectly useless thing for him to attempt to waste his time over the repetition of these experiments unless he has not only the manual dexterity which is required, but unless he has the apparatus also?—Yes.

2684. For instance, there is a sort of notion afloat that a student who gets your book into his hands immediately sets to work to repeat all the experiments he finds in it. Now I presume that, taking a rough estimate, some half or three quarters of those experiments could only be performed if a man had access to considerable instrumental aid? —Certainly; that is certainly not over-stating the matter. There are very few of the investigations which can be conducted without exceptional instrumental aid.

2685. So that in fact what it comes to is this—that so far from sound instruction in the experimental method tending to make students reckless, or even to increase their desire to try experiments, the distinct tendency of that instruction in your opinion will be to render them less likely to try these experiments, more likely not to try them, except in physiological laboratories where they have the means and appliances, and where they are under superintendence?—Certainly.

2686. I understood you, in reply to questions which were put on the last occasion, to give a sort of general approval to the Bill which was proposed by Dr. Playfair, in the sense that you did not actively object to it?—Yes.

2687. That, however, if I may judge by the feelings that have been expressed to me by other scientific men, does not imply that the Bill met with your active approbation, or that you entertained any conviction that legislation was necessary?—I think that legislation is not necessary with relation to organised physiological research, and I do not know any reason for thinking that it is necessary on any other ground.

2688. I understand you to have said that in your belief there is no wanton or cruel experimentation practised in this country?—None to my belief.

2689. And so far therefore as the ordinary grounds for making new laws are concerned it would not appear that there is any case for legislation at all?—No, I think that it is so.

2690. Then taking the particular kind of legislation which is proposed, and looking at it from the general point of view of the way in which laws ought to be made, there are one or two questions which I should like to put to you. Some years ago it was thought expedient to pass an Act for the restriction of the sale of poisons; no doubt you recollect the fact?—Yes.

2691. I do not know much of the details of the Act, and I have nothing to say about the Act itself. Suppose, however, that the framers of that Act had so drawn it that all the pains and penalties of the Act fell upon the medical men who do occasionally use poisons for the benefit of mankind, and at the same time left everybody else free, so that if a medical man used poisons he should be liable to penalty or restriction of some kind, whereas all the rest of the world might buy poison to any extent and use it as they liked; do you think that an Act of that kind would have commended itself to the common sense of the legislature?—Certainly not.

2692. Do you not think that the medical men who were affected by such an Act might have very justly said that it was injurious to them, and threw a most unjust slur upon them?—Yes.

2693. Now to apply that to the legislation which has been proposed in this case, what I apprehend to be proposed is this, that the infliction of pain for certain purposes upon animals, such infliction being held by the persons who inflict it to be for the good of mankind, shall be either altogether stopped or placed under certain restrictions, while the infliction of pain by all the rest of the world for totally different motives, in many cases for the purest amusement, shall not be touched at all. You will understand that I refer to the fact that such Acts as these restrain men of science from experimentation while they leave untouched all that vast amount of infliction of pain upon animals which goes under the name of sport. Now I would ask you whether within your knowledge the principle of such legislation as that is not regarded by men of science as unjust in itself and as casting a slur upon them as a body?—I certainly think that it is. I think that it is felt that the same principle ought to guide legislation in respect of all other occupations, whether connected with sport or not, which produce pain, as it is proposed to apply to the case of experimentation upon animals, without taking into consideration the circumstance that we are working for an important and good object.

2694. Now in case it should be said that there is no parity between the sort of pain inflicted in the two cases, I would beg you to put before the Commission your opinion as a physiologist as to some examples which I will put before you. I understand you to say that while you yourself as a physiologist hold that it is perfectly lawful and justifiable to inflict pain upon animals where you have any reasonable prospect of obtaining thereby a considerable advance of knowledge, yet that you are, on moral grounds, as much opposed as anybody can be to the infliction of pain, unless you have some such definite object; that is so, is it not?—Quite so.

2695. Then I would ask you whether, if a physiologist proposed this experiment, you would think it one which was justifiable for him to carry out. Let us suppose that he took a hare, that he contrived some sort of place in which the hare could run round, a covered gallery we will say, that he set off a machine to frighten the hare very much, which should run as fast as the hare at first, but to a certainty in course of time catch the hare, and then after the animal had gone through all the agonies of fear or exhaustion, should proceed to crush the life out of it. If that experiment were proposed by a physiologist, I take it that you would consider that there must be some demonstrably important truth to be brought out before that would be justifiable?—It would be most unjustifiable, unless the result was of extraordinary importance.

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2696. So that if any physiologist did that for the mere sake of getting amusement out of it, you and all physiologists, I suppose, would look upon it with great reprobation ?—Yes.

2697. And yet the hypothetical case which I have put to you is practically a description of the ordinary practice of coursing, is it not ?—I think it is. I have been in the habit of coursing myself to a great extent in former years, and I think it is a true description.

2698. I will put to you another case. The Secretary of the Society for the Prevention of Cruelty to Animals was before us the other day, and he told us with great horror, of certain experiments which have been performed in relation to asphyxia. I am not sure whether 30 or 40 animals might not have been sacrificed ; however, the person who performed these experiments had a definite scientific object in view. Are you acquainted with what usually happens when rabbits are snared as they are by countless thousands in this country ?—Yes ; I know it very well.

2699. And you can tell us as a physiologist what happens ; will you be so kind as to do so ?—In snaring rabbits it may happen that a rabbit is quickly killed, but in a great many cases it does not happen, and consequently if you go into a rabbit warren at night you always find (as I have had the opportunity of ascertaining very lately) that many of the rabbits are not killed by the snare, and they consequently remain in agonies for a long time ; but as you mention the subject of catching rabbits, there is no doubt that the torture which is inflicted by the snare is not nearly so great as the torture which is inflicted by the trap, which is used even more largely. Not long ago I was in Scotland, in Ross-shire, and there happened to be near the house where I was staying a rabbit warren, and I had the opportunity of inquiring into this matter. On that warren some 3,000 rabbits are killed in the year, and I learned from various persons that as you passed the warren you could hear during the night, the cries of the animals.

2700. I have only one other question to ask you on this topic, and that is this ; I presume that physiologists might consider it desirable to experiment on the effect of gunshot wounds, and to inflict gunshot wounds for the purpose of experiment ; but still I presume that in that case also you would consider that any man who undertook an investigation of that kind should have exceedingly clear and definite perceptions of the advantage to be derived from such an inquiry before he entered upon it ?—I understand you to mean the effects of gunshot wounds on living animals.

2701. Yes ?—Such a mode of investigation would be of course unjustifiable, excepting as I stated before, with reference to some result of extraordinary value.

2702. I presume with your large experience of physiological laboratories, you are able to tell us whether any experiment you ever saw in a physiological laboratory is likely to have inflicted the same amount or a larger amount of pain, I would rather say, than that which is inflicted by an ordinary gunshot wound ?—If the animal survives, you mean ?

2703. Yes ; supposing it to be hit in a not vital part ?—It is difficult to estimate that. It so happens that in connexion with my known interest in the diseases of animals, it has twice lately occurred to me that gentlemen have sent me hares which they had supposed to have been suffering from epizootic disease, in which I found shot corns lodged in the serous cavities, which had produced serious inflammation which had lasted of course over a long time. It is quite clear that in such a case as that, the injury so inflicted, of course perfectly unnecessarily, had given rise to a great deal of pain on the part of the animal. No doubt in every hundred rabbits, for example, that are shot, a very considerable percentage are wounded and escape to their holes to suffer. But it is not possible to estimate the amount of suffering so inflicted. It must be very great as compared with the amount of any suffering that we ever inflict. If you take that one means of inflicting suffering, the number of rabbits made to suffer in that way clearly must be enormously greater than the number that suffer in physiological experiments, and the amount of suffering inflicted must be also of a very serious kind.

2704. (*Mr. Erichsen.*) The other day you gave us an illustration of the importance of physiological investigations or experimental inquiry ; you illustrated it by pathological research as an application of experimental inquiry to the elucidation of various pathological phenomena. Is it your opinion, or have you any view upon the subject, that for the future we can only hope for an advance in real knowledge of disease through means of that kind or of an analogous kind ?—It is my profound conviction that a future will come, it may be a somewhat distant future, in which the treatment of disease will be really guided by science. Just as completely as mechanical science has come to be the guide of the mechanical arts, do I believe, and I feel confident, that physiological science will eventually come to be the guide of medicine and surgery.

2705. In relation to hygiene, to therapeutics, and to all other methods employed for the treatment and cure of disease ?—Yes, in relation to three things, the prevention of disease, the introduction of new remedies, and the investigation of their action.

2706. And that if we are to obtain anything like precision in our knowledge of remedies it will only be through the medium of physiological investigation ?—I feel confident that it will be so.

2707. That the disturbing agencies in a diseased condition of the human frame are such that it becomes quite impossible to determine the true action of any remedy when applied to such a frame ; we see certain results, but we do not know how those results are produced ?—Exactly so.

2708. And with regard to the true nature of disease, that knowledge which is derived from an investigation of the dead body, do you think that there is very much more to be acquired by ordinary dead-house observation, the rude pathology in fact that was practised and that is practised more or less in ordinary post mortem examinations ?—During a period which may be said to be now drawing towards a close there can be no doubt that the nature of disease was elucidated to an extraordinary degree by post mortem investigations ; but as you have said the amount of knowledge which we can gain by that mode of investigation is getting exhausted, as we find, by the repetition and repetition of the same facts. Now we have come to a period at which what we want to know is how processes occur, and what are their relations to each other.

2709. That there is, in fact, something like a finality in that knowledge, in the knowledge acquired by the ordinary dead-house investigation ?—Yes.

2710. And that future progress is to be made through other channels ?—Yes.

2711. Those channels may be microscopical investigations and physiological experiments ?—Yes.

2712. And possibly other methods ?—Yes ; exact observations of different kinds.

2713. And observations based on experiment, I presume, also ?—Yes, no doubt.

2714. Then there was another point on which you could give the Commission some little information ; it was in reference to the students who frequent your different classes. I take it that the students who frequent or who attend the class of ordinary systematic physiology at University College are those medical students who simply wish to qualify themselves to become practitioners ?—Exactly.

2715. They constitute at all events the great bulk of the pupils attending your classes of systematic physiology, and they also constitute the great bulk of the students attending the demonstration class, do they not ?—Yes.

2716. But does any considerable proportion of those students attend or work in the physiological laboratory ?—None ; because it never happens that a man becomes what I call a worker in the physiological

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laboratory until he is very much more advanced than that.

2717. Is it not the case that many workers in the physiological laboratory have passed altogether beyond the state of pupilage, that they are qualified practitioners of medicine, that they have passed their examinations?—Entirely so, it is rarely otherwise.

2718. And that officers in the Army and Navy in Her Majesty's service frequently come there in order to improve their knowledge of the higher branches of their profession?—It has several times happened so.

2719. And that the men who work there are in fact, as a rule, medical practitioners of different kinds, and who wish to obtain a more scientific knowledge of their profession than they have heretofore acquired, and not mere medical students in the ordinary sense? —It is so.

2720. And those men probably find that such physiological knowledge so acquired is of essential service to them in after-life, in the practice of their profession, or they would probably not expend both the time and money that are necessary for the pursuit of that inquiry in the laboratory?—With reference to a certain class of workers that is strictly true. There is another class of workers to whom that does not apply.

2721. And what is the other class?—The other class of workers, a very small class I must say, consists of men who intend to devote either part or the whole of their life to scientific work.

2722. (*Lord Winmarleigh.*) Of no profession you mean?—Of no profession.

2723. (*Mr. Erichsen.*) But yet you find that there is a considerable number, or a certain number at all events, of young medical men who, after they have completed their studies, and passed their examinations, will devote much time and go to a considerable expense in order to acquire a knowledge of the higher branches of physiology?—That is so as regards our workers.

2724. And those men intend in after-life to become practitioners in medicine, and not only scientific men? —Yes.

2725. Consequently we may take it, I presume, that they imagine, whether rightly or wrongly, that the superior knowledge of physiology that they will acquire in a laboratory will be of considerable advantage to them in practice in after-life?—Quite so. I may add to that, that it is my object, particularly in my own laboratory, to connect the studies in the laboratory with those at the bedside in the hospital which is connected with the institution.

2726. In fact that you make them more or less run in the same line with the studies in what are called pathological matters?—Yes.

2727. Is it the case that in all large well conducted medical schools now there are pathological as well as physiological laboratories, so that the men may work in both?—Yes.

2728. (*Mr. Hutton.*) You have admitted that the object of the whole of the school which you represent is to increase very much the class of teaching that is given in physiological laboratories; that you very much lament the fewness of those laboratories in England; is not that so, that your idea is to have in this country something more like what now exists in Germany and France?—I should wish to see a greater number of efficient schools of physiology.

2729. And you would like to see the ordinary practitioners of medicine passing through those schools so far as might be possible?—Before I answer that second question may I fully answer the other? I should not like to see, I should be very sorry to see, physiological laboratories, established in connexion with all the small schools in this country; it would be objectionable.

2730. You would like to see a very much larger number of the ordinary practitioners of medicine passing through the physiological laboratories than now do so, would you not?—No, I should not wish to see that; I do not think it is at all desirable that all students of medicine should have gone through scientific study of that sort. I think that the man to whom it is of advantage is the consulting physician or the consulting surgeon; but I think that it should be confined as much as possible to men who are likely to be advisers, if I may so express myself, in the profession. With reference to medical students in general intended for general practice, the great point is that they should be what we call thorough clinicists, that is to say, thoroughly acquainted with the aspects of disease as seen at the bedside, and that they should be thoroughly up in the technical part of their profession.

2731. But as regards the higher consulting physicians and surgeons, you would wish to see a larger number of them passing through these laboratories, and you would not think them competent to advise on the more difficult cases without it?—No, I would not go so far as that; because as medicine stands at present, it stands on experience, and the man who is most competent to advise is the man of most judgment and most clinical knowledge, in other words, of most experience.

2732. Still my point is, that you wish to see the number of these laboratories increasing, and not decreasing; that you wish to see the education here more like the type of education in Germany than it is?—I do; but I think I must again repeat what I said as to the type of education in Germany. It is not the case at all, that the ordinary medical practitioner there is more educated in this way than our medical practitioners are; but simply that there is a class of men in Germany who work hard at science. I do not want to introduce any German institutions because they are German, but simply because they are efficient.

2733. And you wish to see a higher class of scientific men in the profession also, I understand?—Yes; certainly I do.

2734. And those scientific men cannot be made what you wish them, as I understand you, without passing through the physiological laboratory, at least, not in the best way?—No; I think not.

2735. Now can you give us any idea at all of what I may call the consuming power of a great laboratory like Professor Ludwig's at Leipzig?—I am afraid I cannot.

2736. Can you give me any notion of how many living animals in the year are taken there for treatment?—I really cannot tell you; I really cannot form any idea. I could tell you how many animals are required for a particular research.

2737. In your own most active year how many living animals would have been experimented upon in your own laboratory for all purposes?—I think I had better answer that by considering the number of animals that I have used myself in particular researches. The research which used the largest number of animals of all the researches in which I have ever been engaged, was one to which I have referred before, namely, the one on tuberculosis, and that has extended over a number of years, and I have no doubt that some 200 or 300 rabbits may have been used in the course of that time.

2738. But that was not the only research that you were carrying on at the same time?—The number used in the other researches must have been extremely small. The only one which would form an exception to that perhaps, of any consequence, would be one which I presented to the Royal Society a few years ago, and in which I suppose I may have used 50 animals; I do not know.

2739. You cannot give us any idea, for instance, how many University College Laboratory would use in the course of an active year for all purposes, and of what classes the animals would be?—If I had known beforehand, I could have given you exact information from the book but you will find that the number used for experiments of the so called vivisectional kind is extremely small.

2740. Will you send in a statement of that?—I shall be very glad to do so. It could not be strictly accurate, but it will be accurate within limits.

2741. (*Mr. Forster.*) Giving the number of animals experimented on last year, for instance, and also the sort of experiments; because some of them may not have been as painful as others?—Yes.

2742. (*Mr. Hutton.*) You will divide them into those in which there was no more painful experiment than killing them, and those in which the experiments were of a painful kind?—Yes.

2743. You cannot give us any idea how many Claude Bernard's laboratory would consume in a year, or Ludwig's?—I cannot tell you really.

2744. You cannot give us a fair guess?—I do not think I could give you a guess at all.

2745. (*Lord Winmarleigh.*) Was last year what Mr. Hutton calls a very active year, or more than usually active?—I should think not particularly so, I do not know that it was more one year than the other.

2746. (*Mr. Hutton.*) Now did you verify all the experiments in your handbook yourself, or only those in your own department of it?—Only those in my own department certainly; and there are many cases in which it is stated distinctly they are not verified.

2747. (*Mr. Forster.*) But there are several of the experiments in your own chapter which you have not verified, are there not?—Yes; there are several where that is so stated.

2748. (*Mr. Hutton.*) Taking the whole number in the handbook, could you give us an idea of how many of the whole were painful experiments, and how many not so; how many were either completely under anæsthesia, or otherwise not painful?—I cannot give you any idea; I think I could, probably, in my own part of it, because it relates you see to the great functions, and as to these you have, of course, experiments of more importance. There are many cases in the other part in which one would not be able to say whether it was a painful experiment or not.

2749. Of course you were more particularly responsible for your own part, and you could do that for us for your own part?—Yes, I would do that; I would distinguish between those which could be performed without detriment to the result with anæsthetics, and so on.

2750. Now I understand your position to be that all experiments for a sufficient scientific purpose that are performed by an adequate inquirer without unnecessary pain, are right?—I am glad you have given me an opportunity of answering. My principle about that and all other cases, whatever the purpose may be in which pain is inflicted is simply this, that the question of right and wrong depends upon the relation between the purpose of the experiment and the pain inflicted, and the care which is taken that the experiment shall be done in the most efficient manner. If the purpose is a good purpose, and if the experiment is made in the most skilful way in which it can be made, and if due care is taken that no unnecessary suffering shall be inflicted, I think that the whole thing is a right action; I have no hesitation about that.

2751. I think that you were present when Sir Robert Christison delivered his address in Edinburgh a few weeks or months ago on the subject of vivisection?—You mean in proposing a vote of thanks? I remember the circumstance.

2752. In that address he was showing how very far from hard hearted physiologists are, and he said that there was one experiment which he had entered upon for a sufficient purpose, and from which he had desisted in the middle on account of the agony caused to the animal. You cannot perhaps tell us what that was?—I do not remember what it was: it was an experiment upon some irritant poison I think.

2753. But would you call that unnecessary pain or not? would you say that was an amiable weakness on Sir Robert Christison's part, supposing that the purpose was sufficient, or that it was a conscientious act on his part?—I should prefer not answering that question, with reference to the particular case because I do not remember.

2754. I was only taking it as raising the question with respect to any experiment where you find one inflicting extreme pain?—I think that a man after devising a method which he believes to be the best method that can be used for the purpose, and having considered the pain that is likely to be inflicted, should not desist in the middle because that pain is inflicted; I think it would be foolish to do so.

2755. (*Mr. Forster.*) But would you not consider that if it was an experiment of that exceedingly painful kind, there ought to be the greatest possible reason for its repetition if it is repeated?—I think I have said so before, that whenever much pain is to be inflicted the purpose to be accomplished requires to be correspondingly important.

2756. (*Mr. Hutton.*) But is it not very much a matter of opinion among medical men, and varying very much from one to another, what is necessary and what is unnecessary in these matters? For instance, Professor Rutherford has stated that he thinks various demonstrational experiments necessary which others have told us they do not think necessary at all. Is not that a thing in which the public would have to rely on the opinion of one particular man?—I think that applies to all kinds of conduct; but with respect to the particular instance put before me, I believe there is a general agreement among physiological teachers in this country that for demonstrational purposes, it is not desirable to use experiments which are attended with pain.

2757. There has been a great difference of opinion between yourself and Dr. Swaine Taylor on the subject of poisons. He assured us that it was not necessary even for the sake of showing the effect of strychnine to exhibit it on living animals, and that he had desisted from the practice; that he had drawings so perfect that it was entirely unnecessary; but there I understand you and Dr. Rutherford to differ from him?—I confess I entirely differ from Dr. Swaine Taylor on that point.

2758. My point is this to bring out that not only you differ from each other, but the foreign physiologists would differ very much from you, would they not, on that subject?—I include among those whom I have represented as agreeing in opinion, those who are actually engaged in research. I only refer to men in that position. I was speaking particularly of teachers of physiology just now.

2759. But you see what we have to fear is that as the experimental school increases in this country the opinions that are now held abroad may come with it; and I suppose that is a reasonable fear, is it not?—I do not think that it is a reasonable fear.

2760. I will put this as a test question: Would you think that any physiologist who gave that experiment on recurrent sensibility simply to show his class in a more vivid way the action of the nerves, was going beyond the bounds of reasonable humanity?—I am perfectly certain that no physiologist, none of the leading men in Germany for example, would exhibit an experiment of that kind, because it is an excessively difficult experiment, and quite unsuitable for the purpose of demonstration.

2761. As regards experiments for purposes of demonstration there is likely to be a very general and uniform agreement on the subject in England, as I understand you?—I think so.

2762. Differing only on smaller points?—I think so.

2763. But as regards research you would consider that any pain, supposing it was sufficiently justified, might be inflicted?—Yes, with the proviso that I made just now, that we ought to have constantly in view the object of making it as small as possible.

2764. Now has it ever occurred to you to see any experiments on inducing rabies in dogs?—No, I have

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not; I have seen dogs with rabies which has been communicated to them.

2765. But have you ever seen any attempt at artificially inducing rabies in dogs?—No, I have not.

2766. There was a common story, which very likely may have been false, in the papers not long ago, that some physiologist tried to induce it by keeping a dog starved and without water in the presence of food and water, and I believed that failed; but, however, would such an experiment as that, if conducted by competent physiologists, seem to you at all allowable?—It is so extremely unreasonable to make an experiment of that sort from what we know about rabies, that I cannot conceive of anybody competent doing so.

2767. You mean that it could not be induced by any mental action?—No, it could not be induced in that way. I mean that in that case the purpose could be so clearly shown to be a foolish purpose, that it would be an unjustifiable experiment.

2768. It would not be on the ground of torture, but on the ground of the want of scientific knowledge which it showed that you would condemn it?—I think so, because if that pain could be inflicted with a certainty that the human race would be benefitted by it, then a justification would exist.

2769. Or with a certainty that knowledge would be increased by it, would you say?—Yes.

2770. Are there not a vast number of experiments, which are actually given accounts of in the German and French journals, quite as painful as that, or more so, which in your opinion would be justified?—I do not know any such.

2771. Such an experiment as dividing the vagus nerves and others of that kind?—As I explained on Saturday, the division of the vagus nerves simply produces inflammation of the lungs and certain other symptoms not more painful than those of disease.

2772. This is an extract, as I believe, from the second volume of the Archives de Physiologie; a dog was placed under curari, which, I suppose, as regards a dog at least, is not an ænæsthetic; is not that admitted?—I have stated that I myself have no decided opinion on that subject.

2773. The pneumo-gastric and sciatic nerves were dissected out and irritated during six hours. The operators than went away at night leaving the artificial respiration engine working on the dog, which they found dead the next morning, the machine still working upon it. The object of the experiment which was tried by M. Paul Bert was said to be to test how long the nerves would remain sensitive. Of course, I have no special physiological knowledge. I only want to know whether there are not a vast number of experiments of that kind that might be tried with real scientific advantage?—Well, I think, with regard to such an experiment as that, the rule that I have already stated, namely, the relation between the purpose and the good gained must again be the rule of action. What I mean to say is this, from that statement it would seem to me, that the rule had not been conformed with, namely, that it was not done in the most efficient manner, and with a due regard to the least possible infliction of suffering, and therefore that in so far it was unjustifiable.

2774. Now as to those experiments on starvation which Professor Sharpey gave us an account of, and which he seemed to think were justifiable, to try the absorption of the different fluids at the various stages, what would you say?—It is perfectly true that those experiments of Chossat have given most valuable results, which you find quoted in every handbook of physiology. They give us the statistics of a particular process. It is very difficult to say precisely how the rule would apply to such a case as that; but there is no doubt that it would be very improper to repeat experiments of that kind.

2775. Except for the purpose of further verifying. You would not exclude a further verification of a result which was doubted in any degree?—Certainly not, according to what I have already said.

2776. (*Mr. Forster.*) But now with reference to that answer take this particular experiment, would you think that a physiologist would be justified with the present knowledge about that experiment in repeating it?—No.

2777. (*Chairman.*) And if an experiment is a very painful one the disposition to arrive at a conclusion that its repetition was necessary would be very much enfeebled?—Yes; no doubt.

2778. (*Mr. Hutton.*) Take the experiment which Delaroche and Berger have made, "They have demonstrated that in a dry stove the energy of the " duration of resistance is in direct proportion to the " size of the animal. In those experiments which " have been continued until the death of the animal " the heat of the stove has varied between 50° and " 93·75° (centigrade scale)." In fact animals were baked to death to see at what temperature they would die. Those are, I suppose, purely scientific?—Those experiments might, if they were conducted with skill, be, on the one hand, productive of important results, and on the other not be attended with much pain; because an animal, when subjected to a high temperature, very soon comes to a point at which pain ceases. I cannot comment upon the particular experiment, because I do not know it.

2779. The experiment of freezing animals to death you would say the same of, I suppose?—The same thing applies undoubtedly.

2780. (*Mr. Forster.*) Do you consider that either you yourself, or any other of the English physiologists with whom you are acquainted, would in the case of a very painful experiment, such as one or two of these of which we have heard, think yourselves justified in repeating it after it had been established, as we hear these have been established, by the foreign physiologists?—No, not at all.

2781. (*Mr. Hutton.*) But that of course assumes that it was so established. Now is it not the case that scientific investigation constantly reopens questions and finds that they were not established in a way that was satisfactory to the new investigator?—I should like to make a general statement with reference to that. If you take the progress of physiology, I mean during the past 25 years, there is no doubt that it has consisted, in a great measure, in revising statements of facts which had previously been ascertained by less exact observations.

2782. Revising them by the re-making of experiments?—By making experiments under more exact conditions than they were made before that time. I mean that since 1850, or thereabouts, there has been a change, and that that change has consisted in the introduction into physiology of more exact methods derived from physics and chemistry; and that consequently there has been a certain amount of repetition, revision, and re-building up of facts which were previously known in rough outline; and that circumstance explains to us how it is that physiology has made such very rapid progress during that time. But more and more every year our methods are becoming exact, and the more exact our methods are, it is unquestionably true that there will be less and less repetition of experiments made before; and particularly that in Germany, where exactness is really more valued than it is in France (I have no hesitation in saying so), and in this country also, so far as we do a little in that direction, repetition will be avoided by the great care and exactitude with which experiments are made.

2783. Still all the old experiments suggest new ones, do they not, and new ones that are not less likely to be painful than the old ones?—There is no doubt that questions will arise until the science has become an exact science, so to speak.

2784. What would your criticism be on that statement of Mr. Ray Lankester's, that in this, as in every other experimental science, the number of questions raised at every point increases in geometrical progression, so that the numbers of victims to this process must increase in geometrical progression too?—I must

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say that I think that that statement was a mistaken one.

2785. A scientifically mistaken one?—A scientifically mistaken one. I may illustrate that in this way. If you take such a book, for example, as the one just published, Hermann's Physiology, and compare that book with a book written 25 years ago, we will say Müller's, you will find in the first place that the book is a great deal smaller; and secondly, that all the statements are a great deal more simple; so that the existing position of physiology can now be expressed in much shorter language than it could then. That implies a diminution in the number of facts, and particularly in the number of unconnected facts. I mean to say that our knowledge is becoming methodised, and consequently that the number of separate facts is smaller now than before.

2786. You would not say, would you, that Ludwig's laboratory tries fewer experiments than any laboratory existing at that place would have tried twenty years ago?—No, because really investigation has been going on with more rapidity during the last ten years in physiology than at any previous period.

2787. And it is likely to go on with still more rapidity?—Investigation may no doubt.

2788. Painful investigation?—I do not see why it should.

2789. (*Mr. Forster.*) You would not apply that answer to giving animals diseases, would you, I mean you would not make the answer that there would be less likely to be experiments for the future, as regards pathological experiments of giving animals diseases?—I think the same answers apply on the whole.

2790. (*Mr. Hutton.*) Are you not on the council of the Medical College of Women, or some such council?—Yes, nominally, at all events.

2791. What is the proper phrase?—I think it is "for the medical education of women."

2792. Now one thing which gave a great impression that vivisection was increasing very much was, that a vivisector was chosen, and a French vivisector, who could not express himself in any very good English, for teaching the young women, when two, an Englishman, I think, and a Scotchman, had offered themselves, who would not have used vivisectional practices. Will you tell me anything you can on that subject. Is it not within your knowledge that an appointment was made there, and, as was said at the time, by your advice, of a gentleman who could not speak English clearly, and whose great distinction had been obtained in the region of vivisection?—I took no active part in the affair at all.

2793. The statement as it was made to me was, that two, one an Englishman and one a Scotchman, offered themselves for the post, that this Frenchman, whose great distinction was in this region of experiments, also offered, and that it was referred to you to decide between them, and that you chose the Frenchman?—All that I did was to express my favourable opinion of Dr. Dupuy as a physiologist, but I did not express any opinion of the others.

2794. Then subsequently Dr. Dupuy resigned, did he not, because the young women would not attend at vivisectional experiments?—I am sorry to say I cannot tell you anything about that, I only attended the initial meetings in that school, in order to give it a push at the beginning; and I am sorry to say I have not interested myself about it since, although my name is on the council.

2795. In effect you know very little about the matter?—I know nothing about the matter since that time.

2796. Just one or two questions about the Brown Institute. You were stating just now that these vivisectional experiments do not take place at all at the Brown Institute; the experiments on living animals, except so far as the induction of artificial disease goes?—My words were, I think, that experiments of that kind are not conducted at the Brown Institution as part of the business of the institution at all, but that a certain number of experiments have been conducted in connexion with Dr. Klein's private teaching. I am speaking of the present state of things, not of the past.

2797. But experiments have taken place in the Brown Institute that were of this kind, have they not, some of Professor Ferrier's?—Yes, as you know, experiments of that kind were done, and after they were completed it was explained to Professor Ferrier that it was not possible for such experiments to be made there; and that has not occurred since, as you also know.

2798. Still this institution was founded expressly for the benefit of animals, was it not, and there is nothing in the whole of Mr. Brown's will that suggests anything that is not for the benefit of animals; is not that so?—Perfectly so; but as you refer to Dr. Ferrier's experiments, I must be excused for remarking that those experiments were made in a room not used for any other purpose, and not used for any of the purposes of the institution; and that it was entirely a private arrangement, just as much a private arrangement as if they had been made in one's own house.

2799. Still, however, as to Dr. Klein's experiments you are aware, are you not, that a good many go on in his private room which are really of a painful class, though not under your direct superintendence?—I have stated what I can about it. I know that he has used animals in the course of his private instruction in histology, and also with reference to certain investigations as to inflammation.

2800. (*Mr. Forster.*) Is the laboratory in which he tries those experiments a room in the Brown Institute?—A room in the Brown Institute.

2801. (*Mr. Hutton.*) Can you tell us how many patients there are in the Brown Institute. I mean the number of animals there for purposes of cure?—The numbers are printed every year; I cannot tell you what it is at present.

2802. Is it true or not that about as many animals are kept there for the purposes of these experiments as there are for the purposes of cure?—There are no animals kept for the purpose of experiments excepting guinea-pigs and rabbits.

2803. And rats?—And rats also; but the rats are kept entirely for anatomical purposes; they are very little used there. The only reason why they are kept there is that they can be kept there more healthy; and the guinea-pigs the same. There happens to be space there for keeping guinea-pigs which is very convenient and suitable.

2804. About the relative number, do you suppose that there are more or fewer animals kept there for the purpose of experiments, than there are for the purposes of cure?—If you count the rats and guinea-pigs and rabbits, of course there are more.

2805. A great many more?—Yes, because patients never present themselves belonging to those species.

2806. The practical result is, that an institution founded expressly for the cure of animals contains more animals subjected to painful experiments than it does animals which are being cured?—No; on the contrary, because these animals that you speak of are kept entirely privately. They do not belong to the institution, and are not kept at the expense of the institution.

2807. They are operated on in the institution?—No, not at all. These animals happen to belong to me; they are animals which I occasionally use at University College. I mean, it sometimes happens that I want rabbits or guinea-pigs, and they are sent for to the Brown Institution. These animals are there in store, so to speak.

2808. You store them at the Brown Institution for University College?—I mean to say that the purpose for which these animals are kept there is an entirely private purpose. They belong to me, and are kept for my objects, whether right or wrong. They have nothing to do with the institution, and are not kept at the expense of the institution, or in any of the stables or places intended for the reception of animals.

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2809. (*Mr. Forster.*) How far is the institution of any sort of service in keeping them?—Not of any service, excepting in so far as it affords space. There is not a single shilling of the funds of the institution spent in keeping these animals.

2810. That turns out to be of the greatest possible service, does it not, because you would not know where to put them otherwise?—I could find other places, but it is better for them to be there.

2811. (*Mr. Hutton.*) But it is not only for your own purposes at University College that you kept these animals there; but for Dr. Klein, for the purpose of his private investigations at the Brown Institution, is it not. He takes his supply from the same stock?—Yes.

2812. Practically then, the animals kept there for the purpose of painful investigation are more numerous than the animals kept for the purpose of cure?—If you wish to put that question again, I must give the same answer as before, that if you count these small animals that you have mentioned, it would necessarily be so; (to the Chairman) I am sure Mr. Hutton will do me the justice to admit that I have done the very utmost that I could to give the Brown Institution a practical character for the beneficial investigation of the diseases which affect animals which are most important to men.

2813. (*Mr. Forster.*) The largest number are rats, are they not?—Rats and guinea-pigs.

2814. Are they kept for the purpose of painful experiment, or for the purpose of keeping them until they are killed, with a view to anatomical investigation afterwards?—They are kept for any purpose for which they are wanted; as regards guinea-pigs, mostly, almost entirely, for anatomical purposes.

2815. The question which Mr. Hutton asked you was, whether a larger number of animals are kept for painful experiment than for cure?—I have answered that question.

2816. You think that there are?—Yes. These animals are kept there as my animals, and if I have been wrong in keeping them there of course the governing body of the institution would interfere. But whether I am right or wrong my keeping them there has nothing whatever to do with the purposes of the institution.

2817. (*Mr. Hutton.*) Except that Dr. Klein, who investigates on the subject, derives his supply of animals from the same stock?—Surely that comes to the same thing. If Dr. Klein is engaged in doing work which is not part of the business of the institution, it makes no difference whether he uses them or I use them.

2818. (*Mr. Huxley.*) The fact is that keeping those animals there does not exclude a single animal from being treated?—Not the least.

2819. And, if there were any difficulty in keeping them there, there would not be the least difficulty in your keeping them somewhere else?—No.

2820. (*Chairman.*) But an officer of the institution is engaged in performing painful experiments on animals in the institution; I am speaking now of Dr. Klein's experiments?—Yes.

2821. (*Mr. Hutton.*) Will you tell me how you obtain the supply of animals for University College laboratory. We were told that at St. Bartholomew's Hospital, special directions were given as to the mode in which they were to be obtained, and were to be treated. Have you anything of the kind at University College?—We depend very much upon our servant, who is a very reliable and respectable man, and who always acts in a straightforward way in the purchase of animals. I have no precise knowledge as to the methods which are used.

2822. You do not know from where they are drawn?—I know as regards rabbits and guinea-pigs and so forth, that they are drawn from the usual sources; that they are bought in the market.

2823. I particularly refer to dogs and cats?—I cannot tell you where they come from. There is no proper provision in this country by which one can obtain dogs even for the most legitimate purposes, and of course I am not informed as to the way in which they are obtained. They are always paid for at a proper price.

2824. You do not know whether they come from the Home for Lost Dogs for instance?—I do not believe so.

2825. (*Chairman.*) I will just ask you one or two questions. You think, as I gather, that the proper organisation of physiological inquiry will diminish the number of experiments, upon the whole?—In proportion to the results to be obtained, certainly.

2826. So that if that proper organisation were to be regulated by any legislative measure, it would have the effect of excluding outsiders probably from the practice of unnecessary experiments?—I think it would, and as I said before, I would not entirely exclude outsiders.

2827. But still the tendency of it would be not to interfere with men of the greatest eminence in science, so much as to prevent unnecessary and undesirable experiments on the part of those whom I have called outsiders?—I agree to that, with the limitations which have been before made.

The witness withdrew.

Adjourned to to-morrow at twelve o'clock.

Tuesday, 26th October 1875.

PRESENT:

THE RIGHT HON. VISCOUNT CARDWELL, IN THE CHAIR.

The Right Hon. Lord WINMARLEIGH.
The Right Hon. W. E. FORSTER, M.P.
Sir J. B. KARSLAKE, M.P.
THOMAS HENRY HUXLEY, Esq.
JOHN ERIC ERICHSEN, Esq.
RICHARD HOLT HUTTON, Esq.

N. BAKER, Esq., Secretary.

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WILLIAM RUTHERFORD, M.D., called in and examined.

2828. (*Chairman.*) You are Professor of the Institutes of Medicine or Physiology in the University of Edinburgh?—Yes.

2829. You have also been a teacher of physiology in King's College, London?—Yes, I have been a teacher of physiology five years in the University of Edinburgh and five years in King's College, London.

2830. The University of Edinburgh is one of the largest medical schools in the country?—Yes, it is decidedly the largest in the country.

2831. Is that science there taught by means of a course of systematic lectures?—Yes; we teach it in two ways, first of all by a course of systematic lectures, and secondly by a course of practical instruction. The practical course consists chiefly of a microscopical study of the dead tissues of the body, and very

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largely too of physiological chemistry, *e.g.*, the chemistry of digestion, chemistry of the blood, and so on, and also it consists to a much smaller extent of the study of physiological apparatus, with a few experiments on living animals.

2832. Are those experiments on living animals conducted under anæsthesia?—Both in the systematic lectures and in the practical courses experiments on the higher animals, for example dogs and rabbits, are entirely conducted to the best of my knowledge devoid of pain. In the case of frogs that is generally the case but not always.

2833. (*Lord Winmarleigh.*) Devoid of pain in what manner?—Produced in the case of dogs and rabbits by opium, or chloral, or chloroform.

2834. Not simply by curari?—Not at all. The three things given for the purpose of preventing pain are opium, chloral, and chloroform. In the case of frogs, they are almost always rendered insensible.

2835. (*Mr. Hutton.*) By what?—Usually by a blow on the head.

2836. Pithed?—Yes; first of all the animal receives a blow upon the head producing instantaneous insensibility, and then the head is removed and the brain pithed. Most of the experiments on frogs are made on the nerves or muscles of the leg or the heart, after their removal from the body of an animal rendered insensible in the manner which I have just described.

3837. (*Chairman.*) In your systematic lectures are there any experiments on living animals which are not performed under complete anæsthesia?—A few in the case of frogs only. These I should say are to illustrate the action of certain poisons, for examples, strychnia and curari.

2838. What animals are used in the class of practical physiology?—A few rabbits, six at the outside, chiefly frogs, no dogs, and no cats.

2839. The rabbits being made altogether insensible to pain?—To the best of my knowledge altogether insensible to pain.

2840. How are experiments for the purpose of original research conducted?—Those are conducted either by myself, or by my assistant, or by some person acting under our immediate superintendence. Sometimes they are conducted painlessly; in other cases the animals are not insensible to pain.

2841. What is the rule by which you guide yourself in determining whether they shall be rendered insensible to pain or not?—When the mode of rendering them insensible to pain would interfere with the due result being obtained from the experiment we do not so render them.

2842. Is that any large proportion of the experiments?—I should say a considerable proportion.

2843. (*Mr. Forster.*) Would it be more than half the experiments?—I should have a difficulty in saying how many, but I should think about half the experiments that I have done.

2844. (*Mr. Hutton.*) It differs in different years I suppose?—It differs at different times.

2845. (*Chairman.*) And according to the object that you have in view?—According to the subject that you are proposing to investigate.

2846. These of which you are now speaking are only for the purpose of original research?—They are only for the purpose of original research.

2847. Now when the object in view has already been attained by former experiments at home or abroad, do you consider it justifiable to have recourse to a repetition of those experiments?—Yes, if I think it necessary in order to be persuaded of the truth of this or that fact, I consider it to be necessary to repeat the experiment that was previously performed.

2848. Has there been any great increase of the practice of vivisection in this country?—Yes, I think there has been a considerable increase of late years, owing to physiology now being taught in a more experimental manner, and also owing to the increase in the amount of physiological research which is now done, more of us devoting our entire time to the study of physiology quite detached from medical practice; on that account I think there is a considerable increase in physiological experimentation.

2849. Now you are aware I think that a great deal of feeling has been expressed in opposition to the continuation or increase of this practice?—Yes; I confess I believe that that feeling is largely founded upon misrepresentation of what is done in this country, based upon statements made with regard to what is said to be done in other countries.

2850. Do you think that there is a difference between this country and other countries in regard to the tone of feeling on this subject?—I think so.

2851. That the tone of feeling in this country is much higher than the tone of feeling in others?—Yes; I think that the English race are really upon the whole more anxious not to give pain to others than the inhabitants of some portions of the Continent. I have, I am bound to say, noticed that there is a difference in the amount of delicacy with which the human subject is treated in some hospitals abroad; and that has struck many of those who go to study medicine abroad.

2852. The higher tone of which you are now speaking as prevailing in this country is the tone of the medical practitioners in this country, and also of the students?—I think so.

2853. And that higher tone, I need scarcely ask you, you think it very desirable that we should encourage and perpetuate;—Yes; certainly.

2854. And if there were supposed to be any tendency to an approach in this country to the tone which you think has prevailed in some other places, you would regard that as a circumstance very much to be regretted?—Yes; but of course always provided that there was sufficient reason for the supposition as to its being an actual fact that there is a departure from just humanity in England. If it be a fact, then no doubt I think it requires serious consideration.

2855. Do you know whether the medical students in this country are in the habit of performing experiments in private, in their own residences, or otherwise than under the control and direction of their teachers?—I never heard of such a thing being done, neither when I was a student, nor have I heard of anything of recent years, and I am persuaded that if it were done I should be sure to have known of it.

2856. Now for the discovery of new truths, I understand you to think that experiments upon living animals of a painful nature are justifiable?—I believe so, justifiable for the discovery of new facts, and for the repetition of it may be important facts which have been recently stated. I do not know that it would be necessary for me to offer evidence with regard to the effects of discoveries in physiology upon the practice of medicine, or the advantages that have resulted from them. Perhaps indeed you may have heard abundant evidence upon that point.

2857. But you desire it to be taken as in your judgment a matter of necessity that such experiments shall occasionally take place?—Thoroughly so.

2858. Now for the education of medical students do you think it necessary?—Not except in really very exceptional circumstances. I do not think it at all necessary that any higher animal, such as a dog or a rabbit, should be subjected to pain for the education of medical students.

2859. It would be quite possible then, without any injury to science, altogether to prohibit experiments otherwise than under anæsthesia for the mere purpose of medical instruction?—In the case of higher animals; but in the case of frogs I do think that such a prohibition would somewhat interfere with tuition. For example, it is a very important thing to show the action of strychnia in producing tetanus. That is done upon a frog not anæsthetised; the animal is very soon killed by the effect of the poison; I do not think it would be judicious to have a law prohibiting our showing that to the medical student, who has to study cases of tetanus which are so rarely seen.

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2860. (*Lord Winmarleigh.*) How long would the animal live under strychnia ?—Two or three minutes.

2861. Not more than that ?—No, not generally ; it depends upon the dose given ; with the dose commonly employed for showing the experiment the animal would be allowed to live three or four, or at the outside I should think five minutes.

2862. (*Chairman.*) Now do these last remarks extend to the education of physiologists ?—Yes, I think, generally ; I should say they extend to every one until he should desire to do an original research.

2863. The number of those persons whose pursuit of original research would render it necessary that they should perform painful experiments without anæsthesia, I suppose, is small ?—A very small number indeed.

2864. And the advantage to science of any large number of persons being so employed would not be considerable ?—I think not, unless they are thoroughly competent.

2865. It is, therefore, possible for the purpose of our inquiry to regard the number of persons who should perform painful experiments without anæsthesia as a very limited number ?—Certainly.

2866. And if it were known to you that any other persons besides this limited number of well qualified persons were performing such operations you would think that undesirable on the ground of humanity, and not to be commended on the ground of science ?—I do not think it should be.

2867. Where did you study physiology yourself ?—I studied it in Berlin chiefly ; I had to go there to have a whole course of experiments performed for my special benefit, not having been taught physiology experimentally in this country ; and the consequence was that there was a great expenditure of time and teaching power, and also of animals to teach me alone ; and I have always since thought that if these experiments had been shown to me when I was a student in Edinburgh the same number of animals which would have served for my teaching would have served for it might have been a hundred students.

2868. I understand you to argue upon these considerations that an organized system of public teaching would be conducive to the interests of science, and also to the interests of humanity ; is that your view ?—I think so, decidedly. I think we ought not to do away with the demonstration of certain known facts before students under certain conditions ; that physiologists ought to be permitted in their lectures to show certain experiments on animals for teaching purposes.

2869. The greater number of your experiments are upon dead tissues, I suppose ?—Yes.

2870. And a great part upon chemistry and subjects that have nothing to do with sensation ?—Yes. For example, the experiments on digestion, and those upon the changes of the blood in respiration, do not require experiments on living animals ; also those on the movement of fluids in tubes, and various experiments in vision, the accommodation of the eyes to near and distant objects, and so on, indeed, the majority, do not imply vivisection.

2871. Are you desirous of offering to the Commission any opinion as to the relative sensitiveness of animals ?—I believe that the animals that one commonly employs for experiment are not so sensitive as men ; and I infer that from this sort of reasoning : human beings differ one from another very much in their amount of sensitiveness. One man may stand a severe operation almost without wincing, while another is caused to faint or is thrown into great excitement thereby. And you may notice the same thing in the case of the lower animals ; for example, dogs ; greyhounds and spaniels are exceedingly sensitive, and are thrown into a great excitement by an operation which, if done on a sheep-dog or a mongrel dog, would scarcely produce any excitement at all ; it is wonderful what one may do to a sheep-dog without the animal making any commotion. Then there is a great difference between different species—between cats and rabbits, for instance ; a cat is a far more sensitive creature than a rabbit ; I infer that because it is thrown by similar operations into much greater commotion than the rabbit is. On that account I never perform an experiment on a cat, or a spaniel, or greyhound if I can possibly help it. I consider these the last animals I would resort to ; I always take rabbits and common dogs. Then, frogs appear to me far less sensitive than the higher animals.

2872. (*Mr. Hutton.*) May I ask you whether you think guinea pigs are sensitive ?—It would be rather difficult to say. I sometimes have thought they are not very sensitive, because sometimes they seem almost to disregard a cutting operation upon them, but on the whole I think they are tolerably sensitive animals.

2873. (*Chairman.*) I observe that in a recent publication of yours you mention Lord Henniker's Bill as having been "drawn up by the anti-vivisection party ?"—I was under that impression then.

2874. And you say that "had this Bill passed it would "have made no essential change in the conduct of the "physiological department of this University ;" is that your opinion ?—I was of that opinion when I wrote that; but on re-considering that Bill it appears to me that it might be almost a dangerous thing, at any rate an inconvenient thing I should say, for the power of granting a license to be vested in the Secretary of State. It appears to me to be difficult for that gentleman to judge whether or not a license should be granted ; that he would have to consult some person who would be likely to know, and I do not know whom he would consult. I fancy that it would be better, if such a measure be thought necessary when you have considered the real facts of the case, to vest the power of granting the license in such a body as the Councils of the Royal Societies, the Councils of the Colleges of Surgeons of London, Edinburgh, and Dublin, and the Councils of the Colleges of Physicians of London, Edinburgh, and Dublin ; and the Inspector of Anatomy might act under the jurisdiction of these councils in seeing what was carried on by the experimenters ; and these councils might in turn be responsible to the Secretary of State. It would, I believe, be extremely objectionable to make the inspector directly responsible to the State, otherwise he would virtually have the power of granting or removing licenses in his own hands, and I don't think he ought to be entrusted with such power. He ought to be simply an inspector for the licensing bodies I have mentioned.

2875. And if those arrangements suggested by you were carried into effect, you see no reason to change the opinion that the Bill would not have interfered with the practice of the University of Edinburgh ?—I do not think it would have done so had the licensing and the inspection been conducted as I have indicated.

2876. It would have interfered, if at all, then with the persons we were speaking of as a class whom it is not desirable for the interests of science to have performing these experiments ?—Certainly it would have prevented that.

2877. With regard to the other Bill which was introduced into the House of Commons by Dr. Playfair, have you considered that ?—It seems to me that that Bill was extremely objectionable.

2878. In what respect ?—Objectionable because we could not for teaching purposes, for the demonstration of known facts to our classes, have performed an experiment upon a living animal, even one that had been anæsthetised, without being liable to be fined 50*l.* and imprisoned for three months.

2879. (*Lord Winmarleigh.*) Is that so ?—Yes ; the wording of the Bill is extremely complex, but when you consider its outs and ins it comes to be so.

2880. (*Chairman.*) Supposing the Bill to bear the interpretation that you have now put upon it, it would have interfered with what you consider necessary for the instruction of your students at Edinburgh ?—It would certainly have done so, and have driven students from this country to study physiology on the Continent.

2881. But if the exhibition of such experiments to

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students under complete anæsthesia had been permitted by the Bill that objection would have been entirely removed, would it not?—Not altogether, though nearly so; because there are a few experiments where a little latitude is required to be left to the teacher—a few experiments on the action of strychnia, and various things on frogs which cannot well be performed under anæsthesia, because the conditions would have been changed.

2882. But with that small amount of latitude the Bill would not have interfered with the practice of the University of Edinburgh?—No, as far as I can see at present. The wording of that Bill in many points was so complex and indefinite that I do think it would have been a very dangerous Bill. I should much have preferred Lord Henniker's Bill, if there had been legislation on the subject. I confess that if legislation were thought advisable I believe that the Government ought to license certain men to carry on vivisection, but ought to leave the details of the manner in which the vivisection is to be carried on to the persons licensed. Of course they would always remember that if they are not discreet in what they do the license will be taken from them. I think it would be very difficult to define exactly what experiments should be done under anæsthesia, and what should not.

2883. But I think you said a little while ago that you would not object to a competent inspector, and I think you instanced the present inspector of anatomy, being acquainted with everything that passes at Edinburgh?—Certainly I would not object to that, provided that his reports are to be submitted to the licensing bodies I have mentioned, and not to the Secretary of State, who could scarcely be supposed to understand all the details of this or of that case.

2884. And you would not object to a full and complete record of all that passed being kept? indeed, I suppose it is kept already?—Yes, in the published experiments.

2885. I mean you have, in your archives at Edinburgh, a record of all experiments that are performed?—Most of them; not all.

2886. You would not object to have a record of all?—I do not think that there would be any hardship in that.

2887. Then the operation of such a measure as we have now under immediate consideration could not be to interfere with the proceedings of a competent well organised school, like that of the University of Edinburgh?—I do not think it would.

2888. But it would have the effect of interfering with persons of a totally different description, if any such there be, who employ themselves in these pursuits?—Certainly.

2889. Now there has been a great deal said in some quarters about the proposed extension of the University of Edinburgh, which is now, I believe, the subject of a contract and being carried into execution?—It is so.

2890. Is it the fact that that new building is intended to be the theatre of a great development of this practice of vivisection beyond that which it has already attained in the University of Edinburgh?—Certainly not. The statement has been made by persons who are entirely ignorant of what is contemplated. We do not propose at all to increase the amount of vivisection that goes on; we simply want greater space for the carrying on of various developments of physiology—for teaching different departments; we simply want greater space because the school is so large and the students so numerous.

2891. Then if you are to carry on vivisection in a painful manner at all, I understand from you that the larger your scope and the more convenient your means the less the number of experiments that will have to be carried on; that repetitions will be rendered unnecessary, and the sufferings of the animals, on the whole, much diminished?—Yes. I should simply say that if we are permitted to perform experiments on living animals before classes for teaching purposes, the end of it would be that a smaller number of animals would be used for the repetition of known experiments, because we would do one experiment, say upon the nerves of the heart, before 200 men; they would all see it and no doubt understand the mode of experiment; whereas if 200 persons wanted to do original research upon the nerves of the heart and had not previously seen the fundamental experiment, an animal would require to be sacrificed for every one of the 200. Therefore there is a great economy of vivisection if we are permitted to teach students by vivisecting animals occasionally.

2892. The particular point I want to direct your attention to is the new building in Edinburgh; and I understand you to say that the construction of that new building will diminish the number of experiments for the reason which you have just given?—It will not diminish the number of experiments, the construction of the new building will permit of our studying physiology more easily, but about the same number of experiments will go on. At this present time we are obliged in one room, not very large, to work with the microscope, to teach chemistry, and to teach experimental physiology, that is, explaining the use of apparatus and doing experiments; but in the new building we are to have a separate room for each of these things. The size of our room, which has been called a room for experiments on animals, has been commented on as being 30 feet by 25 feet. The architect said that it was for "experiments on animals," but that means really all experiments that do not imply observations with the microscope and chemical experiments—for example, experiments on electricity, animal heat, and the motion of fluids through tubes, and so on. I think almost the smallest part of the experimental work carried on in that room, which has been called a room for the experiments on animals, would be on animals living.

2893. Then you entirely negative the impression which appears to have prevailed that the construction of this new room is to be taken as part of a determination of yours to increase and extend the practice of experiments upon living animals?—I absolutely negative it.

2894. And I understand you to say that if you have the facility of showing an experiment to a larger number of students with convenience you do in fact diminish the total number of such experiments?—Certainly.

2895. And thereby diminish the sufferings that are inflicted upon animals?—Certainly.

2896. May I take it from you then that you are of opinion that there is a different and a much more desirable tone of feeling in this country than there is in some foreign countries?—I think it is highly desirable that we should avoid the giving of unnecessary pain, but at the same time that we should have a due regard to the great importance not only of a physiological research, but also of a right method of educating our medical students.

2897. That in organized schools there are securities against abuse which do not exist in the case of private experiments?—Certainly in the case of private experiments by incompetent persons.

2898. That the tone of the medical teachers and of the students would be entirely opposed to the infliction of any unnecessary suffering?—I am sure of that. I remember that it has happened to me that a dog that I may have been showing an experiment upon has come out from its anæsthesia, and I have felt on one or two occasions that to permit it to do so is the most dangerous thing; the students at once resent it, and indicate that they desire nothing of the sort. That occurred to me some years ago, long before this furor got up, and I have since then always felt that I dare not show an experiment upon a dog or a rabbit before students when the animal was not anæsthetised.

2899. (*Lord Winmarleigh.*) Do you say that as regards London?—I say that of students in London particularly, and also in Edinburgh. I remember that my teacher, Dr. Bennett, when I was an assistant in Edinburgh, (I did not, perhaps, think so much about it

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as he did, he being an older and more experienced gentleman,) would not permit of any experiment on one of the higher animals not perfectly anæsthetised, he was so afraid of the students, in addition to the fact of having his own feelings in the matter.

2900. (*Mr. Hutton.*) That applies only to the higher animals I suppose?—Yes, to dogs and other higher animals; indeed, he was so alarmed that he never had an experiment upon a dog.

2901. (*Lord Winmarleigh.*) I see that you are Professor of the Institutes of Medicine. Are all of these lectures that you give strictly connected with medicine only, or are any of them purely physiological?—I should say that the Institutes of Medicine is a term that must be taken almost simply to mean physiology. I am Professor of Physiology really; I lecture only to medical students. No doubt the medical applications of physiology are largely entered into, but it is also treated as a pure science.

2902. I see in the British Medical Journal of October the 23rd, 1875, an experiment made by you upon a dog, and the whole account of it is given there?—Yes, on the liver—on the biliary secretion of the dog.

2903. Was there any great object connected with the human frame which you had in view?—I think a very great object. Much more definite knowledge has been gained from those experiments with reference to the action of medicinal agents upon the liver than we had before.

2904. Do you think that that object would not have been obtained except through the use of a living animal?—I think certainly not; notwithstanding the great number of years that rhubarb has been given to persons there has always been a dispute whether rhubarb increases the secretion of bile or not, because rhubarb gives a colour to the stools or fœces of men somewhat like that given by an increased secretion of bile, and therefore many persons have found it impossible to say whether it increases the secretion of bile or not. It has been suspected, but not proved. Now if you put a tube in the bile duct of a dog, and measure the amount of bile secreted before and after giving rhubarb, you find that certainly the rhubarb always increases the secretion of bile; therefore we now know definitely, what was not known before, that rhubarb is a powerful stimulant of the liver.

2905. And that is a great object in medical science, is it?—Undoubtedly. And then also with reference to colchicum, which is given so much in gout; it has been suspected that colchicum was a stimulant to the liver, but it was never definitely proved until these experiments were done. We now know that colchicum is a powerful stimulant of the liver, and the information thus obtained will doubtless add greatly to the definiteness of our knowledge regarding this matter.

2906. That also you think would not have been obtained except by experiments on the living animal?—Well, such definite knowledge has not hitherto been obtained, although this medicine is daily given to the human subject.

2907. Now I observe that in this experiment, made on the occasion to which I have just referred, you used curari?—Yes.

2908. Could not that have been performed under anæsthetics?—It could not have been performed, so far as I know, under any other agent but curari, the object being to keep the animal perfectly still, because whenever an animal moves the muscles of its abdomen it squeezes out bile, and the consequence is that the flow is rendered irregular; therefore it is absolutely necessary that any movement that takes place should be a regular movement; in consequence of that; curari is given to prevent motion of the limbs—to prevent motion of the muscles of the belly; and the only compression of the liver which takes place is produced by the lungs being inflated by means of a pair of bellows at regular intervals.

2909. The fact then of curari having been applied to that dog did not then tend to diminish the pain of the experiment?—That is a point about which there is a dispute, whether curari is to be regarded as diminishing pain, or not. I believe the evidence is in favour of its producing, to some extent at all events, a state of insensibility, although I am perfectly aware that Bernard made years ago the statement that it did not. There have been experiments performed since that time which I think go far to demonstrate that it is to some extent an anæsthetic.

2910. Could you state to the Commission any circumstances which fortify your belief that curari does to a certain extent limit the pain?—I only know this, that there is an experiment on the frog introduced by Bernard, an experiment which I have shown many a time to my students, and recent experiments have been performed somewhat similarly by Mr. Yule, of Oxford, which quite corroborated experiments that I have often done and the impression that I was deducing from them, that it is to be regarded not as a substance that acts merely, as Bernard says, upon the peripheral ends of the motor nerves, but also upon the brain and the spinal cord.

2911. It has been stated by some gentleman who has given evidence here that the effect of curari was different upon different classes of animals—that it had a different effect upon the upper class of animals from that which it had upon the lower class; is that your opinion?—I am not aware of any fact which demonstrates that.

2912. Can those anæsthetics which you have spoken of, any of them, be used in such an experiment?—In some experiments opium, and chloral, and chloroform can be used in the case of the higher animals.

2913. But I understand you to say that they would have retarded your investigation in the case which you have given?—I could not have given them. It has been proved by Röhrig that the action of such a narcotic as opium diminishes the action of the liver; therefore if I had given them, and given a substance like colchicum, I might have got results upon which no definite conclusion could have been founded. It is sometimes impossible to arrive at correct therapeutic knowledge if two substances be given.

2914. You have made a comparison between medical practice in foreign countries and in England. Are you of opinion that equally beneficial results have been obtained by the more humane practice in England as in foreign countries by what you consider more cruel practices?—I was speaking of medical practice. With regard to the practices of physiologists I am bound to say that its inhumane practice has never been seen by me. I have studied in Berlin and also more recently in Leipsic, and I thought that the practice of vivisection there was exceedingly humane. I am entirely unacquainted personally with what is stated to have been done elsewhere.

2915. From your knowledge of those practices generally should you say that equal benefit has been derived by English practitioners with foreign professors?—To that I can only reply that if vivisection for teaching purposes be conducted in the humane manner which I have indicated, all the benefit that the young medical practitioners of this country require can be obtained.

2916. I understood you to say, in answer to a question put by Lord Cardwell, that you do not believe that those professing physiology with whom you are acquainted would object to some regulations being established which would secure to the public a conviction that no unnecessary cruelty was exercised in the experiments in vivisection?—I do not think that they would object to it, provided that whatever regulation is adopted the teaching of physiology be really not interfered with, and provided that the prosecution of research on the subject by competent persons be not interfered with.

2917. (*Mr. Forster.*) Take this experiment of yours of which we find a report in the last British Medical Journal. Can you tell us to what extent each of the animals would be likely to have suffered pain?—Well, I could not answer that question further than to say this—the animal I feel pursuaded would not have suffered so much pain as the human being would have

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suffered under the same circumstances; I should go the length of saying, I think not nearly so much.

2918. How long was the operation ?—Usually half an hour; the after observations, which, I think, would imply scarcely any pain, lasted about eight hours in some cases; the animal was killed always at the end of the eight hours.

2919. What I understand is, that first the animal was kept fasting for 18 hours ?—Yes.

2920. I suppose that the fasting would not cause any great suffering to a dog, would it ?—I think scarcely. The animal was fed at three in the afternoon, and next morning at nine the experiment was performed, that is what is meant by the 18 hours fast.

2921. Then the actual operation would last about half an hour ?—About half an hour.

2922. In what state was the dog at the end of that half hour ?— Simply paralyzed by curari, having artificial respiration by means of a pair of bellows kept up, having a tube in its common bile duct with the bile dropping from it.

2923. And in that state it would go on for eight hours ?—Not exactly, not simply in that state; the wound in the abdominal wall being once, or twice, or sometimes three or four times, I dare say, opened, and a substance injected into the bowels, the wound was then closed again, and the animal wrapped up in cotton wadding to prevent a fall of the temperature.

2924. The animal was kept under the influence of curari the whole time ?—The whole time.

2925. Then supposing that curari does not deaden pain, would not there be very great pain during that eight hours ?—I do not think so, certainly not very great pain; I question if anything more than trivial pain.

2926. Does not the artificial respiration cause pain ?—No, I think not; it is impossible to say unless the animal can speak; but the conditions are such that you would not reasonably suppose it could cause pain, wind being blown into its chest, and distending it as in ordinary respiration.

2927. Each one of these cuttings which I see mentioned would cause pain of course ?—That is to be inferred.

2928. Did any of the dogs die during the eight hours ?—I think in one case; at this moment I can scarcely remember. A dog got very weak owing to its having got too much curari; curari paralyzes the heart if it be given in too great a dose.

2929. You consider that the curari killed the dog ?—Decidedly.

2930. Did any of the dogs cease to be under the influence of curari before they were killed ?—Not that I remember.

2931. Now with regard to the half hour of the actual operation; supposing that curari did not deaden pain, I suppose that would be a half hour of very great pain ?—It would be a half hour of pain, I should not say very great.

2932. You say that chloroform or opium could not have been administered without marring the experiment; could not ether have been administered ?—I do not think any of those could have been administered. I must tell you that I performed about 30 experiments on the same subject as that, but treated differently, some six years ago, and in every one of the operations the animal got chloroform; every dog had chloroform thoroughly given then, but not in this case, because I felt that one did not dare give the chloroform because of the risk of having the results possibly interfered with. I advisedly on this occasion in this research never gave chloroform, although I did it in every case in the previous research.

2933. The administration of curari stops any cries from the dog ?—Certainly.

2934. Then the effect is that, although it may not diminish the distress of the animal, it will diminish that distress which any operator, any spectator, must feel when an animal shows that sign of agony ?—Yes; but then I think that the evidence is distinctly in favour of its diminishing the distress felt by the animal.

2935. My reason for asking that question is, that although I feel sure, from what you have stated to be your own feeling, that this would not operate with you, I think you will admit that there might be danger that before an audience curari might be administered for painful experiments in order to prevent the students expressing their disapprobation, which you said they would do if the dog uttered cries ?—Yes; but I must state that when I do an experiment before students, for teaching purposes, I never give curari as the substance which is to prevent the animals feeling pain, because I think there is a doubt about it. I always give either opium, or chloral, or chloroform, substances about which there is no doubt, because it is not necessary to give curari alone in that case, and certainly the animal should have the benefit of the doubt.

2935a. I am not speaking of this last experiment, but there is nothing, I suppose, to prevent both chloroform and curari being given in some cases, is there ?—It is conceivable; I do not know anything to prevent it.

2936. What I mean is this—that you might give curari for the sake of the experiment in order to make the dog perfectly motionless, and that you might insure its insensibility by giving chloroform ?—You might do so. It would depend whether you would think it advisable to do so or not—upon the result that you might desire to obtain.

2937. With regard to this curari, about which we have had different evidence, because, I suppose, the facts are not positively ascertained, has there been no experience gained by the knowledge of what has happened with any human beings that have been poisoned, or slightly poisoned, by it as to whether they have suffered pain during that time or not ?—I should not like to make a statement on that matter, the reason being that I do not feel at this moment a sufficient acquaintance with all the details of the observations on human beings.

2938. (*Sir J. B. Karslake.*) Has it come to your knowledge that human beings have been poisoned by curari ?—Yes; I have read of one or two cases amongst Indians.

2939. (*Mr. Hutton.*) Claude Bernard gives experiments on human beings; you have not read the account of them, perhaps ?—I do not remember to have read anything but Bernard's account of what was seen by others in the case of Indians. But I am aware that Bernard was led by his experiments on rabbits and frogs to say that curari primarily affects the motor nerves, and he always expresses the belief that sensation is not affected. That is Bernard's statement, but I have told you that Mr. Yule's experiments warrant a different statement.

2940. (*Mr. Forster.*) You stated that you thought it was necessary to some extent to repeat experiments ?—I think it is essential in teaching.

2941. You say that it is essential in teaching ?—Essential in teaching, and essential, I should also add, in order to ascertain whether or not there is truth in an experiment which has been published.

2942. I understood you to state that, for the purposes of teaching, you would not, at any rate with dogs or with the higher animals, use painful experiments ?—No.

2943. Then, consequently, such an experiment as this recent experiment would not be used simply for the purposes of teaching ?—No.

2944. Now take it for the purposes of research; there would be a time, of course, at which you would consider that repetition was no longer necessary ?—Undoubtedly.

2945. Now can you give us any kind of impression, with regard to this particular experiment, how soon you would consider it proved so as not to require repetition ?—That must vary with the nature of the results derived from any one experiment. Take the case of the first experiment on podophylline there

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given. After doing, I think, six experiments, I became convinced that it was unnecessary to repeat it any more. I do not think I should have done as many as six if some of the experiments had not yielded different results.

2946. As regards your own research, you would not, as I understand, think it necessary to try more experiments than you have tried. Now would you apply the same to other investigators, or would you consider that it was their duty also to try these experiments?—Well, I should, with regard to my experiments, think that it was not necessary for them to do so. At the same time they might adopt quite another idea, and regard it as necessary to repeat them, possibly under different conditions, and possibly with a view to saying whether this is really so, because the results are so important.

2947. I am asking this question because it seems to me that if there be any legislation to control experiments that legislation must provide that some person or persons should give an opinion as to when a repetition was, or was not, to be considered permissible?—I think you would involve the whole subject in endless difficulty unless you left it to the discretion of the person licensed that he should do this or that.

2948. But then is not that really not only leaving it to the discretion of the person, and therefore to his knowledge, but making the matter depend upon his sensibility with regard to these matters, which certainly, as between England and foreign countries, very considerably varies, and might possibly vary in England itself?—No doubt it would; but it would be exceedingly difficult for any legislative authority I think to decide as to when it would be necessary to do this or that, to repeat this or that. I do not see how it could be done without really stopping physiological research.

2949. You do not object I think to a record being kept of experiments?—I should not object to that.

2950. Both as regards the quantity of the animals, and the object of the experiment, and the description of the experiment?—Yes, I do not think there would be anything objectionable in that, because it is as you see all detailed and published there.

2951. You stated that you saw no objection to an inspector having access to experiments, and you mentioned that you personally would not object to the Inspector of Anatomy at Edinburgh?—I would not object to that provided that there is to be legislation.

2952. Is that inspector a medical man?—He is a medical man.

2953. I am not giving any opinion myself, but I may mention that some witnesses say that they do not consider that either the licensing or the inspection ought to be entirely in the hands of the medical profession, because, they say, to put it in very short language, that they might be led away by their objects, and by their commendable enthusiasm in their objects, and require to be checked by some layman. Do you think that the truth of that opinion is not to be admitted?—I feel convinced that the medical profession are men as humane as laymen; indeed their whole vocation is to relieve pain. I confess I think that we are anticipating matters a good deal. I feel persuaded from my own knowledge that there are not those abuses carried on by the medical profession that have been stated. I confess I think it is altogether a misrepresentation, and we are anticipating matters. Why not suppose a great many things that it would require legislative enactment to prevent? But that would be meddlesome legislation unless there actually are abuses to be corrected.

2954. Would you consider that in those countries in which they do occur or may occur, they are owing rather to the general tone in those countries in regard to the treatment of animals than to any especial carelessness about animal pain on the part of the medical profession?—As I have had no personal experience regarding alleged cruelties in vivisection on any part of the Continent, I would rather offer no opinion.

2955. As regards this experiment in the Medical Journal, would you kindly tell me again what, so far as rhubarb was concerned, was the object and likely to be the practical result of this experiment?—The object was simply to ascertain whether or not rhubarb does excite the liver.

2956. Will you kindly tell me what is the practical advantage in medicine of ascertaining that fact?—I will suppose a case. If a man have a gall stone in his common bile duct, obstruction is offered to the exit of the bile. It passes into the lymph vessels, thence into the circulation, and poisons the system. The gall stone usually finds its way slowly down the duct, passes into the bowel, and is thus got rid of. Owing to the interruption to the exit of the bile, the liver appears to become sluggish, and it is of great importance to know definitely what agents really do excite the liver to increased action, so that its normal activity may be restored as soon as possible, and thus hasten recovery from the jaundiced state. Moreover, it is quite conceivable that when all pain resulting from distention of the bile duct by the gall stone has subsided, and the stone just about to pass into the bowel, that by exciting the liver and increasing the pressure of the bile behind the gall stone its removal might be to some extent facilitated.

2957. As regards rhubarb, has it been the habit of the profession to give it for that purpose?—There have been various statements, some believing that it has the effect which I state, and others that it has not. When I did these experiments I said, "I have been trying rhubarb and a number of other things," and a medical practitioner said to me, "Oh! but rhubarb has no effect on the liver; it is all nonsense."

2958. Would the practical result then be that in the case of having to treat a patient with the gall stone you would give rhubarb?—It would be a very sensible treatment in the latter stage of the jaundice that I have mentioned, to give rhubarb or perhaps podophylline.

2959. My reason for asking this question is, that I could understand that you might obtain by this experiment this amount of knowledge, that rhubarb would have some effect in increasing the flow of the bile; but unless you ascertained that rhubarb would have considerable effect, all that you seem to me to get at by these painful experiments is, that something does a little, but not enough to be of any practical use in medicine?—I must remind you that the medicine can never be perfectly exact. We may by our experiments remove the hitherto existing doubt as to whether or not rhubarb really does excite the liver. To remove that doubt is a great step, but we shall never be able to say that in two different individuals with livers in states of torpidity from different causes that the same dose of rhubarb or of podophylline will produce the same amount of effect, or even whether in any case they will produce the same marked effect that they do in dogs. Apart from gall stones altogether, the liver is apt to become sluggish in many cases; and although these experiments on dogs cannot tell us what effect or what amount of effect will follow when the medicine is given to man, it is a great step to know that even in the dog rhubarb never fails to excite the liver, and that colchicum and other substances also do so. The great approbrium of medicine at this moment is the indefiniteness of our knowledge regarding the actions of medicines. It is now evident to all that the proper way to diminish this ignorance is for the physiologist to perform such experiments as these on the lower animals, and then to submit his results to medical practitioners, so that they may obtain therefrom suggestions regarding the employment of these medicines on man.

2960. Then I am to understand that from that knowledge of physiology you say that the facts you obtained from these experiments are not only of importance as increasing the general range of knowledge, (and I do not dispute that that is important, because I know that inferences may be drawn from it,) but that they are of practical advantage in the actual practice

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of the medical profession?—Yes, they are calculated to prolong human life and diminish human suffering.

2961. (*Sir J. B. Karslake.*) As I understand you, in the experiment which has been referred to, you consider that you have established the fact that the action of rhubarb upon the liver is to increase the flow of bile?—Yes, in the case of the dog.

2962. Supposing the case of the dog and the case of the human being to be analogous, then, as I understand you, you say that you leave it to the medical men to determine whether they will use that rhubarb which you have shown has the effect, in the case of the dog, of affecting the flow of the bile in the liver?—Yes. I show the fact on the dog, leaving it to others to experiment on man.

2963. And as I understand you, in the instance which you gave of the gall stone, what you anticipate would be the effect would be that the mechanical action of the increased flow of bile might discharge it also?—It would probably facilitate the passage of the gall stone into the intestines.

2964. In the case of colchicum, which you have also spoken of, you have established, you say, in your judgment, that colchicum administered to a dog increases the flow of bile in the liver?—Yes.

2965. And again you leave it to the medical man to determine whether, you having told him that, he will apply it to the human subject?—Yes. It has been suspected to have this action upon the liver in man, but has never been definitely proved to have it. The fact that it does produce this action in the case of the dog strengthens the belief that it does so also in man.

2966. Passing to another question, in your judgment and your own experience, are operations of that description upon the dog to be taken as being evidence of what the effect would be on the human being?—Certainly not, but merely as suggesting what the action would be; that is all. The experiment must also be tried upon man before a conclusion can be drawn.

2967. Supposing it has been stated that in the case of colchicum, or some other drug, the operation upon man would be so different from what it would be on the dog that it would be useless to try the effect of using it on the dog for the purpose of ascertaining what the effect would be on man, what would you say?—That is hypothetical. No conclusion can be drawn until the experiment is tried.

2968. Now with reference to your own education, as I understand you, having gone through the education which you could acquire in England, you found it necessary to go to Berlin for the purpose of perfecting that education in regard to physiological research?—Yes; and also for the purpose of getting to know really something more about the mechanism of the body, in order to comprehend diseased conditions better.

2969. May I ask you how many years ago it was, about, that you studied at Berlin?—It is eleven years ago.

2970. At that time were there any means by which you could acquire the knowledge which you desired in England or in Scotland, and which you did acquire at Berlin?—No.

2971. Do you think it was essential in order to complete your education that you should see that series of experiments which you witnessed at Berlin?—Yes, both for the purpose of becoming a physiologist, and also in order to have a more perfect education as a medical man.

2972. You think that for both purposes it was necessary?—Decidedly.

2273. And you could not acquire that knowledge in this country which you acquired at Berlin?—No, not at that time; but I could now.

2974. Then, how lately was it that you went to Leipsic for the purpose of seeing further experiments?—Five years ago.

2975. Could you have witnessed those experiments in this country at that time?—No. I did not see the whole range of experiments when I was in Berlin; and in Edinburgh when I became a teacher of physiology I had to really educate myself to a large extent, and I went to Leipsic to see further modes of experimenting, which I had not the opportunity of seeing in this country.

2976. Now you have told us to what extent as a teacher you demonstrate by experiments on animals certain matters which you consider as essential for the students to understand?—Yes.

2977. Do you think that the students under your charge would be sufficiently and duly educated without witnessing those experiments?—Certainly not.

2978. Let me take one instance that you gave us more than once, that of showing to the students who are under your charge the effect of strychnine; did you administer that to a frog?—I gave it to a frog.

2979. In your opinion is it essential for a student not only to know from reading what the effect of strychnine is but also to see its effects?—I think it is a great advantage for him to see the effect, so that he may recognise it in the human subject in the case of poisoning by strychnine at once, and moreover to have a right comprehension of what is meant by a state of tetanus, and to have it demonstrated to him by a series of experimental steps that tetanus from strychnia is not due to direct action of the poison on the muscles or nerve trunk, but upon the spinal cord. Not only does the student learn this in the case of strychnia, but he also learns the mode of investigating the action of any new substance which may act like strychnia.

2980. A gentleman who has been examined as a witness before the Commission says, as I understand him, that there is nothing which you could learn from seeing an animal under the influence of strychnine that you could not learn from reading a description of the effect of strychnine upon an animal. Is that your opinion?—I thoroughly disagree with that statement. I am convinced that in many matters it is impossible from reading to gain such an idea of a subject, or of a fact, as can be obtained from seeing the thing; the impression produced upon the mind is so much more vivid, and the impression is so much more true. I believe it is not possible in many facts of physiology for a student to get a sufficient idea of the matter from books. For example, in experiments on the heart, there is a nerve which when stimulated retards the heart's action. There is a poison, belladonna, which when given, prevents the action of that nerve upon the heart. The student could not apprehend these matters so fully and be so thoroughly impressed by them by reading as by seeing.

2981. Is it in the belief then that it is essential that the student should see the operation of those poisons upon animals that you find it necessary, or that you think it necessary, in the course of your instruction, to show those experiments to them?—Yes. But let me say that showing the action of poisons is certainly the least part of the whole. It is essential to see the action of a few poisons on animals, but it is more essential that they should see a number of other experiments on animals, in order that they may comprehend the mechanism of the body.

2982. And is it in that belief that the experiments that you introduce are shown to the students under your charge?—Only so.

2983. (*Mr. Hutton.*) From what you say I take it that the question of whether or not an experiment is desirable for demonstrative purposes is very much a matter of opinion among physiologists. Can you tell us whether there is any great difference in that respect between the German schools and the English?—I do not know that there is any difference now in the matter of opinion as to whether or not it is desirable to have such experiments for demonstrative purposes.

2984. I mean to say as to the number of painful experiments?—I think there is a difference of opinion. I believe in this country we are more careful about repeating a painful experiment.

2985. You seem to think that the German schools are on the whole very moderate in that respect?—The

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German schools which I visited appeared to me to be moderate.

2986. But still they would go further than the English?—Well, I think rather further than I should go for the tuition of medical students.

2987. In Berlin and Leipsic did you see demonstrative experiments of a more painful kind than you would show in your own school?—In Berlin I did, but these experiments were shown to me as a person desirous of becoming a physiologist.

2988. Then if this school of physiological experiments extends to England, it is more or less likely that men of that way of thinking might grow up in England also; it is a matter of opinion?—No doubt it is a matter of opinion.

2989. It is simply, I suppose, that the Germans are as tender to pain generally as the English, and that in the zeal of science in Germany they do as a matter of fact find it necessary to show more experiments than we do?—I must decline to make any comparisons between the Germans and the English, because, although my physiological friends in Germany showed me some experiments which I do not think it necessary to repeat merely for the tuition of medical students, they showed me these experiments at my own request, and I thank them heartily for the gain to my knowledge. I really have had no personal experience of any unnecessary or reckless vivisection on any part of the Continent.

2990. Now can you give me any impression at all of what you might call the consumption of living animals for all purposes, research and otherwise, in your laboratory in Edinburgh?—That there is so much I can tell you for teaching purposes.

2991. For teaching purposes I think you have already published the numbers, have you not?—Yes; but as to research the number must vary with the research. For teaching purposes in systematic lectures I use two dogs, three rabbits, and a considerable number of frogs in the course of the year. In the class of practical physiology, in the course of a year I use not more I think than six rabbits, and a considerable number of frogs, that is for teaching purposes.

2992. (*Sir J. B. Karslake.*) And nothing else?—I should say we may use guinea pigs; I really have never used guinea pigs, but they might be used. I do not use them for teaching purposes; that is all that I use for teaching purposes.

2993. (*Mr. Hutton.*) Then for purposes of research how many animals do you use?—For purposes of research the number of animals used varies with the research that may be undertaken. Last year for purposes of research I think I used about 40 dogs perhaps, and of rabbits, at the very outside, eight.

2994. (*Mr. Forster.*) Did you use any cats?—No cats, and no guinea pigs. I should say further that that number of dogs is quite an exceptional thing.

2995. (*Mr. Hutton.*) That was the largest that you have ever used?—Yes; during the whole five years that I was in London, for the purposes of research, I am sure I did not use more than 10 dogs; this is quite an exceptional thing.

2996. Then I think that in a published letter of yours you state that you occasionally allow students to operate under your direction?—I did not state so.

2997. That is so reported, is it not?—I said that they might be permitted; they never are for original research. Let me state exactly what students do in Edinburgh. They perform no experiment upon a living animal, excepting that of demonstrating the circulation of the blood under the microscope. If any student desired to undertake an original research I should allow him to do so under my superintendence, provided that I were persuaded that he was competent to perform the necessary operations, and that the subject was worth it. Such a case has never occurred during my ten years experience.

2998. Now can you give me the least impression as to the yearly consumption at a great laboratory like Ludwig's, at Leipsic, of living animals?—I cannot, because I was only there one month.

2999. Can you supply that information as regards Berlin?—No; I only attended one lecture by Dubois Reymond; I had simply a private course of instruction in the laboratory. The number in Berlin is not large, because they have not done much experimentation on animals.

3000. It might be taken to be very much larger than the English, I suppose?—I am quite unable to offer any opinion.

3001. You say that you have noticed a considerable difference in the treatment of human patients abroad as compared with their treatment in this country in hospitals—more disposition to experiment on them, I suppose you mean?—No, not at all.

3002. I understood you to say that you had noticed a difference in the treatment?—I noticed that there was less delicacy in the treatment of human beings in some parts of the Continent that I visited, but I hope I am not to be asked to say where.

3003. Do you not connect that in any degree with the deadening practice of constantly operating on living animals?—Certainly not.

3004. You think it is a matter purely of national temperament?—Purely.

3005. Do you think there would be any practical inconvenience in excluding what we may call the domestic animals altogether from these experiments—dogs, cats, and horses?—I think it would be distinctly not only inconvenient—but it would be a great blow to the teaching of medicine and physiology in this country.

3006. In what way?—Because the students would not be educated as they ought to be—not so well as they can be.

3007. Why could you not show the same experiments on animals that are not domestic animals?—You cannot possibly. For example, if you want to measure the pressure of the blood, it is scarcely at all to be done in a frog; the animal to take is a dog or a rabbit.

3008. Or a guinea pig?—A guinea pig one never takes for this purpose. A student will not gain a sufficient idea of what is meant by the blood pressure in the human subject unless you take an animal which in some measure represents man; not a frog of course, but a dog or a rabbit. We never take a horse.

3009. A very eminent man tells me that he should like to give evidence that experiments in arterial pressure are entirely unnecessary before a class—that they might be given through hydraulic machinery, which would show exactly the same; is that your opinion?—Certainly not, it would not show exactly the same. But by all means let the eminent man give his evidence.

3010. Where you speak of chloral and opium, those I suppose have not at all the same effect as chloroform; they are simply naraotics; they stupefy, but do not take away pain; they diminish the sense of pain, but do not destroy it; is not that so?—I do not know why you should suppose so, because the evidence is so exactly the opposite.

3011. You think that opium and chloral destroy pain as much as chloroform, do you?—Yes, if you give them in sufficient doses.

3012. Now I understood you to say that the action of the drugs on which you had experimented might be very different on dogs from that on man. Is it not true that it is very different as regards calomel?—I should not say it is true that it is very different; I do not know who has proved that to be true. I say that is a question open for consideration.

3013. I thought it had been demonstrated to be so?—That is a question much disputed.

3014. But you have not come to any conclusion yourself on the subject?—No; because I have not performed the experiments on man.

3015. But were not the differences between you and your predecessor, as regards the results of those experiments, considerable, although you explained them, I believe, by the fact that your experiments

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were on fasting animals and his on non-fasting animals ?—Yes, the conclusions drawn regarding podophylline were different.

3016. Does not that suggest that the same experiment must be repeated to see whether that was the cause of the difference ?—It might be; I do not know that it would be necessary, but still it might be.

3017. My point is, that on the whole the results are purely approximate, and may be subsequently disputed ?—Well, I could conceive that they might be disputed, although I do not think that there is any reasonable ground for disputing them.

3018. I understand you to say (and that is a point on which I am rather anxious to get a distinct answer) that you find dogs and cats so very much more useful than other animals for the purposes of experiments that it would be destroying your practical physiology to exclude them ?—I said dogs and rabbits are required in addition to frogs; I am distinctly of that opinion.

3019. The rabbit, I take it, is not a domestic animal; would not rabbits alone be sufficient for your purposes? I believe they are less sensitive, are they not, than most other animals ?—I do not think that rabbits alone would suffice; I think it is necessary to have my two dogs.

3020. Would not your experiments on podophylline and other substances have succeeded equally well if tried on rabbits ?—My very first experiment with podophylline was tried on a rabbit, and I got no result; I tried it on a dog and got a result similar to what is supposed to take place in man.

3021. Is it not conceivable that somebody might say that the results were spoiled by your use of curari ?—Quite conceivable, but not likely, because I give sufficient reason for stating that it is not likely in the report.

The witness withdrew.

Mr. William Turner, M.B., called in and examined.

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3022. (*Chairman.*) You are Professor of Anatomy in the University of Edinburgh ?—I am.

3023. In that capacity do you yourself come into connexion with the subject of experiments upon living animals ?—I do not myself for class purposes, for demonstration purposes, experiment on living animals; my work as a teacher lies with dead bodies, not with living animals.

3024. Have you given much consideration to the subject which is referred to us ?—I have.

3025. Will you state what is the result of that consideration ?—I have made a few jottings to assist me in what I have to say on this matter, and I propose in the first instance to speak of the practice of submitting living animals to experiments for scientific purposes in its bearings on the advancement of physiological science generally. With this object I have made notes of the history of various discoveries, and I propose to take in illustration the discovery of the circulation of the blood, the discovery of the lacteal and lymphatic system of vessels, and the discovery of the compound function of the spinal nerves; and I have selected these three subjects for illustration because they lie at the very foundation of all our present physiological knowledge; without them physiology, and consequently practical medicine, would be a perfect chaos; and moreover, the history of the discovery of these matters is so well ascertained and so generally accepted that I do not think that any exception can be taken to the narrative which I shall give. With regard to the discovery of the circulation of the blood, this, as the Commission of course know, was made by the immortal Harvey, our own countryman, who published his memoir on the motion of the heart and of the blood in the year 1628. Before the time of Harvey many important observations had been made by anatomists, and many facts discovered. The structure of the heart, the difference between the arteries and the veins, the presence of valves in the veins, were all known; and these were discoveries which were necessary as a preliminary to the discovery of the circulation of the blood itself. These discoveries were all anatomical, made by observation on dead bodies; but in order that the discovery of the circulation of the blood should be made, it was necessary that the living body should be examined; and I may select one or two passages from the memoir of Harvey, to which I have just referred, so that the Commission may have before them Harvey's own testimony on this matter. In the first chapter of his memoir, as rendered by his translator Dr. Willis, I find the following statement: "When I first gave my mind to vivisections, as a means of discovering the motions and uses of the heart, and sought to discover these from actual inspection and not from the writings of others, I found the task so truly arduous, so full of difficulties, that I was almost tempted to think, with Fracastorius, that the motion of the heart was only to be comprehended by God. For I could neither rightly perceive at first when the systole and when the diastole took place, nor when and where dilatation and contraction occurred, by reason of the rapidity of the motion, which in many animals is accomplished in the twinkling of an eye, coming and going like a flash of lightning; so that the systole presented itself to me now from this point, now from that, the diastole the same; and then everything was reversed, the motions occurring, as it seemed, variously and confusedly together. My mind was, therefore, greatly unsettled, nor did I know what I should myself conclude, nor what believe from others. I was not surprised that Andreas Laurentius should have said that the motion of the heart was as perplexing as the flux and reflux of Euripus had appeared to Aristotle. At length, and by using greater and daily diligence, having frequent recourse to vivisections, employing a variety of animals for the purpose, and collating numerous observations, I thought that I had attained to the truth, that I should extricate myself and escape from this labyrinth, and that I had discovered what I so much desired, both the motion and the use of the heart and arteries, since which time I have not hesitated to expose my views upon these subjects, not only in private to my friends but also in public in my anatomical lectures, after the manner of the Academy of old." Then in the second chapter of the same treatise is a passage which refers to the same subject, and states what the animals are on which he made his observations. These statements are as follows: "These things are more obvious in the colder animals, such as toads, frogs, serpents, small fishes, crabs, shrimps, snails, and shell fish. They also become more distinct in warm-blooded animals, such as the dog and hog, if they be attentively noted when the heart begins to flag, to move more slowly, and, as it were, to die; the movements then become slower and rarer, the pauses longer, by which it is made much more easy to perceive and unravel what the motions really are, and how they are performed. In the pause, as in death, the heart is soft, flaccid, exhausted, lying, as it were, at rest." In the sixth chapter of the same treatise there are also observations bearing on the same matter, and showing how errors had arisen through the non-employment of vivisection by previous observers. He says, "Since the intimate connexion of the heart with the lungs, which is apparent in the human subject, has been the probable cause of the errors that have been committed on this point, they plainly do amiss who, pretending to speak of the parts of animals generally, as anatomists for the most part do, confine their researches to the

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"human body alone, and that when it is dead. They obviously act no otherwise than he who, having studied the forms of a single commonwealth, should set about the composition of a general system of polity; or who, having taken cognizance of the nature of a single field, should imagine that he had mastered the science of agriculture; or who, upon the ground of one particular proposition, should proceed to draw general conclusions." Then it may be a matter perhaps of some interest to the Commission to know that when Harvey had published his very important treatise, objections were raised to his results on the ground that he had got at them by practising vivisection—so that in his time, as in the present day, objections were made to the use of this method of inquiry as an aid to physiological discovery; and it may be interesting, perhaps, just to read a passage from his letter to Riolanus, who was a noted anatomist of the period: "There are some, too, who say that I have shown a vain-glorious love of vivisections, and who scoff at and deride the introduction of frogs and serpents, flies, and others of the lower animals upon the scene, as a piece of puerile levity, not even refraining from opprobrious epithets. To return evil speaking with evil speaking, however, I hold to be unworthy in a philosopher and searcher after truth; I believe that I shall do better and more advisedly if I meet so many indications of ill-breeding with the light of faithful and conclusive observation." These observations of Harvey are those by which the circulation of the blood was established. But there was one link yet wanting in the chain. That link could not be supplied by Harvey because in his day the means of observation were not at his disposal. It was, however, supplied in 1661 by the Italian anatomist, Malpighi, who by observations with the microscope made upon the lungs of a living frog saw the passage of the blood from the arteries into the veins through the capillaries; and thus, by observations on a living animal, completed the discovery. The next illustration that I wish to bring forward is the discovery of the important system of vessels known as the lacteal vessels, or the lymphatic vessels, vessels which are intimately concerned in the absorption of the food and in the process of nutrition. In short, the observations and discoveries on these vessels lie at the very root of all our knowledge of digestion and the assimilation of the food. The first observation on the discovery of the lacteal vessels was made by the Italian anatomist Asellius in the year 1622. I may explain that these vessels are of a very remarkable character; they are, as a rule, extremely minute; their coats are so transparent that they cannot be seen excepting when they are filled; they must be filled either with the chyle which they convey from the bowels, or by some artificial means. The best means of seeing them is when they are filled with their natural fluid, that is the chyle; and their chyle contents are only to be seen at the time when digestion is going on. After the food has passed away from the bowels the chyle is no longer in these vessels; they must be seen, therefore, when digestion is going on. Now Asellius discovered these vessels by opening the body of a living dog at the time when it was digesting food; and then they became a subject of frequent demonstration after that period. But although Asellius discovered them he did not entirely comprehend what their direction and course was. He supposed that they ended in the liver. It was reserved for the French anatomist, Pecquet, in 1649, to determine that these vessels terminated in the great veins at the root of the neck, and that the chyle or absorbed material of the food was poured into the venous system to mix with the blood, and so flow onwards in the course of the circulation and be applied to nutritive purposes. This observation of Pecquet's was also made on a living animal, and he determined the passage of the chyle along the thoracic duct by tying it and observing that the chyle flowed up to the seat of ligature, and that then the vessel became distended immediately behind the seat of ligature. The attention of anatomists and physiologists having been directed to these vessels, they have been experimented on and observed with great care since that period, and even now the attention of two very eminent physiologists in this metropolis, Dr. Burdon Sanderson and Dr. Klein, is being directed to these lymphatic vessels, and there is every reason to believe that the observations and experiments which they are making on this system of vessels will tend in the most material manner to the advancement of medical practice, as well as of physiological science. The third illustration that I have selected is the discovery of the compound function of the spinal nerves. Here again anatomy had led to a certain amount of knowledge. It was known that each spinal nerve arises by two roots; but until the beginning of the present century no one had supposed that these roots of the spinal nerves had different functions. An Edinburgh anatomist, Mr. Alexander Walker, published in 1809 certain speculations on the roots of these nerves, which were based entirely on anatomical observation without the use of experiments. He supposed that the anterior roots of the nerves were concerned in sensibility, and that the posterior roots were concerned in volition. If Mr. Walker had resorted to experiments he would soon have satisfied himself that, although he was quite right in the supposition that the roots of the nerves had different functions, he had made a most important error in ascribing to the anterior root sensibility and to the posterior root volition. At the same time that Mr. Walker was speculating on these questions, Sir Charles Bell, then Mr. Charles Bell, was considering the matter; and from extracts from some of his letters which have been published it appears that in the years 1807, 1808, and 1809 he had been pondering in his mind the functions of the nerves. I may quote a sentence from a letter of his dated the 4th of December 1809, written to his brother, in which he says "I shall be very bloody in this brain of mine," (that is in the researches which he was contemplating,) "I must make experiments, and that is what I hate to do." Then in a letter, dated the 2nd of March 1810, he says: "It occurred to me that, as there were four grand divisions of the brain, so were there four divisions of the spinal marrow; first, a lateral division, then a division into the back and forepart. Next, it occurred to me that all the spinal nerves had within the sheath of the spinal marrow two roots, one from the back part, another from before. Whenever this occurred to me I thought that I had obtained a method of inquiring into the function of the parts of the brain. Exp. I.—I opened the spine, and pricked and injured the posterior filaments of the nerves; no motion of the muscles followed. I then touched the anterior division; immediately the parts were convulsed. Exp. II.—I now destroyed the posterior part of the spinal marrow by the point of a needle; no convulsive movement followed; I injured the anterior part, and the animal was convulsed." In these experiments, Sir Charles Bell made certain discoveries; he showed that the anterior root, instead of being, as Walker supposed, concerned in sensibility, was really concerned in influencing the movements of the muscles because the parts were convulsed when the root was pricked. But Sir Charles Bell failed at that time to discover what the function of the posterior root of the spinal nerve was. It required many more experiments both on his part and also on the part of the French physiologist Majendie to determine precisely, as we now understand them, the relative functions of the two roots of the spinal nerve.

The second head into which I had arranged my observations was with reference to the influence exercised by experiments on living animals in making improvements in the art of healing, the medical art. And in the first place I propose to direct attention to the experiments of Dr. Jones made in the beginning of the present century as regards the suppression of hæmorrhage from divided arteries, and also as regards

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the use of the ligature. With regard to these experiments I might perhaps be allowed to make one or two quotations from the writings of eminent surgeons as to their great importance; and I would first read a passage from an essay written by the late Sir William Lawrence, who was eminent not only as a practical surgeon, but also for his knowledge of all matters connected with the literature of surgery. He says, speaking of Dr. Jones, "He has banished the use of thick and "broad threads, of tapes, of reserve ligatures, of cylin-"ders of cork and wood, linen compresses, and all the "contrivances which, employed as a security against "bleeding, only served to multiply the chances of its "occurrence." The late Mr. Samuel Cooper, who was also an eminent surgeon and the author of an important work on surgery, says, "The principles "which should guide the surgeon in the use of the "ligature were not known until the late Dr. Jones "published his valuable treatise on hæmorrhage." And a living surgeon, Professor Spence, one of my colleagues in the University of Edinburgh, who is entitled to speak authoritatively as to the value of Dr. Jones's experiments because he himself has performed many experiments in the same direction, says, "When we look at the principles laid down by Dr. "Jones as the results of his investigations, and for "which he has received so much credit, it will be "found that the great service he performed consisted "not so much in pointing out any new fact observed "in the process of obliteration of an artery as in scien-"tifically investigating the subject as a whole, ascer-"taining in a great measure the relative value of the "different parts of the process, and drawing from his "experiments sound practical deductions as to the "causes of secondary hæmorrhage, and as to the best "means of procuring obliteration." From the opinion of these distinguished surgeons, as well as from the fact that the principles which Dr. Jones advocated, are daily applied in practice, I think there can be no question that his experiments have exercised a great influence on the improvement of the healing art. With reference to his experiments Dr. Jones, in the preface to his book, makes some observations which may perhaps be interesting to the Commission. He says, "He has only a "few words more to say, addressed to men out of the "pale of his profession into whose hands this little "book may fall, whose opinions he esteems, and "whose feelings he honours. He regrets the necessity "of obtaining even this important knowledge by the "sacrifice of brutes. But when we remember the "incessant scourge of war which has followed man "through all the ages of his history—not to mention "the consequences of accident and disease—it is not "too much to assert that thousands might have been, "and may still be, saved by a perfect knowledge of "these subjects; which can only be directly obtained "by experiments on brutes; indirectly, and very "slowly, by observations on the injured arteries of "man; and even these cannot be made until he has "fallen a sacrifice to the want of assistance, or to the "imperfect knowledge of the surgeon."

3026. (*Mr. Forster.*) What was the date of that?—This book of Dr. Jones's was published in 1805.

3027. (*Mr. Erichsen.*) He made his experiments chiefly on horses, I think?—Horses, asses, and dogs. The experiments also made by John Hunter on the arteries of dogs, with reference to the treatment of the serious disease of the arteries called aneurism, have been of great value in guiding surgeons to the best mode of treating this disease. The experiments of Drs. Hope and Williams are of importance in determining the cause of the sounds of the heart, and in enabling the practical physician to diagnose certain of the diseases of that important organ. Then the important experiments which were made by Dr. Jenner in connexion with the subject of vaccination may also be referred to. It is well known that the discovery of vaccination was made by Dr. Jenner from observations on cows, and those who milked cows, in the dairy county of Gloucester. He observed that cows had occasionally a pustular eruption on the udder, that those who milked cows so affected, contracted pustules on their hands, and that such persons enjoyed an immunity from small-pox. From these observations he proceeded to investigate carefully the whole subject, and established the practice of vaccination which has been of such enormous importance to humanity. But Jenner's observations were not limited to cows, for there was another step in the process, namely, the prior communication from the horse to the cow of matter, which occasioned in the cow the pustular eruption. Jenner observed that those cows which had their udders affected had been milked by persons who had been tending horses having an affection of the hoof, called grease of the hoof. The farm servants who attended on these horses went and milked the cows, and in this manner from the horse to the cow matter was communicated which led to the production of pustules on the udders of the cows. This is brought out not only in Jenner's original memoirs on vaccination, but also in the Life of Jenner compiled by his biographer Dr. Baron. Jenner himself, in order to establish a complete history of the transference of this matter, was desirous of inoculating a cow from a horse, but it so happened that, as he states in a letter which is quoted by Dr. Baron, he could not procure a horse at the time which had the grease in its heels, so that he himself was not able to carry out the experiments, although he was strongly urged to do it by various of his friends. But so important was it considered that those experiments should be carried out, that it was done by another observer, a Mr. Tanner, and Dr. Baron states: "Mr. Tanner had the merit of "proving the truth of this doctrine; he succeeded in "communicating the disease to the cow by inserting "some liquid matter taken from the heel of the horse. "This produced on the teat of the cow a complete "vaccine pustule." The whole history of the discovery of vaccination, a discovery which is second to no other in the benefits it has conferred on the human race, proves that it is based on experiments on living animals, the full meaning of which was interpreted by the acute and philosophic mind of Jenner. Then lastly I would refer to the experiments performed by the Italian physician Galvani in the year 1786. These experiments are of enormous importance in their results, not merely in their bearings on the science of physiology, on the medical art and its improvement, but upon the arts generally and upon the comforts and welfare of mankind. The experiment of Galvani's which is of importance in connexion with this matter was one performed in the course of a series of researches which he was carrying out on frogs relative to the determination of animal electricity. He found that by applying a plate of silver and a plate of copper to the nerves and the muscles of a frog he excited contraction of the muscles of the frog. Now that experiment, simple though it may appear, is the fundamental experiment which has led through the researches of Volta, Davy, Faraday, Œrsted, Thomson, and others, to all that we know about current electricity, about electro-magnetism, and about magnetic electricity, with all their applications to therapeutics, to electric telegraphy, to electro-plating, and to lighthouse electricity. All these most important applications take their origin from this experiment of Galvani's on a frog. Such were some of the observations which I had thought of in connexion with this subject.

3028. (*Chairman.*) You have adduced these various cases for the purpose of showing generally what you consider to be the necessity of occasional recurrence to experiments upon living animals?—I have.

3029. Now coming down to the present time and to our practice, are the majority of experiments capable of being performed under the newly discovered anæsthetics, or some of them?—A large proportion of experiments may be performed either on animals that are anæsthetised or on animals that have had their heads cut off, and that no longer have sensation.

3030. For the purposes of demonstration to students, do you consider experiments on living animals to be

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necessary?—In some cases they may be, but I am by no means an advocate for the indiscriminate performance of experiments before students; I think it should be limited.

3031. You think that the use of experiments on living animals at all for purposes of demonstration to students should be very limited?—I think so.

3032. If limited to experiments performed always under complete anæsthesia, would that be the sort of limitation to which you would point?—It is possible that experiments might have to be performed on animals not under the influence of anæsthesia, but I should say speaking generally that the great majority of them might be performed under its influence.

3033. Speaking generally, experiments for the purpose of demonstration to students may quite well be performed under anæsthetics?—Speaking generally.

3034. Now with regard to experiments for original research, may the greatest number of them be performed under anæsthetics?—I believe so.

3035. And of those that cannot be performed altogether under anæsthetics, may a large part be so performed that the most painful part of the operation is under anæsthetics?—I believe so.

3036. Leaving, therefore, only a limited portion of the whole, where in your judgment it would be impossible to take away the sense of pain?—Yes.

3037. If an experiment has once been performed, and the result completely established, do you hold it to be necessary to have a recurrence to that experiment?—Well, that opens the question when is a result completely established? That is a question which is not perhaps at all times very easy to answer. There must be a certain verification of experiments. Men of science do not as a general rule admit a new proposition without testing it.

3038. Supposing a proposition to have been so tested as to have been generally admitted, and the process of arriving at it to have been very painful, would it in your judgment be justifiable to have recurrence to it?—With regard to such experiments for example as I have referred to in connexion with the brief history of the subject that I have given, they of course are admitted, and I do not see that there would be any necessity to repeat them for the mere purpose of instruction.

3039. Experiments of this kind ought to be performed, I presume, by very competent persons?—Certainly.

3040. It would be unjustifiable to perform an experiment of this kind in the case of a person who is untrained, or inexperienced, or ignorant?—Certainly.

3041. Is the number of persons who, in your judgment, could usefully perform experiments of this kind very great?—Not in this country.

3042. Would the restraint of such persons, that is to say, incompetent persons, receive your approval?—Then would arise the question who is to judge of the competency or incompetency of the person.

3043. No doubt; but I suppose that, for instance, in Edinburgh, it is pretty well known to yourself and to those eminent men with whom you act, who are competent and who are not?—Undoubtedly we know who are competent; but I am not prepared to say that I know every one who is competent. There may be persons competent, and of their competency I may not be aware.

3044. Everything human is more or less exposed to uncertainty perhaps, and difficulty; but is it not as certain who are competent people to perform painful experiments as who are competent in almost any other branch of knowledge or practice?—Undoubtedly with regard to men who are well known, who hold official positions, positions as teachers, and so on, of their competency we may be well aware. We may assume that. But still there may be persons who are quite competent to perform experiments, and who might advance not only physiological science but also the healing art, with whom I and others are not acquainted.

3045. Must not a person to become competent pass through some school and receive some teaching?—Certainly.

3046. And will it not be known to those who have taught him whether he is a competent person or not?—I suppose that one may to a large extent admit the accuracy of your view. I do not wish to refine too much in the matter, but still I do not wish to commit myself to the statement that I know, in Edinburgh for example, all competent persons. That is the only exception I wish to be made.

3047. Making that exception, you would argue that speaking generally it is pretty well known, or may be known, who are the competent persons in a country to pursue such investigations as those of Harvey, or Jenner, or Jones?—Yes, speaking generally.

3048. Now would it be in your opinion any restraint upon physiological science if the performance of painful experiments upon living animals were limited to those who are known to be competent?—If you mean limited by legislative measures I have difficulties in the way of legislation on this matter. Perhaps you would allow me to state them. In the first place I have a difficulty in connexion with the expression which I find in the letter of your secretary to me as to the object of the Commission, "the practice of subjecting live animals to experiments for "scientific purposes." In what sense are you to employ the term "live animal" or "living animal." Is it to be supposed that the meaning of the term "living animal" is to be defined by Act of Parliament? I apprehend that if there were to be legislation on this matter it would be necessary to lay down what is a "living animal" or what is "life." Now to give a definition of life has been a difficulty from the time of Aristotle to the present day; and by referring to the history of physiology you will find that physiological writers are by no means agreed as to what is meant by the term "life." Take a frog, for instance, that has had its head cut off, the muscles and nerves of which are capable of responding to a stimulus; are we to consider that frog a living animal? There is a difficulty. A frog in that condition, with its head cut off, and with its muscles and nerves capable of responding to a stimulus, is an animal of the utmost importance in physiological research. There is life in the muscles and life in the nerves so far as regards the capability of responding to a stimulus, but that is not life in the animal itself according to the ordinary conception of the term; so that this is a difficulty which faces me at the very onset—are you to define by Act of Parliament and determine by statute what is a living animal?

3049. Then one of your fundamental objections, or perhaps the fundamental objection to any interference by the legislature is the difficulty of drawing a distinction between an animal that is living and an animal that is not?—Yes, that is a difficulty.

3050. Are there any other difficulties?—Then I find a difficulty in this respect, that vivisection is by no means exclusively practised by men of science; that vivisection is extensively performed by others than men of science; and that if restrictions are to be made by legislative enactments for the purpose of preventing men of science from operating on living animals, one does not see why in common justice others should not be also prevented from cutting living animals, and yet, if this were done, it would lead to difficulties and complications in one of our most important industrial occupations. I may just give by way of illustration of this matter some few facts that I have collected with reference to the practice of vivisection on the farm. I was inquiring the other day of a farmer on the Borders, who has a large number of sheep, how many lambs he had in the year. He told me he had 600 lambs, that was his average number. I said "How "many of these lambs are males?" "About half the "number." "Do you castrate these males?" "Oh yes; "we castrate them; we castrate some 300 lambs each "year; and in addition to castrating them, we cut off "their tails, or we remove considerable portions of their

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" tails; we find it useful to have the animal's tail " shortened. And then in addition it is necessary ' also that the lambs should be marked, which is done " partly by snipping portions out of the ears, and " partly by branding them." Over the whole country therefore many thousands of lambs are annually subjected to the painful operations of castration, tail docking, and branding, without chloroform or other anæsthetic being administered either by the farmer or shepherd. Moreover, calves and colts are also castrated in large numbers annually. Now all these are vivisections practised upon living animals for certain purposes—important purposes undoubtedly, because they improve the quality of our mutton and beef and make it more palatable to the consumer, or enable the farmer to recognise his own flock from that of his neighbour, or furnish us with more useful beasts of burden than if entire horses were employed.

3051. But suppose the object of the legislation was to confine scientific experiments to persons of the highest competence in science, and to exclude from the performance of such experiments those who had not got that competence, would that hinder the progress of scientific discovery?—Well, I can see difficulties even in that matter. I can imagine that cases of importance might arise in which a man who has not an established name in science might find it necessary to perform an experiment on a living animal; and if you will allow me I will give you an illustration. Suppose that a medical man in some country town or district is attending a patient with certain symptoms, which perhaps he may consider to be due to the administration in the food or medicine of a poison by some one in the house of the patient. We know that a very good means of determining the poisonous action of a substance is by administering a portion of that substance to a living animal, and by watching its effects on that animal. In such a case as I refer to, the medical man in charge might obtain a portion of the suspected food or medicine administered to the patient, and give that to a living animal, and so from the symptoms observed on that animal ascertain whether it contained poison. Now if you were to restrict the experimenting on living animals only to such competent persons who let us say had been licensed to employ animals for scientific purposes, you would prevent this medical man, unless he happened to be one who had been so licensed, from administering the suspected matter to an animal, and so perhaps the life of his patient might be sacrificed.

3052. Would not it be more natural in that case for the medical man to analyse the medicine or the food?—That is undoubtedly one method of getting at the result; but a large number of medical men have not the necessary knowledge to perform a careful analysis, and there are many substances that when mixed with food, or with various medicaments, are extremely difficult indeed to detect; and a much more simple way of detecting their presence would be by an experiment on an animal. Take, for example, strychnia; strychnia is a substance extremely difficult to detect by analysis, except by an expert, but the administration of something containing a solution of strychnia to a living animal very soon shows its presence by the tetanic symptoms produced.

3053. Would you have the patient go on taking the suspected medicine or food while the animal was being experimented on?—The experiment could be performed without any loss of time, so that the poison, if present, could be at once detected, and there would not be the delay which would be required if a complex chemical analysis had to be performed.

3054. Another principal objection of yours to legislative interference would be the prevention of a medical practitioner who by the hypothesis is not competent to analyse food and medicine, from trying experiments on living animals as to the effects of that food or medicine?—That is one of my difficulties.

3055. Are there any others?—There is another difficulty, that when legislative checks are enacted, the progress of discovery in science is likely to be impeded. I might put this case: suppose that Galvani had lived in a country where legislative restrictions had been in force, and that he had not been a person who had received a license to practice on living animals, he might have been stopped at the outset of his discovery, and the whole progress of electrical science might have been delayed for half a century or more. I think this, that it would be unwise by legislative measures to do anything that would be likely to check the progress of science, because I hold that the welfare of mankind is intimately bound up with scientific advancement.

3056. But supposing that it be admitted for the sake of the present argument that nothing is to be done which will check the advancement of science, would it not depend upon the nature of any restrictions which the legislature might impose, whether they would have that tendency or not?—If there is nothing in the legislation to check the advancement of science then of course this part of my argument disappears. What I feel is this, that as a man pursuing a scientific career, I have a difficulty in admitting the wisdom or expediency of making any legislative enactment that would be likely to interfere with the progress of science.

3057. (*Lord Winmarleigh.*) I understand that those three points that you have given to us, you have brought before us as instances of benefit derived from vivisection which would not have been derived from any other source than from experiments on living animals?—I have.

3058. Do you think that there is an impossibility of a similar benefit being derived from experiments on human bodies?—If you could get human bodies to experiment on.

3059. I mean you do not see any reasonable means of getting at those facts through human bodies?—I do not.

3060. Do you think it would be possible to place before the Commission an accurate list of all similar benefits which have been derived from vivisection, from the earliest times to the present?—It would be a work of labour.

3061. Do you think it is possible to do it?—I think it is possible to do it, but it would be a work of considerable labour, more especially in reviewing the progress of science in late years.

3062. But you seem to have paid attention to that particular point; could you, not now, but hereafter lay before us some of the main discoveries that have been made by vivisection, and which could not have been made without experiments on animals?—I could do so if you gave me time to get the facts together, but it will take time.

3063. But indisputable facts?—I hold that these facts which I have laid before the Commission to-day are indisputable facts.

3064. You think that you could with time furnish the Commission with a list of similar experiments which could not have been made by any other means than through experiments on living animals?—I think so.

3065. (*Mr. Forster.*) With regard to these three cases of which you have given us an interesting account, take first the discovery of the circulation of the blood; at that time there were no anæsthetics; do you imagine that those experiments, if the discovery were now to be made, could or could not be conducted under anæsthetics?—I think that Harvey's experiments might have been conducted under anæsthetics.

3066. And now take the lacteal and lymphatic vessels, in that case could they?—Most certainly.

3067. The nervous experiments could not, I suppose?—In part, but not altogether.

3068. Then with reference to two out of the three altogether, and in regard to the last in part, anæsthetics could have been used?—Yes.

3069. (*Sir J. B. Karslake.*) How many years have you been engaged as lecturer on anatomy in Edinburgh?—I have been engaged for 21 years.

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3070. Have you a considerable number of pupils under your charge?—The largest class in Great Britain.

3071. About what would be the average of that class?—My numbers last winter were 468.

3072. Now, are many of those students also attending lectures on physiology?—About from one-third to one-half of those students.

3073. And they therefore have the advantage, if it be an advantage, of witnessing the demonstrations by the gentleman who was last before us, Professor Rutherford?—They have.

3074. Do you know many of these gentlemen privately, so as to know what their work is?—Some small proportion, but out of so large a number of students drawn from all parts of the world, only a small proportion have I any very intimate acquaintance with.

3075. From anything that you have heard about them, are you induced to believe that they practise vivisection in private?—I have no knowledge that they do.

3076. Have you any reason to think that they do?—It is possible that a student may occasionally, but certainly there is no practice of it, that I am quite clear about. I am not prepared to say that here and there a student may not do it, but certainly it is not the practice. On that I can speak, I think, without hesitation.

3077. Now passing from students, do you think that after they become medical practitioners themselves, they carry on the practice of vivisection to any extent?—Certainly not.

3078. Have they either the time, generally speaking, or the opportunity of doing it?—They have neither the time nor the opportunity, nor, I think I may say, the inclination either.

3079. As far as you have observed, is the tendency rather against vivisection on the part of those gentlemen than in favour of it?—I think that on the part of most men in this country there is a repugnance to experiment on a living animal. I have adduced evidence from the writings of Sir Charles Bell, and the writings of Dr. Jones, that both those gentlemen experimented on living animals unwillingly; and with regard to myself, when I have to do it for my own private researches, it is only resorted to when there is a feeling of its being of paramount importance.

3080. And as far as you have had any opportunity of observing, is that practice which you say you follow yourself followed by others?—Such is my belief.

3081. (*Mr. Huxley.*) I should like, following up the question that Lord Winmarleigh put to you just now, to inquire whether, in addition to the cases that you have put before us a great number of similar cases might not be produced?—They might undoubtedly.

3082. Suppose I take the several functions. Take digestion, for example, is it possible that our knowledge of that function could be what it is, without the knowledge which we derive from vivisection, and vivisection only?—Certainly not.

3083. Take secretion, and reminding you of the late experiments on the action of the nerves on the glands, let me ask you, could we have any conception of what that consists in without vivisection?—No conception.

3084. Turn to the important functions relating not only to the circulation but to the distribution of the blood over the body, one of the most important of all matters in regard both to healthy and diseased states, would it be possible for us to have any notion about them without vivisection?—The action of the vaso-motor nerves, which are the regulating nerves of the blood vessels, can only be got at by experiment.

3085. Then I will take a matter of importance alike to pathologists and physiologists, the whole doctrine of the inhibitory nerves, or those nerves the function of which is to arrest the action of the other parts?—That knowledge can only be got at by experiments.

3086. Indeed, I may say, may I not, with respect to the functions of the nervous system generally, that we should be absolutely in the dark respecting them, except for experiments on living animals?—Yes; anatomy had done all that it could do; experiment was absolutely essential to work out the function of the nervous system.

3087. To pass from the nervous system to organs such as the kidneys, what possible mode could there be of ascertaining whether the formation of urea takes place in the system generally, or in the kidneys, except by experiment?—I do not know of any.

3088. To take next the whole reproductive functions, may I ask you whether, from the first, our knowledge of the essential nature of that process in the higher animals is not entirely due to experiments such as those of Bischoff?—And the experiments of Harvey.

3089. It has been suggested before us frequently that this method of experimentation which is now practised is something newly introduced into physiological science, but I think that the cases which you have quoted have sufficiently refuted that statement?—I think so.

3090. Then as regards the undoubted fact that such experiments are more largely performed now than they were 30 years ago, is it not the case that that has arisen, not from the introduction of any new method, but simply from the greater thoroughness of teaching in physiology now as compared with the teaching of physiology 30 years ago?—Partly from the introduction of a greater thoroughness of teaching, and partly, I think I may also say, from a great scientific revival. The tendency of the present day is undoubtedly towards scientific advancement and progress, more men have given their minds to science, and consequently there are more workers in the field.

3091. Then as respects the use of experimental demonstration for teaching, it is very hard for those who have not been occupied in scientific research, to understand the great difference which there is between a person who is acquainted with a fact at first hand and a person who knows it only by hearsay; but it will be within your knowledge that in every branch of physical science it has been considered within the last 30 or 40 years to be of fundamental importance to give the student practical instruction?—That is a matter about which there can be no dubiety at all. The whole tendency of modern scientific teaching is to make the education practical.

3092. I suppose that if there is any one thing of which it might be hoped or expected *à priori* that you could teach it just as well by books and pictures as you can by practical work, it would be anatomy?—Yes, but I should attach no value to anatomical teaching that was not practical.

3093. That, however, is a clear case of mere structure?—Yes.

3094. And you can give exceedingly good pictures of a leg or an arm, and probably you can convey a better image of a thing without seeing it in anatomy than you can in anything else?—Yes, I think so.

3095. And yet I suppose you, as a professor of anatomy, would simply laugh to scorn the idea of making a man an anatomist without dissecting these parts practically?—Most certainly so.

3096. And it is the case with every branch of science, is it not?—I believe so.

3097. Your University of Edinburgh is at present making great sacrifices, and has been collecting large sums of money for the purpose of very largely extending its practical teaching?—Yes.

3098. So that, taken together with the development of laboratories in all parts of the Continent, shows that there is a complete and general consensus amongst those who are practically acquainted with this matter, that practical teaching is absolutely necessary?—It does.

3099. You, I think, are one of the editors of the Journal of Anatomy and Physiology?—Yes, I have been so from the commencement.

3100. So that you are, as a matter of business, obliged to keep up a knowledge of what is doing in

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Great Britain and elsewhere ?—Yes, it comes before me naturally.

3101. And I suppose you can tell us whether those countries in which practical research is the most systematic and the most thorough are not those which furnish you with the largest quantity of new discoveries ?—The greatest amount of new material comes from Germany, which is the country where practical instruction and training in scientific methods are probably most systematically and actively carried on at the present time.

3102. I think I understood you to say that you had a general objection, as a matter of principle, to legislation on the question which is before us, because you considered that it would be making a law for a particular class of so-called offences when precisely similar offences, it they be such, would be committed on a vastly larger scale, and would not in any way be affected ?—That is one of my difficulties; I think it would be class legislation of a most offensive and objectionable kind.

3103. If I am not greatly mistaken, that is a feeling which is shared very strongly by sundry of your colleagues and other persons of eminence in Edinburgh ?—I think so.

3104. Now it has been suggested that it is possible to control practices in physiological laboratories by some system of inspection, has it occurred to you to consider how that would work in practice; and I ask you, as knowing so thoroughly the real manner in which these things do work ?—I am familiar, and have been familiar ever since I became a teacher of anatomy in 1854, with the workings of an Act applicable to anatomists called the Anatomy Act. All anatomical teachers are subject to inspection—a government officer, an inspector of anatomy, is appointed by the Home Secretary, and all anatomical schools are inspected by this official under the Act of Parliament in question. But there is a difficulty, perhaps, in applying the same principle to physiological teaching, in this way, before the Anatomy Act the practice of anatomy was illegal, there were no legal means of obtaining bodies for dissection before the passing of that Act, excepting the bodies of those who were hanged; and the Anatomy Act, although called an Act for regulating the schools of anatomy, really in its operation is an Act for facilitating the practice and pursuit of anatomy, because the Anatomy Act provides us by legal means with the material which we have to examine in our anatomical work. If a system of inspection could be introduced which would not hinder the progress of physiological inquiry, or prevent a practitioner from acquiring knowledge for the benefit of his patient, I should see no difficulties in it, but if a measure of inspection were to be introduced which would hinder the progress of physiological inquiry and medical research, then on the grounds that I have stated in the earlier part of my evidence, I should have an objection to it.

3105. The duties of the inspector of anatomy are exceedingly simple, I think. I think that his business is simply to see that the bodies which are dissected in your theatre, for example, have been legitimately come by ?—That they have been legitimately come by.

3106. And when he has satisfied himself about that point, and that they are properly buried, he has nothing else to do ?—Nothing else.

3107. But supposing that it was part of the duty of the inspector of anatomy not only to see to these points, but also to see that you did your dissection properly, and that you did not employ clumsy people in making dissections, and to satisfy himself that you were not employing people in making useless dissections, do you think it would be possible that your teaching could go on ?—It would be quite impossible, because it would be making the inspector of anatomy, who probably would be a person without any special anatomical knowledge, a judge over experts.

3108. In the case of an inspector of physiological laboratories you have two alternatives, either he must be a person who knows something about it, a competent judge, or he must be a person who does not know anything about it, or knows very little about it. If he is a competent judge it has been suggested that he would in all probability be in sympathy with the scientific person who was in the laboratory, and that his inspection therefore would be of no good. If he is an incompetent person, on the other hand, however well educated a person he might be, I presume that the difficulties you have just referred to would infallibly arise, that is to say, he would be unable to comprehend the drift of your experiments, and he might very often report that you were doing barbarous and horrible things when you were doing that which was perfectly right and necessary. I am not putting that difficulty to you merely speculatively, but I am basing it upon what has actually occurred. I presume that you could not meet with a more respectable and in many ways excellent body of persons than the committee who manage the Society for the Prevention of Cruelty to Animals. I have every respect for them and their efforts. But a representation from that committee has come before us, containing statements respecting cruelty said to be perpetrated on animals. One of those statements upon which a formal assertion of cruelty, although it was excused, has been put forward was this, namely, that a lady teacher had taken a lobster to a class, and had cut it up before her pupils, the lobster being alive and moving about after it was cut. The whole evidence, as a matter of fact, was that the lobster in being cut moved its limbs. Is that evidence in your opinion that the lobster was sentient, or may it have made those movements as a purely reflex action ?—There is no evidence from a general statement of that kind that the lobster was a sentient creature, and in my opinion it was not, because we know well that movements will occur from reflex action where there is no consciousness either of the movement or of the application of the irritant which may have occasioned the movement.

3109. In other words, if the persons who reported upon that case had had any instruction in elementary physiology, they would not have dreamt for a moment of basing a charge of cruelty upon that fact ?—Most certainly they would not.

3110. Then I apprehend that, as a physiologist, you would not feel very happy if your laboratory were inspected even by persons of that degree of respectability and good faith, and it were left to them to report upon what they might think was wrong ?—I think it would be most dangerous.

3111. In fact, physiologists under these circumstances would probably be incessantly worried by charges which had no sort of real foundation ?—I think I would go so far as to say this (it may be, perhaps, a somewhat strong statement to make), that if physiological laboratories in this country were put under the inspection and supervision of persons of that kind we should become the laughing stock of scientific Europe; that is my strong conviction.

3112. (*Mr. Erichsen.*) To pursue the subject that Mr. Huxley has been inquiring into, in the event of a method of inspection being devised that would not be oppressive to physiologists, or that would not be a check to physiological science, do you think that there would be any objection to that? No such method has as yet, I believe, been devised or proposed ?—I should like to have an opportunity of seeing the proposed method before I answered the question.

3113. I think one of your objections to legislation was this, you stated that there was a difficulty in the definition of the words "living animal," and you illustrated that by the case of a frog whose head had been removed, and who still possessed the capability of automatic movements ?—Yes.

3114. But in that case is not the commencement of the experiment the removal of the brain or head, and up to that time the animal is certainly a living animal, and the experiment commences when the brain or head is removed, and everything subsequent is upon an animal in that condition ?—Undoubtedly, Mr. W. Turner, M.B.

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if the removal of the head of the frog were to be regarded as an essential preliminary to the performance of the experiment, the removal of the head would be the beginning of the experiment, but most persons would regard a frog as killed when its head was cut off, and that any experiment afterwards performed was on a dead, and not on a living animal.

3115. I think that with regard to this matter of inspection, or of licensing, or at all events of legislative interference in some way or other, there is one view that may be taken, and it is this, that as matters are at the present moment I suppose that any man can, if he has the time and the means, engage a room and pursue any kind of physiological inquiry that he pleases, provided that he keeps himself free from the operation of the Act for the prevention of cruelty to animals?—So far as I know it is so.

3116. And that any man might pursue a chimera. A man might, for instance, wish to ascertain the seat of the soul or the seat of life, (and I will give you in an instant an example of a very eminent man who wished to do so,) and he might immolate many thousands of animals in the pursuit of that chimera in trying to find the seat of the soul or the seat of life, and there is nothing to interfere with his doing so? Is not this the case at the present time. If I, for instance, as a medical man, chose to investigate the seat of the soul, or the seat of life, or had ideas about that, I might experiment on any number, on hundreds or thousands of animals that I chose to immolate in pursuit of that chimera?—Well, I am not sufficiently conversant with the exact working of the Act to answer that question.

3117. There was a very eminent man who is now dead, Dr. Stevens, who was a great surgeon; he first tied the internal Iliac artery, and he performed a great many physiological experiments upon the blood, and some experiments on Asiatic cholera. I was very well acquainted with him, and in the later years of his life he was seized with a great desire to ascertain the seat of life. I do not know that he experimented, but he wrote a great deal and talked a great deal about it, and he might have experimented to any extent. He had been an experimenter and a successful surgeon in early life, and might have gone on experimenting to any extent in pursuit of what most of his friends thought were mere idle fancies. So that, I think, there is that view with regard to the possible expediency of some kind of inspection?—Then who is to be the judge?

3118. That I know nothing of. Then I would take another point which you stated in reference to this very matter, namely, that a practitioner in the country who had a case of suspected poisoning might find it necessary to experiment on animals in order to ascertain what poison his patient was probably suffering from. We know that there have been cases of that kind which have now and then occurred. But is not that case analogous to the case of a country practitioner having to perform a surgical operation which he has never done, and who wishes to refresh his knowledge of the anatomy of the parts concerned in that operation; he is precluded by the Anatomy Act from taking a dead body into his house in order to do it, and he comes (as I know to my certain knowledge men have done) to a London school, or a central school, and there dissects the part on which he is afterwards going to operate. Will it not be possible for the medical practitioner in the majority of cases, at least in the cases of suspected poisoning, and making the necessary experiment in some central school that is legalized or licensed, to obtain in that way the information that he wished to acquire?—That is just a point that I have considered, and I think I can show that the cases are not strictly comparable. There are certain operations which have to be performed by the surgeon, and which must be done without delay, relying upon the knowledge which he possesses—he has no time to resort to schools of medicine, dissecting schools, and so on—I refer to such operations as the performance of tracheotomy in impending suffocation, and the performance of the operation for relief of strangulated hernia. I would cite those as comparable with the case I put just now, not such cases as excision of joints, where the operation may be put off for a time, and where the practitioner may revive his anatomical knowledge by going to a medical school to make a special dissection.

3119. But I am speaking of a case of chronic poisoning, because it is the cases in which you have suspected chronic poisoning that you make the experiments on animals?—This feeling, however, I think must act on the mind of any practitioner who is in attendance on a case of such a kind, that the sooner it is settled the better; that if there is some one in a house administering poison to a resident in that house the sooner the administration of that poison is checked the better. It should be done at once; and it is just in the case of a vegetable poison, such as strychnia, the detection of which in organic fluids by ordinary chemical analysis is so very difficult, that a rapid solution of the question is desirable.

3120. But still, as there would be many central schools licensed all over the country, in all of the large medical schools and most of the great towns, no medical practitioner would be many hours removed from one of those centres; and it seems to me that, practically, the difficulty would not be very great?—Undoubtedly what you have stated does to some extent remove the objection which I have raised, but I am not prepared to say that it altogether removes it.

3121. In addition to those cases which Professor Huxley brought forward, in which there was direct advantage obtained by physiological investigation, direct advantage to science, and indirectly, perhaps, an advantage to the treatment of disease, and so on, I would ask you, as it might be interesting to some members of the Commission, about two or three other cases that have a direct bearing upon the treatment of disease, whether surgical or medical. One, for instance, is the whole subject of asphyxia; the nature of that process, the gases that are respirable and irrespirable, and the influence of artificial respiration has been worked out, has it not, during the last century or more by a series of experiments on animals?—Yes, it has.

3122. And could not have been ascertained otherwise. For instance, is it not the case that it was generally believed that a person who was drowned was drowned by swallowing water, or by the water getting into his lungs and filling them up, until it was proved by experiment that such was not the case?—I believe that was the idea.

3123. And it would be impossible to subject human beings to the experiment of breathing irrespirable gases?—Yes.

3124. Those experiments must be made necessarily on the lower animals?—Yes.

3125. Then, again, if we take a subject which is of considerable importance in surgery, namely, the reproduction of the bone from the periosteum, the method of union of broken bones, and of bones that have been partially excised, and the method of the repair of tendons, all that has been, or at least a very great part of it has been, worked out by a series of experiments upon lower animals by physiologists and surgeons in France and a number of surgeons in this country?—Yes, certainly.

3126. Then, again, if we were to take that disease called diabetes, it was supposed, not many years ago, that the sugar was formed in the kidneys; it is now known by physiological experiment that the sugar may be produced by a lesion of the nervous system. Claude Bernard has shown, that if a certain portion of the brain is injured you get sugar in the urine; that the sugar has nothing more to do with the kidney, and is no more a kidney disease, in point of fact, than the purulent expectoration in a consumptive patient has to do with the mouth; that the kidney merely evolves it from the system just as the mouth ejects the purulent matter from the lungs?—That is the case.

3127. (*Mr. Hutton.*) You seem very much afraid of preventing any competent person from making these inquiries; but I thought you did not seem very much afraid of allowing incompetent persons to make them?—I think you can hardly have taken my answer correctly. I think I raised the objection who is to judge whether a person is competent or incompetent; there is my difficulty.

3128. That is precisely my point. Supposing that any system of restriction practically did prevent a few competent persons, but also a great many incompetent persons, from making these experiments, is not the loss on the one hand much more than balanced by the gain on the other?—That is a question which I am not prepared altogether to answer in the form in which you put it, because I hold that competent persons ought not to be restricted, and I should be sorry to see any measure introduced which would prevent competent persons from conducting experiments which they may conceive to be of physiological importance and of practical value.

3129. That comes, does it not, to what I said, that you are much more afraid of preventing competent persons than of not preventing incompetent persons?—I am not prepared to say that in this country experiments on living animals are carried on by incompetent persons.

3130. Now you spoke with very great confidence as to the fact that medical students in Edinburgh do not perform these experiments in their own private rooms?—I have no reason to believe that they do.

3131. We have had before us the statement of a gentleman signed by him, who probably was one of your own pupils, Mr. James B. Mills, M.R.C.S., who states that in his first session he saw a few of these experiments, and that in the following term, having become a senior, he was introduced to a great number of such vivisections, and on some occasions operated himself?—I have no knowledge of Mr. Mills at all.

3132. But do you not think that in all probability, if these things did happen at all, they would not come to your knowledge rather than that they would?—I think that if there were any abuse at all it would come to my knowledge.

3133. At all events, if this statement is true, here are a number of experiments which have happened of the kind which you would object to, and which did not come to your knowledge?—I am not prepared to admit anything about the statement, because I know nothing of the individual making it.

3134. I hope we shall have him before us, and shall know from him how far the statements can be confirmed. Then, again, you take a strong objection to putting any kind of restrictions on these experiments, on the ground that no such restrictions are put in the case of farmers and shepherds who perform these kinds of experiments on living animals. Is there not this great distinction between the two cases, that in their case it is obviously to the interest of the persons who perform those experiments not to do more than the necessary injury to the animal in question, while in the other case the interest is purely scientific, and the welfare of the animal itself is in no respect the interest of the operator?—I am afraid that you do not in that question altogether recognise the conditions under which a scientific experiment should be made; because it is most certainly to the interest of the experimenter that the animal should be injured as little as possible, just as much as it is to the farmer's interest that the animal should be injured as little as possible. One of the great conditions of experimenting on animals is that you should inflict as small an amount of pain and disturbance as possible; the greater the pain or the disturbance that you occasion to the functions of the animal the less your experiment is likely to answer; you must minimise the pain and minimise the disturbance.

3135. Surely such an answer as that has no application at all to such an experiment as poisoning an animal by strychnine for the purpose of demonstration to a class?—I do not think that I cited that experiment at all in connexion with class demonstration.

3136. There must be surely numberless experiments in which the experiment made is perfectly made whether the animal is more or less injured by the experiment. Of course, if it is an object to save the life of the animal, or if it is an object to prevent any disturbance to its system, except for the particular object of the experiment, what you say is perfectly true; but there may be plenty of experiments, may there not, in which that is not the object at all?—Many experiments undoubtedly result in the death of the creature; there is no question that many experiments must result in the death of the creature.

3137. And in those experiments there very often can be no identification, except such as humanity causes between the welfare of the animal and the object of the operator?—I must again repeat (because I think this is a matter of cardinal importance in connexion with this inquiry) that it is of the utmost importance in physiological experimentation that as little pain and as little disturbance of the function as possible should be occasioned to the creature, and that it should all be done as gently and as humanely as is possible, because, unless it is so done, so many disturbing elements are brought into the case that it is excessively difficult to interpret the experiment.

3138. On the other hand, it is often essential that there should be pain. For instance, your colleague, who has just preceded you, has shown that a variety of experiments could not have been tried without giving pain to the animal, and that he was not able to give anæsthetics on that ground?—Undoubtedly in those experiments which have for their object the sensibility of the nerves, pain must be given; but anyone with ordinary humane feelings would minimise that as much as possible.

3139. That is always assuming, as you seem to assume throughout, that the operator will be a man of these highly cultivated feelings and of the proper competence?—But I have no reason to think that men who practice experiments on living animals in this country are not humane men.

3140. But supposing that that could be shown, and a case of that kind could be made out, in that case you would give up your argument?—I do not know that I am called upon to consider a hypothetical case of that kind. I have no experience of that sort; my experience is the other way.

3141. Is it not true that the greater number of practitioners now living have learnt physiology without any practical teaching of the kind that is involved in painful experiments on living animals, and most of them without any experiments on living animals at all beyond those of course of the hospital and their ordinary private practice?—There is no doubt that physiological teaching up to within a comparatively recent period was not practical teaching; there is no doubt that it was merely teaching from lectures and books.

3142. Would you not say that a large number of the practising physicians and surgeons now living have had very little of this particular kind of teaching?—I think, as far as my knowledge goes, that there was very little practical teaching of physiology in the schools of medicine in this country until within a recent period. It is only indeed within a recent period that it has become strongly impressed upon the profession, and the various authorities in the profession, that practical teaching in physiology is required and is necessary. That is a matter which the medical authorities have only of late years satisfied themselves of and regulated for.

3143. But strong as your impression is that it is requisite, you would shrink from saying that all the eminent men who never have had that teaching are incompetent on that account?—But I think you will find, if you go into the histories of these eminent men, that although they may not have had teaching in practical physiology in the schools they taught it to themselves. If you look at the life of the late Sir Benjamin Brodie, or if you go back to the earlier years of Sir James Paget, and men of similar eminence in the profession, you will find that they were all practical experimenters in their younger years. In short, that has been undoubtedly one of the means by which men have risen to eminence in the medical profession.

3144. One, but not the only means ?—I say one of the means and a most important means. They have employed the earlier years of their life in experimental physiological inquiry, and on the knowledge acquired in their earlier years they have based their practice.

3145. But you would shrink from saying that no eminent men are now practising who have not obtained their knowledge in that way ? — It would require a more extensive acquaintance with the earlier life of men now in practice than I possess before I could say that; but I could mention many. I have told you of the late Sir Benjamin Brodie and Sir James Paget, and I will add Sir George Burrows; all to my knowledge men who have practised experiments and published their researches. But it should be remembered that those who may not themselves have practised physiological experiments have yet availed themselves in their methods of practice of the conclusions arrived at through the experiments of others. Perhaps you would allow me, in supplement to what I have just stated, to say that there is no question at all that many physiological experiments can be done without injury to living animals. A large number of experiments, for example, on chemical physiology, do not require living animals to be killed or injured.

3146. As to your impression that there would be a certain degradation to the profession in any kind of restriction being imposed, surely you would not say that there was any degradation to the millowners in the restriction that was put on the employment of children as factory hands in the mills ?—I do not think I used the word " degradation ;" I have no recollection of employing that expression.

3147. I think you employed some sort of equivalent for it ?—I have no recollection of using such an expression.

3148. In that case, then, did I misunderstand you as saying that the profession would feel injured as a whole by any restrictions ?—The expression I used was, that if such persons as were referred to in the question put to me (3111) were made inspectors of physiological laboratories we should become the laughing stock of scientific Europe.

3149. It was to a different part of your evidence I was referring. I understood you to say that the profession as a whole would feel injured by the assumption that any inspection was necessary ?—I have no recollection of saying anything of that sort. I do not think that I should be prepared to give any opinion as to what the view of the profession on the matter would be. I am merely giving my own opinion.

3150. And it is not your own opinion, then, that there would be any kind of slur cast upon medical men by that kind of inspection ?—I am not prepared to say that there would be a slur cast upon medical men by it. My argument was that I feared the progress of scientific discovery would be impeded.

3151. (*Chairman.*) I think I understood you to say that before giving any opinion about restrictions you would like to know definitely what those restrictions were ?—Yes.

3152. I think you said that there would be a difficulty on your part if there were any hindrances to the progress of scientific inquiry ?—That is my great difficulty.

3153. I think you said that you particularly wished to impress upon us that it was the interest of the scientific inquirer to avoid any disturbance of the animal subject to experiment, because it interfered with the success of his experiment ?—I said so.

3154. From that I daresay we may infer that, in your opinion, an experiment, to be useful to science, must be performed by the most competent persons ?—Certainly.

3155. And that if such experiments were performed by bunglers and incompetent people, not only would they be objectionable on the ground of inhumanity, but they would be comparatively misleading, and not tending to promote the progress of science ?—Certainly.

3156. Then with regard to the argument from the form I understood you to say that many of the scientific experiments were not more painful than other things that are done in other modes of life ?—Yes.

3157. But I do not understand you to found upon that any argument for an absence of humanity in the prosecution of scientific inquiry ?—Most certainly not.

3158. Nor if it be admitted to be necessary that certain things should be done upon a farm, do you found upon that any argument why bunglers should perform scientific experiments, or why eminent and most scientific men should perform unnecessarily, or with indifference to humanity, those that they do feel obliged to perform ?—Certainly not.

3159. Then with regard to the inspector of anatomy, I understand you to say that you feel no sense of class legislation in regard to that appointment, but that if a person not of the highest eminence in physiology were set to inspect and report upon the most eminent men in physiology you would think that to be injurious to those eminent men, and also injurious to the progress of science ?—I do not know whether I have sufficiently brought out what I intended, that there is to me at least a difference between the Anatomy Act and an arrangement as regards physiology such as seems to be indicated from what has transpired. The inspector of anatomy has nothing whatsoever to do with the methods or object of the dissections conducted in the localities which are licensed for the purpose; all that he has to do is to be satisfied that no subjects are received in the apartments licensed for purposes of dissection unless they have come through a regular official channel, and that when the anatomist has pursued his dissection to completion, the bodies are removed for decent interment. Those are all the duties that the inspector has to perform. An Act for the physiologist parallel to the Anatomy Act would provide and regulate a supply of frogs, rabbits, and other living animals to the schools of physiology, and see to their removal from the schools after being used, but would not take into account any experiment performed upon them when in the schools.

The witness withdrew.

Mr. J. C. Browne, M.D.

Mr. James Crichton Browne, M.D., called in and examined.

3160. (*Chairman.*) You are the head of the lunatic asylum at Wakefield ?—Yes; the West Riding Lunatic Asylum.

3161. Has your attention been directed particularly to the subject into which we are appointed to inquire ?—It has for ten years past.

3162. Do you yourself practice experiments on living animals ?—I have practised experiments, not what I should call vivisection, that is to say, I have never cut a living animal with the knife, nor operated upon one in any way, but I have experimented with drugs on living animals.

3163. What has been the nature of your experiments ?—My own experiments have consisted of two series, one undertaken last year, in which there was no destruction of life, to ascertain the actions of nitrite of amyl, and another undertaken last spring, with a view of testing the antagonism existing between certain drugs. There is a poisonous substance called pycrotoxine, the essential constituent of coçculus indicus, and I instituted a series of experiments to ascertain whether chloral was antagonistic to that substance, and I succeeded in proving that after a poisonous dose of pycrotoxine has been given to an animal if a dose of chloral be given subsequently the animal will recover.

3164. Have you tried that upon many animals?—In my whole series of experiments 46 were sacrificed.

3165. What kind of animals were they?—Rabbits and guinea pigs, one dog, and a few cats; chiefly rabbits and guinea pigs.

3166. Did it operate upon them all?—Upon them all.

3167. I suppose the occasion has never yet occurred of a human being being poisoned by this poison of which you speak?—Yes; there are several cases on record.

3168. But since that period there has been no case of a human being to your knowledge being poisoned by it, upon whom the supposed remedy has been tried?—No; cases of poisoning by pycrotoxine are of very rare occurrence.

3169. Now have any other persons practised experiments upon living animals in your laboratory?—Dr. Ferrier's first series of experiments was carried out there nearly three years ago.

3170. Were you present?—I was present at all of them.

3171. Were they of a very painful kind?—Not at all.

3172. Was there so much of a painful character about any of them that it was necessary to employ anæsthetics?—Anæsthetics were employed in every one from first to last. I generally administered the anæsthetic.

3173. (*Lord Winmarleigh.*) Would they have been painful experiments without anæsthetics?—Most undoubtedly.

3174. In every case?—In every case; the operation consisted in the removal of the top of the skull, and the exposure of the brain, and in the application of electricity to the convolutions of the brain; the movements that resulted when electricity was applied in this way being carefully noticed.

3175. What was the duration of the experiment?—The longest lasted about six or seven hours.

3176. During the whole of that time the animal lived?—Yes; the animal lived, being kept constantly under the influence of chloroform. There were four or five medical men present, and one of them always gave his individual attention to the administration of the chloroform or ether.

3177. Were the animals killed immediately?—Immediately, either by pithing or by a poisonous dose of the anæsthetic, except in three cases where the hemispheres had been removed, and where the animals were kept alive for subsequent examination.

3178. In such a case it was supposed that there could be no feeling?—Yes, it was believed so; and these animals were kept alive to see the results. I should like to say that in the whole series of experiments resulting, as I believe, in an important discovery, the sacrifice of animals was 29, and of these five animals died before they were touched or operated on in any way from the effects of the anæsthetic, showing that it was carefully and liberally given.

3179. Was it generally known in the neighbourhood of Wakefield, amongst the medical profession, that you were performing these experiments?—Yes; the results were published afterwards in the medical reports of the asylum, and papers descriptive of the experiments have been read before the British Association and elsewhere.

3180. What were the class of medical men that attended your lectures?—They were all qualified medical men, my own colleagues and assistants in the asylum, with one or two medical men living in the neighbourhood, one being a member of the Society of Friends.

3181. Were any of your pupils present?—There were some generally present, but they were all qualified medical practitioners.

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3182. These experiments then were not made with a view of instructing young men?—Not at all; it was with a view to discovery entirely. After the conclusion of the series of experiments, about six months afterwards, they were repeated upon two animals at a medical conversazione, attended by about 150 medical men, with the view of demonstrating the accuracy of Dr. Ferrier's statements, and of firmly establishing his novel and remarkable conclusions.

3182*a*. Do you believe that there are any other private laboratories except your own in which the same thing occurs? Are there experiments within your knowledge which have taken place in other parts of the country in such private laboratories?—I do not know of any such, except those performed in the great medical schools, the results of which have been published, for instance the vivisectional experiments of the Edinburgh Committee upon the antagonism of remedies.

3183. But you do not know of any other experiments outside the great medical schools, except your own?—I have not heard of any.

3184. (*Mr. Forster.*) With regard to those experiments there has been a controversy in the newspapers, has there not, between you and Mr. Jesse, the secretary of the society for the abolition of vivisection, and also between Dr. Ferrier and Mr. Jesse, as to how far there was chloroform given or not during the experiments?—I am not aware of any controversy; two or three letters passed, but we did not reply to Mr. Jesse beyond exposing his misrepresentations.

3185. I have had put into my hands (I believe it was sent to this Commission) a letter from Mr. Jesse in reply to a letter written by you to the "Times" on August 3d; his letter does not seem to have appeared in the "Times;" but it is a letter in which Mr. Jesse seems to wish to convey the impression that there could not have been complete chloroform administered; and you entirely deny that, as I now gather?—I do.

3186. You have not got the account of the experiment here in the publication of the Royal Society, have you?—I know nothing personally of Dr. Ferrier's work done for the Royal Society; I am only speaking of his first series of experiments performed at the West Riding Asylum.

3187. I think that this paper which has been sent to us alludes to the papers that were in the West Riding Lunatic Asylum Medical Reports?—I believe it does.

3188. If you turn to Mr. Jesse's letter, you will see that he states there that a cat "uttered long continued cries, screams, and gnawed its own legs, &c." Was that the case?—Quite so, that is quite correct.

3189. Will you explain how that was possible without its feeling pain?—The actions of the cat were mere mechanical movements performed during a state of profound unconsciousness. The animal, the top of the skull having been removed, was laid on a table; the electrodes were then applied to the convolutions of the brain; the animal would then move its paw as if striking a ball, open its mouth, twitch its ear, scream, or execute other actions according to the points touched; the moment the electrodes were removed from the brain all movement ceased. The animal was deeply narcotised by chloroform; the electrodes were again applied to the same spots, and the same movements were repeated; and so on with the whole series of movements—those of the tongue, arm, shoulder, &c. The cries of the animals under some of these experiments were no more significant of pain than are those cries which we frequently hear in the operating theatre when a patient is under chloroform.

3190. You have frequently given chloroform to human beings, I suppose?—Yes.

3191. Do you feel confident that in this case as much chloroform was given to deaden pain as you would have given in the case of a human being?—Yes, chloroform was given far more freely than it would be safe to do in surgical practice.

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3192. Mr. Jesse says at experiment 3, on another strong cat, these words, "the animal was "only partially narcotised;" is that so?—That is quite correct. Absolutely deep narcosis, when an enormous quantity of chloroform was given, would approach so nearly to death that no movement would be elicited whatever. I may say that we know that chloroform acts first upon the sensory nerves, and long before unconsciousness comes on the pain has disappeared, as must be known to anyone who has inhaled chloroform for the relief of neuralgia.

3193. Immediately after that Mr. Jesse says, "on "another page we find the words 'with and without "'chloroform.'" It would be an unfair impression, would it, to deduce from that statement that any of the experiments were without chloroform?—I believe that in one experiment Dr. Ferrier was doubtful about a movement, and it seems the animal was allowed partially to rally from the deep narcosis in which it was; but in all the experiments at Wakefield chloroform was given continuously, or at intervals as occasion required, except in those three animals which were allowed to recover and live for several days, and in them, of course, the chloroform was not continued after the operation was over. The animals experimented on were never permitted to recover so far as to be capable of feeling pain.

3194. They were in the same condition as a human being who has had chloroform who lives after it?—Yes.

3195. (*Lord Winmarleigh.*) What is the nature of the establishment at Wakefield where this goes on?—The West Riding Lunatic Asylum, a large hospital having upwards of 1,400 patients.

3196. Not having for its object physiological inquiries?—No; but one of the objects is of course the treatment of mental and nervous disease; and these studies were undertaken to throw light upon the pathology of the brain and of mental diseases.

3197. (*Mr. Huxley.*) Will you allow me to read this first passage to you:—"Dr. Ferrier's asser-"tion that ether or chloroform was administered "before and 'throughout all' his 'experiments' is, "we believe, invalidated by the details which accom-"pany it. If ether or chloroform was administered "throughout 'experiment 4,' on a full grown strong "cat, and (as Mr. J. Crichton Browne's credulity "permits him to affirm, though it is what he cannot "possibly know) the creature felt no more than a "pianoforte, how was it the animal gave evidence of 'astonishment, anger, rage, and pain? How was it "the creature uttered long continued cries, screamed, "and gnawed its own legs, &c.?—the last a strong "manifestation of agony." Now of course on the face of this it looks a very powerful statement; but let me ask you, have you ever seen any cases of what is commonly called mesmerism?—I have.

3198. You have seen a person put into the condition of mesmeric sleep by the well-known operation? —Yes.

3199. And I dare say that you (as I have done) have, while the person was in that condition, tested the amount of sensibility he had?—Yes, I have.

3200. I dare say that you (as I have done) have passed a pointed instrument into the skin of the hand, or inflicted a severe pinch in order to be quite certain that there was not the smallest perception of sensation in that person?—Yes, I have, and to my knowledge operations have been performed painlessly under mesmerism.

3201. Nevertheless if you have seen the same sort of manifestations that I have, and I would ask you it you have, a person in that state may be made to perform the most singular actions—as, for example, to sing, to express the depths of bodily pain, to express hilarity, and to do a vast number of other things. Have you seen phenomena of that kind?—Frequently; repeatedly.

3202. And in fact they are exceedingly well established, and very common?—Yes; the phenomena of mesmerism and hypnotism are familiar physiological facts.

3203. And should be known to all well-informed persons?—Yes.

3204. So that in such cases as these anybody might argue, just as strongly as Mr. Jesse has done here, that it is inconceivable that the person who exhibited these phenomena should not be feeling at the time that he exhibited them?—Quite so; the cases are quite parallel.

3205. And yet you and I know perfectly well that when that person is awaked out of his mesmeric sleep he is absolutely unaware that anything has taken place; and, so far as we have any power of knowing, he has been in exactly the same condition throughout as a person under chloroform?—Yes; exactly. He seems to himself to have passed through a placid sleep, and is unconscious of having moved or suffered.

3206. (*Mr. Forster.*) But is it not also supposed by some persons that there is this difference between mesmerism and the influence of chloroform, that what happens during mesmerism is absolutely and utterly forgotten afterwards, and that there may have been sensations which disappear from the remembrance of the patient?—The same has been asserted of chloroform, but of course it is insusceptible of proof.

3207. But has it not been asserted more strongly of mesmerism?—It has been asserted of both, I believe.

3208. (*Mr. Huxley.*) I will now put to you a question in reference to the action of chloroform itself. I suppose you have seen many persons operated upon under chloroform?—Great numbers.

3209. It will be within your knowledge that persons differ almost indefinitely in the effects which chloroform has upon them?—They do.

3210. Some for example will become completely stupefied and lie like logs, and make no manifestation when the most severe operation is performed?—Quite so.

3211. And from that there is every gradation through the most violent movements, as if of pain, occasionally with groans and screams; and yet, however diverse these outward manifestations may be, the people in all these cases of administration of chloroform, are insensible to pain?—That is so. Persons under the influence of chloroform will sometimes smile and even laugh immoderately while actually undergoing the most painful operations.

3212. So that in your judgment the argument that is used in the passage which I have just read has no validity to any person who is acquainted with the effects of chloroform?—None whatever.

3213. (*Mr. Forster.*) I think I understood you to say that this action which appeared to look like the action of pain, such as the cat gnawing its own leg, was a sort of physical action induced by touching a particular nerve during the operation?—Yes. Might I just say that when the electrodes were applied to this point these movements took place; afterwards a red hot wire applied to the same point resulted in no movement whatever, but the animal lay perfectly still.

3214. (*Mr. Huxley.*) Is it not the case that in these experiments you may cause an animal while in a state of chloroform by touching particular parts of the surface of the brain to perform associated motions quite as complicated as those of biting its foot, each of which is associated with the mere superficial irritation of a particular region of the brain?—Yes.

3215. In fact you may play upon the animal as if it were a machine?—Exactly, as if it were a pianoforte when anyone is playing upon its keys.

3216. That is not done by touching the particular portions of the brain which, so far as we know would give pain, but by sending a comparatively weak electric current through a part of its superficial layer?—Yes, a current which Dr. Ferrier invariably tested upon his own tongue before applying it to the brain of the animal.

3217. So that it is not to be supposed that these motions are the result of doing anything which would inflict pain on the animal if it were sensitive?—That is so. I may mention that I have frequently had

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cases of paralysis under my care where when you cut or pricked the foot it would be withdrawn as if with intense pain, and yet the patient would tell you he was not aware that his foot had been cut or touched. I should like also to add that these experiments on animals have been of great service to us already in enabling us to diagnose brain diseases; they have given us a new power of prediction as to them.

3218. (*Mr. Forster.*) They have been useful to you you mean, in your treatment of the insane at your Asylum?—Yes. I have been denounced for my own experiments, for the cruelty of searching by experiments on animals for an antagonist to pycrotoxine. There the operation consisted in the introduction under the skin of a little fluid by means of a perforated needle. It is an operation I have had performed on myself frequently; and of which the pain is infinitesmal. Then I was told that the operation was unobjectionable, but that violent convulsions were produced in the animal, and that these were painful and cruel. Now it so happens that this pycrotoxine is sold in large quantities as "Barber's poisoned wheat" for the destruction of birds, and numbers of animals die of it in convulsions every year, and I have never heard the sale of this wheat objected to or condemned. I may say also that convulsions themselves are not necessarily painful; although unconsciousness generally accompanies them, in a few cases consciousness remains, and in these cases we can ascertain the feelings experienced during the spasms. I had a patient lately under my care who during the fit used to say, "what a shocking bad fit I am having," and on asking her if she had pain she replied, "not any;" there was a feeling of wonder during the fit, and of muscular soreness and fatigue afterwards, but there was no pain in the convulsive movements themselves. We should desire, of course, to have protection in the performance of these experiments, because we feel that we are at present liable to annoyance by anyone who might choose to interfere with us.

3219. As you are the only holder of a private physiological laboratory I believe in this country, would you feel that any legislative inspection of that laboratory would be onerous to you, or in any way disagreeable or likely to interfere with the progress of science?—Not in the slightest degree. Inspection would, however, be impracticable, as there may be years together in which no experiments are going on. We keep for our own information a record of the animals destroyed.

3220. (*Lord Winmarleigh.*) You would not feel it any disgrace in any way to be under inspection?—No; inspection would be difficult, but we should wish to have some legal authority for what we might be doing.

3221. (*Mr. Erichsen.*) You would then feel that you were under the protection of the law?—We should feel that.

3222. (*Chairman.*) As far as you are concerned, I understand you to say that these experiments have been absolutely free from pain?—They have been free from pain.

3223. And that you have been subjected to the imputation that you were inflicting upon animals terrible pain?—Undoubtedly.

3224. And that a legal remedy which would secure you against being placed in what you consider an unjust position would be acceptable to you?—It would.

The witness withdrew.

Adjourned to to-morrow at 2 o'clock.

Wednesday, 27th October 1875.

PRESENT:

THE RIGHT HON. VISCOUNT CARDWELL, IN THE CHAIR.

The Right Hon. LORD WINMARLEIGH.	THOMAS HENRY HUXLEY, Esq.
The Right Hon. W. E. FORSTER, M.P.	JOHN ERIC ERICHSEN, Esq.
Sir J. B. KARSLAKE, M.P.	RICHARD HOLT HUTTON, Esq.

N. BAKER, Esq., Secretary.

Mr. DAVID FERRIER, M.D., called in and examined.

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3225. (*Chairman.*) I think you are professor of forensic medicine at King's College?—Yes.

3226. And also assistant physician at King's College Hospital?—Yes.

3227. We heard yesterday of some experiments which you had performed at Wakefield in regard to the examination of the various portions of the brain?—Yes, I made experiments at Wakefield on that subject.

3228. Were those experiments always performed under anæsthesia?—The method followed was to place the animal completely under chloroform, then to proceed to lay bare the brain by opening the skull, the animals being kept thoroughly under the influence of chloroform at the time, and during the whole of the subsequent operations, chloroform was administered intermittently in order to allay any movements indicative of pain on the part of the animal, or generally with the intention of preventing any suffering whatsoever.

3229. And do you believe that that intention was effectually carried out?—Thoroughly so.

3230. You have tried, I daresay, many other experiments?—Yes, I have performed other physiological experiments.

3231. Some of them painful in their nature?—Sometimes.

3232. But when they have been painful in their nature, have they always been under chloroform?—I think most of the experiments which I have performed have. I have some difficulty in recollecting all the experiments that I have performed for physiological purposes, but I have certainly always endeavoured to prevent unnecessary suffering in any animal upon which I did experiment.

3233. And may I take it that in the great majority of cases that endeavour has been successful?—I should say so, certainly.

3234. If I divide the subject of experiments into three parts, those which are properly called vivisectional, those which are upon the operation of poisons, and those which consist in the communication of disease to animals for the purpose of examining the progress and effects of the disease, dividing the subject into those three parts, and now asking you only about the first, is it necessary for any purpose of demonstration, in lectures, that the experiments should be otherwise than under complete anæsthesia?—Well, I think as to the great majority at least, of experiments for the purpose of teaching physiology, they may be

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performed without causing the slightest pain to the animal under the influence of chloroform, or some anæsthetic. Practically, all experiments for demonstrational purposes of that nature are performed under anæsthesia.

3235. Then we may take it that for the purpose of demonstration experiments that fall under the head of vivisection are painless, because performed under chloroform?—I was endeavouring to think of any that are performed without chloroform, but I am unable to think of a single one.

3236. With regard to the second head, those that concern the operation of poisons, those I presume cannot be done under chloroform?—No, because that would interfere with the action of the poison itself.

3237. Now, are those experiments generally of considerable pain?—The experiments are of very short duration, because for purposes of demonstration the poisons that are used generally kill the animal within a very few minutes; as, for instance, the experiments with charcoal vapour, prussic acid, and strychnia, and poisons of that sort, which cause very characteristic symptoms, and which it is, I think, essentially necessary that students should see, so that they may be able to recognise the effects which these poisons produce in man.

3238. But they are rarely then you think, if ever, of any extreme amount of pain?—I think very rarely indeed.

3239. But is it ever the case that, for the mere purpose of demonstration to students, extremely painful operations are performed with poisons?—I have never seen any, and I should not care to do any such experiments myself.

3240. Then we may take it that as far as your practice and judgment are concerned, under that second head, namely, that of the administration of poisons, no very painful operations in that need be performed?—Nothing further than the injection by means of a needle underneath the skin, or into a blood vessel, of the poison; that is the physical pain to which the animal is subjected before the effects of the poison are produced.

3241. And, as I understood you, the effects of the poison are rapid, and there is no protracted agony?—No protracted agony.

3242. (*Mr. Forster.*) For instance, in the case of prussic acid the effect would be almost instantaneous?—Almost instantaneous.

3243. (*Chairman.*) Now, with regard to the third head, experiments in the way of causing disease for the purpose of observing its processes and its effects, is that ever done for the purpose of demonstration?—No, the very nature of the experiment itself makes it unsuited for demonstration; because to establish a disease is a long continued process, and in a lecture one can only do what is capable of being seen in a comparatively short period. The induction of a disease requires observation from day to day, to follow it in all its phenomena.

3244. Then, speaking of demonstrations to students only, at present we may say that those experiments which belong to vivisection may be entirely done under anæsthesia, that those which concern poisons are rarely painful and never involve protracted agony, and that those which concern the communication of disease have no application at all to the subject of demonstration to students?—I think I quite agree with that mode of putting it.

3245. Now, with regard to original research, how would you express yourself upon that subject?—I should say, that wherever it is possible to avoid the infliction of pain on animals subjected to experiments the means should be adopted, either by chloroform or ether or opium, or other anæsthetic; but that where the administration of an anæsthetic would prejudice the object for which the experiment was conceived, then the experiment is still justifiable, notwithstanding the fact that it might inflict a certain amount of pain on the animal.

3246. I understand you to imply, though perhaps you scarcely stated it in that answer, that the utmost pains should be taken to diminish suffering where it cannot be entirely avoided?—Certainly.

3247. In the great majority of experiments for original research is complete anæsthesia possible?—I think so, in the great majority of cases.

3248. Of the other cases, is there a large proportion where the most painful part of the experiment can be done under anæsthesia?—Certainly, all the operative part; the laying bare, for instance, of the organ which it is necessary to reach for purposes of experimentation; all that can be done under the deepest anæsthesia, and I believe, as far as I know of experimental physiology, is done so.

3249. With regard to the general tone of sentiment towards the lower orders of animals on the part of the medical profession in this country, and of the medical pupils in this country, are you able to speak to that?—I have never observed in any of my friends who have experimented on living animals, or in students or others who have seen such experiments, any indication of indifference to suffering on the part of animals.

3250. (*Lord Winmarleigh.*) Have you observed the reverse?—I have observed the reverse. Some of my friends, who I believe have performed as many experiments on living animals as most men in this country, are the most humane kind-hearted men that I know. I can mention several. I am perfectly convinced that experimentation on living animals does not tend to produce a want of sympathy with suffering, or to harden a man's nature in any degree.

3251. (*Chairman.*) Now, have you been consulted at all about the propriety of legislative measures upon the subject?—I have talked over the matter with some of my friends.

3252. What would be your judgment upon the effect of any measures which might restrain the progress of physiological investigation?—I think any legislation that would retard physiological research would be a discredit to this country.

3253. Suppose that there were any opinion that such experiments were resorted to by persons who were not possessed of the highest qualifications for them, would it be, in your opinion, any interference with the progress of science if such persons as those were restricted from trying experiments?—Well, I should say that it would be so extremely difficult to ascertain or to determine who were qualified, or to frame a standard of competency to make original research, that I believe legislation in that direction would be injurious.

3254. But supposing that the effect of any measures that might be proposed were to exclude ignorant and incompetent persons, without interfering with such persons as those to whom you have lately referred, would any objection in your mind apply to that legislation?—No, if a standard of qualification could be framed, which, however, I think would be exceedingly difficult. Indeed, it seems to me that it would be impossible to frame a standard by which it would be possible to test the competency of any man to perform experiments for the purpose of original research.

3255. Admitting it to be difficult, supposing that difficulty were overcome, would your objection be removed?—Yes, I think so.

3256. Is the number of persons in this country who do perform such experiments for the promotion of physiological science a great number?—Very few indeed.

3257. They are very well known, are they not?—Yes, I think so.

3258. They are men of great eminence, great reputation, and deserving the confidence of the country on the score of humanity?—Yes, certainly.

3259. But if other persons were to perform such experiments, it would perhaps not advance much the cause of science?—But a person often attains to eminence by making an original research before he is recognised as an eminent man; so that that is the first indication of his possessing the qualifications to rise to fame.

3260. The performance of the experiments by an ignorant or incompetent, or bungling person, would not conduce, I presume, to the advancement of science ?—Unless he were capable of appreciating and interpreting the true significance of the phenomena which were produced by his experimentation science would not benefit.

3261. And, of course, experiments in the hands of such persons would be very detrimental to the sentiment of humanity ?—I think they might be.

3262. (*Lord Winmarleigh.*) I understand you to say that you have no objection to some restriction being put upon unqualified persons experimenting in this way ?—I have no objection to a restriction being put on unqualified persons, provided you establish a sound criterion of the ability to perform experiments.

3263. But I presume that, according to the law of this country, even at the present moment, a person experimenting in surgery or in medicine without having the proper qualification, would be subject to that law ; therefore, there could be no reasonable objection in this branch of science which is so connected with surgery and medicine. Or would there be any objection on your part to a severe restriction against the practice of the examination of living animals by persons until they have attained some recognised qualifications for that purpose ?—Do you mean a medical qualification ?

3264. No. I bring forward the medical profession as an instance that the laws of this country will not allow anybody to practice medicine or surgery who has not established his qualifications. Is there any objection to the same rule being applied to the examination of living animals for the purposes of physiology ?—There is one case in which I should say it would operate injuriously, in the case, for instance, of a student of psychology who has no medical qualification, and who may wish to determine some question, say with regard to the nervous system, having sufficiently studied phsyiology and psychology as to be able to appreciate the results he would obtain by experimental research ; and I think such an one ought not to be restricted by any form of legislation which would confer this power only on medical men.

3265. Without any restriction would you have it ? —Without any on a man of that ability, I should say.

3266. But how would you test his ability ?—That is just my difficulty.

3267. But, supposing the same rule were applied in medicine and surgery, a man might rise to eminence in medicine and surgery by practising on his own foundation, without being under the regulations established by law ; would not the same rule apply to physiology when you have to practise upon living animals ?—If it were possible for this hypothetical individual to establish his qualifications before men who were competent to judge them, if he could establish his competency before them, then I should say he ought to be allowed to act without restriction.

3268. But you say you find great difficulty in that, and, finding that difficulty, I recollect from your evidence that you would allow everybody to practise experiments without any restriction by law or otherwise, because you say that a man often gets to eminence by the investigations which he makes in private ?—I believe that the absence of legislation would create no practical difficulty. Theoretically, every man may perform vivisectional experiments ; practically, very few do.

3269. Do you think that the law which is now in existence for the prevention of cruelty to animals is contrary to reason ?—Certainly not.

3270. You think that a proper law ?—I quite agree with it.

3271. But, under the circumstances which you have just stated, anybody whatever might practise any cruelty to animals under the plea that he was making experiments in vivisection ?—Well, I suppose under the present act each case would have to be judged on its own merits.

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3272. Should you, as a practical physiologist, consider it any stigma upon yourself to have some limitation put upon the practice of these experiments ? Should you object to a law which stipulated that these experiments should be performed under some limitation, either of inspection, or in some other manner ?—I should certainly object to inspection of any kind in my experiments. I think it is possible that the method which has been suggested of giving a license to those who are qualified to perform experiments, a license simply, without any inspection or the necessity of keeping a record, would not interfere to any great extent with the development of original research, which, I think, ought to be encouraged in every way rather than restricted.

3273. You are, on the whole, of opinion, as I understand, that the practice of research by experiments on living animals should be open to the public without restriction ?—Well, I think, owing to the great agitation that has taken place in the public mind so recently, it is very desirable to distinctly dissociate experiments for the purposes of original research from cruelty to animals, or the law which punishes cruelty to animals. I think that the two are totally different, and that experiments for original research do not come in any shape or form under the provisions of the enactments against cruelty to animals ; but, as the popular mind has been so excited in regard to this subject, I think that some legislation, in which a clear distinction should be drawn between the two, would be advisable.

3274. (*Chairman.*) Your attention has been called to the evidence of the Secretary of the Royal Society for the Prevention of Cruelty to Animals ?—Yes.

3275. Have you any observation to make upon that evidence so far as it affects yourself ?—I am ignorant of what he alludes to, or of any expressions that I may have used which would justify his criticisms. I gave a lecture on the functions of the brain before the London Institution (I think that was the lecture he referred to), in which I described the effects produced by applying electricity to certain parts of the brains of the lower animals ; but I am not aware of having described them with any degree of levity, such as he charges me with ; and if, for instance, I stated that the application of the electricity to a certain part of the brain of a dog made him wag his tail, that may have appeared ludicrous, but I am unable to see how a statement of this fact can be construed into levity on my part. The monkey under the stimulus of electricity, makes in a state of unconsciousness a number of grimaces and movements of its arms and legs. These may appear very laughable to some persons, I myself cannot see anything laughable in them, and beyond that I am utterly unable to understand to what he refers.

3276. I think I may say that any charge of indifference to suffering was altogether disavowed by the witness ; but the committee of the society brought under our notice certain remarks, particularly upon the monkey, which you are said to have made at that lecture. Referring to the answer to question 1576, do you entirely disavow the statement that "the whole lecture place was really like a comic scene," and that three gentlemen were there who were members of the committee of the society for the prevention of cruelty, "and two of them left the room in disgust ;" do you know anything about that ?—I certainly do not. The effect I observed on the audience was an intense desire to listen to me as long as I would go on. I kept them half an hour or three quarters of an hour beyond the time I had stipulated ; some of them had to leave to catch their train, but the attention generally was most intense and continued during the whole time of the lecture. I saw nothing comic in the matter, nor did I see any levity on the part of the audience, and there was certainly none on my part.

3277. Do you entirely disavow any notion that animal suffering was the subject, either on your part

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or on the part of the audience, of anything approaching to a joke ?—Entirely.

3278. The animals that you were speaking of in your lecture were animals which had been the subject of a former experiment ?—Yes, of a former experiment, not of any experiment I was performing at the time.

3279. (*Mr. Forster.*) Were the audience informed that the experiments which you had conducted, and which you were describing to them, were conducted under anæsthetics ?—I think so ; I have always done so, so far as I recollect, in my lectures.

3280. Then you consider that it would be a most unfair impression that anybody could take away from your lecture that you were describing to anybody the sufferings of animals in a way that should induce them to laugh at them ?—I certainly think it would be a most unfair misrepresentation.

3281. (*Mr. Huxley.*) A question was put to the secretary of the society mentioned as follows,—at No. 1588, "Then the laughter which it induced may " have been from the inherent grotesqueness of the " thing, and not from any desire of Dr. Ferrier to " make it grotesque ?" And the answer was " Yes, " but the skill of the lecturer was used to cover the " grim character of the experiment by his humour." Have you any observations to offer upon that statement ?—It is a very extraordinary statement. The "character" of the experiment may have seemed "grim" to him, but I am not aware that I lectured with any degree of humour ; it is a sort of thing that I do not claim credit for, at least, in my lectures. I was trying to describe in plain language the simple results of my experiments.

3282. Although it is a trifling matter, and one that I am almost ashamed to trouble you about, may I ask as a matter of fact whether you yourself laughed during the description ?—I should say I did not.

3283. You had no desire to give what is called a sensational lecture ?—No.

3284. You had very important facts to put before your audience, and you put these facts before them to the best of your ability ?—That was all.

3285. (*Sir J. B. Karslake.*) Might I ask you, following that up, how long ago was this lecture delivered ?—Last spring ; I think in the month of March.

3286. Where was it delivered ?—At the London Institution, Finsbury Circus.

3287. And was it delivered before a mixed audience ? —Delivered before a popular audience, the subscribers to the institution.

3288. Amongst those there may have been many who were not medical men, or connected with the medical profession in any way ?—The great majority were not.

3289. If I understand you, you were describing the effects of electricity upon animals when in a state of anæsthesia ?—Yes.

3290. Did you speak from notes ?—I think not.

3291. Is there any journal which has been published to your knowledge which gives an outline of your lecture ?—I have never seen one. The lecture was essentially the same as that delivered before the British Association at Bradford, and delivered in the same manner.

3291*a*. Can you at all recollect the terms in which you described to the audience, the gesticulations or the grimaces of the monkeys and the dogs ?—The way in which I described them was simply this : I had diagrams before me, and with the pointer I would point to a certain part of the brain, and say, " When I apply the " electrodes to this part of the brain the animal will per- " form such and such an action ; for instance, it will " open its mouth, wag its tail, or stretch forward its " leg," and I described generally the actions caused by the stimulation of each individual part of the hemispheres.

3293. Now, whatever effect it may have produced upon the audience, were you conscious at all of having gesticulated or imitated the contortions of the monkey, so as to cause a laugh among the audience ?—I am not aware of it.

3294. Had you any intention of doing so ?—Certainly not.

3295. Had you any remonstrance from anybody about the lecture ?—Certainly not. This is the first time I have heard of any exception being taken to it.

3296. How long have you been a lecturer to classes ? —For the last five years.

3297. Have you used demonstration upon living animals in a state of anæsthesia, and sometimes when they were not in a state of anæsthesia, all that time ? —No, not all that time. In 1870 I lectured on physiology at the Middlesex Hospital, and I did not perform any experiments there ; but a year afterwards I went to King's College, to become demonstrator of practical physiology. It was then my duty to perform what are called the fundamental necessary operations and experiments, for the purpose of instruction of the students ; as, for instance, to show the laws of the physiology of the circulation ; but in all these cases we give anæsthetics.

3298. About what is the number of your pupils in the class ?—I lecture to a class varying from 40 to 50.

3299. May I ask you, in your judgment, is it essential for the due teaching of those students that they should see these demonstrational operations ?—I think some of them are absolutely essential for proper instruction. I am at present professor of forensic medicine, which includes the whole subject of toxicology, and in this I think experiments are absolutely necessary, in order that the students may recognise the characteristic symptoms produced by some of the more active, and most commonly used poisons, and also that they may see the methods by which we may counteract these, the methods of administering antidotes, and the method of restoring suspended animation in animals that have been asphyxiated by carbonic acid, or those gases that paralyse the respiratory function. All these, I think, are exceedingly important, and some of the effects are physiological tests for poisons themselves. For instance, there are some poisons, such as digitalis, which gives no very definite chemical reaction, but which has a very distinct effect upon the heart of the lower animals ; and that is a test by which it is distinguished. Such tests have been employed with the sanction of Government in prosecutions against poisoners, often establishing important evidence, which could hardly have been ascertained by any other method.

3300. Have you yourself been engaged in experiments which, as you say, have been sanctioned by Government as experiments on living animals for the purpose of ascertaining the effects of poison ?—I have not.

3301. Do you know of cases where scientific men have been so engaged ?—Certainly.

3302. And where they have had to make experiments on living animals for the purpose of ascertaining the effect of poisons with a view to bringing criminals to justice ?—Certainly.

3303. You have spoken particularly about these experiments which are made before a class, or some portion of a class, in reference to poisons. In your opinion, could the knowledge which is obtained by them from the exhibition of animals under the influence of these poisons be effectually obtained from reading or description ?—I think not ; it could not be effectually communicated by any oral instruction.

3304. You have stated your objections to legislation with reference to any licensing of persons who should make these experiments with a view to research in physiological subjects. In the medical profession, for physicians and surgeons, there are examinations in order to test their ability before they are permitted to practice ?—Yes.

3305. Is there any such examination at the present time in order to ascertain their knowledge of physiology ?—Not specially ; it is only part of the medical examination.

3306. Then it would be necessary to have some examination to ascertain the competency of any particular person, if the license in this case was to be

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founded upon any special knowledge?—It would require a special examination.

3307. (*Lord Winmarleigh.*) Does physiology form a part of the examination for medical men?—Yes, one of the most important parts of the examination.

3308. Universally?—Yes.

3309. (*Sir J. B. Karslake.*) It forms part of the examination, but the power of manipulating and carrying out these things by experiments on animals is not tested in any way, is it?—No.

3310. Then, for the purpose of granting a license, founded upon special knowledge of the mode of experimenting upon animals with a view to physiological research, there must be some examination and diploma which does not exist at the present time?—There must be, if it is considered necessary to license those who make such experiments.

3311. In your opinion, would a simple license to a person who wished to carry out this sort of research, founded upon the certificate of persons in whom trust might be placed, in any way interfere with the study of physiology?—I think it would be objectionable, but I think it is the best compromise in the case. I should not like it for my own part, but I would rather submit to it than that there should be any further interference with experimentation on living animals for purposes of research.

3312. (*Mr. Huxley.*) You, as a medical man, are doubtless acquainted with the state of the law regulating medical practice. Am I right or wrong in supposing that in the present state of the law there is nothing whatever to prevent a man from practising surgery or medicine, if he likes?—I think that is so.

3313. A document has been laid before us which is said to have been prepared under the sanction of the Society for the Prevention of Cruelty to Animals, and is addressed to us; it is a very formal document, and at the end of that document it is stated, as evidence of the "growing carelessness" with regard to the sufferings of animals in this country, that "popular "lectures were being given, of a sensational character, "by a learned professor, who made his audience laugh "over the grim behaviour of his unfortunate victims." Now, I suppose that if any person could have been shown to do that, no one would be more ready than yourself to say that his conduct was unworthy, not only as a man of science, but as a man, and wholly worthy of condemnation?—No one.

3314. From what you have heard, you will gather that the sole foundation for this official statement that has come before us is the allegation made by the secretary and others with regard to your own lecture; but I understand you to absolutely and utterly deny that there is even a shadow of foundation for that statement?—Entirely.

3315. Was the audience on the occasion on which you addressed it a large one?—It was an audience of 200 or 300 persons.

3316. Was the theatre of the institution full?—Pretty nearly.

3317. On occasions of that kind, when important lectures are delivered, it commonly happens that the managers of the institution are present?—They were present on that occasion; I gave two lectures, and they were present on both occasions.

3318. You are aware that the managers of the London Institution are gentlemen of high standing and character?—Certainly.

3319. And yet I gather from what you have said, that no one of them made any remark to you, even privately, upon the subject upon which you have just been questioned?—No one; they were extremely anxious that I should lecture again that very season, and they have asked me to lecture this year again.

3320. You have heard a plan suggested for inspecting physiological laboratories?—Yes.

3321. But I understand you to express a disapprobation of it?—A very great disapprobation of it.

3322. Will you state your reasons for your disapprobation?—In the first place, one would be prevented from performing experiments without having to write and give an intimation that such and such an experiment was to be done. Perhaps, after all, the experiment might turn out to be unsuccessful; and there would be so much trouble thrown in the way of doing anything that I believe most experimentors would rather refrain from work than be at the trouble of summoning the inspector on all occasions on which they wished to perform experiments.

3323. And I presume that as a man of science, knowing what investigation is, and being well aware that even among men of science, very few, except those who are engaged in a special inquiry, are really competent to judge of its value, you would view with some trepidation inspection even by competent men of science?—I should.

3324. And supposing that the other alternative which has been presented to us were adopted, and that the persons who were made inspectors were simply, I do not like to use the word laymen, but I mean persons not accomplished in physiological matters, do you think it likely that they would report on such cases with more fairness and understanding of what was going on than the official representative of the very important and useful body, the report of whose committee I have just read to you?—I should say they would be extremely incompetent to give a correct account of the results of experiments.

3325. In fact the sort of report which I have just read to you does not fill you with any great comfort at the prospect of what I may call lay inspection?—Not at all, exactly the reverse.

3326. (*Mr. Erichsen.*) Would you see any objection to places being licensed? We have spoken hitherto of inspectors, and inspection of actual experiments; but would you object to places being licensed in which experiments might be performed, say physiological laboratories, throughout the country, and merely subject to that general sort of supervision that the inspector of anatomy exercises over a dissecting room?—I should object. I should allow everybody liberty to perform experiments in his own private laboratory. A great many experimentors live in the country, and have no access to a public laboratory, and that would entirely prevent them from carrying on research.

3327. Do you think that there are many such persons?—Yes.

3328. And who are practising in their own laboratories, and unconnected with medical schools, do you mean?—I used to do so when I lived in the country, in Suffolk, at Bury St. Edmunds. I performed experiments there for my own purposes of research.

3329. I ask that question because hitherto we have not been able to hear of any such laboratories, or but very few. Do you know of any, or could you give the Commission the names of any gentlemen known to you who are practising in their own laboratories in the country?—The private laboratory at Wakefield is one, and it is one where a great deal of exceedingly good work is being done. I know that particularly well; and though I am not acquainted with any one at present who is carrying on physiological research in the country, because, of those who are doing so, the great majority are attached to schools, either in London or some of the provincial towns, still I may say that I prefer to do experiments for original research in my own laboratory.

3330. But your own laboratory is at King's College, I suppose?—No, at my own house.

3331. Then you experiment at your own house as well as at King's College, do you?—Yes; it would interfere with my professional work if I were obliged to go such a distance from home to perform my experiments.

3332. You have been connected for some time, I think, with King's College?—For two or three years.

3333. And you are consequently a colleague of Sir William Fergusson?—I am.

3334. Now, we have had it in evidence from him, that for the purposes of experiment, (and I will take the exact words, they occur in the answer to question

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1037,) that animals are "crucified sometimes for several days," and introduced several times during that period into a class for the purpose of observation?—I certainly never heard of such a thing.

3335. I would ask you, as one of the professors at King's College, whether such practices are ever, to your knowledge, carried on at King's College?—I know they are not.

3336. As a physiologist, and one, as we know, extremely well conversant with what is going on at the present day, more conversant at your age probably than older men often are, are you, to your knowledge, aware that such practices are carried on in any part of this country?—I should say they are not.

3337. Have you ever heard it?—I never heard of such a thing.

3338. That an animal is crucified and introduced several times in a state of crucifixion to a class for observation?—I am quite sure no such things ever occur in this country.

3339. Do you believe that any student would tolerate such an exhibition in this country?—I think not.

3340. We heard the other day from a very distinguished physiologist who has lectured for 20 years in London that he would not venture to exhibit a painful experiment without anæsthesia to his class, because it would be repugnant to the feelings of his class?—It would be so, decidedly.

3341. Is that your experience with regard to the students in London?—My experience with regard to students is this, that even when the animals are completely under anæsthetics, there is always a feeling of repugnance to experiments, similar to that which is felt to the first surgical operations.

3342. And that it would be more repugnant to them if it was without anæsthesia?—Certainly.

3343. And it would be extremely painful?—And I believe they would rebel.

3344. That a teacher, in your opinion, would not venture to submit such experiments to his class?—I should not do so myself, and I do not know of any other teacher who would.

3345. I received last night a paper headed "The Moral Aspects of Vivisection," written by Miss Frances Power Cobbe, and you are the first witness that I have had an opportunity of putting this question to. I should have preferred putting it to some one older, but as you have had considerable experience in two London schools you will be able to answer it. I find in this document the following statement: After speaking about students trooping out to see an animal killed, it goes on to say, "The "same keenness of observation or a memory of their "own youthful insensibility ought to teach all pro- "fessors of physiology that they are indulging a "maleficent tendency which already exists in their "pupils' disposition when they invite mere lads of "the Bob Sawyer type to watch their frightful "experiments, the more frightful, so much, alas! the "more attractive." Now, I would ask you whether, as a teacher of physiology in London, you find that, in the first place, the medical students as a body have "maleficent" tendencies?—Certainly not, from my experience.

3346. I would ask you whether it is not the fact that in most of the London medical schools there are young men's Christian Associations?—In many of them.

3347. Is there one at King's College?—I am not sure. I know that there are at Edinburgh.

3348. There are in London; and it being the fact that in London there are these young men's Christian Associations connected with the medical schools, is it also the fact that there is a united young men's Christian Association of the London medical schools?—I have heard of such.

3349. And do you think that the students who belong to that can be characterised as having "maleficent tendencies?"—I should say not.

3350. Is it not, in your opinion, a gross libel upon a class to stigmatise that class as having "maleficent tendencies?"—I certainly think it exceedingly libellous.

3351. Then, according to your experience, will you tell me whether experiments are more attractive the more frightful they are; I will quote the words, "the "more frightful, so much, alas! the more attractive"?—I have never seen any indication of such a thing; in fact I have never seen any frightful experiments of any kind.

3352. I was just going to ask whether any experiments of a frightful character are performed before students?—They are not, as far as my experience goes.

3353. Is it the fact that experiments for demonstrational purposes are constantly performed under anæsthesia?—They are performed under anæsthesia.

3354. Would it be prudent, in your opinion, for a teacher of physiology to perform a "frightful" experiment before his class?—It would not.

3355. The class, as you have already said, would probably rebel?—They would.

3356. And therefore you do not believe this statement, and that it is entirely inconsistent with everything that you know of medical students that "the more frightful the experiment the more attractive" it is?—It is certainly inconsistent with anything that I have seen of the character of medical students in any schools with which I have been connected.

3357. Either in London or in Edinburgh?—Either in London or in Edinburgh.

3358. (*Mr. Hutton.*) With regard to that matter, to which some reference has been made in Mr. Colam's evidence, about your lecture, I see this statement: "I have heard him say" (that is referring to yourself) "that the animals appeared to be in intense "suffering, and then joke about the stupidity of the "animal, especially if the animal happened to be a "monkey;" and further on I asked a question on that subject, and said, "May I ask you to explain "your statement that the lecturer joked about the "stupidity of the monkey. Those movements were "involuntary, were they not?—Of course they would "be," Mr. Colam replied, "because he showed that the animal had no volition at all." Do you recall the words about the "stupidity of the monkey" at all; that those words were used by you, or were capable of causing a laugh at the time?—I cannot think of anything that I said to which such a criticism would be applicable. Perhaps I may have stated the fact that certain lesions of the brain produce special effects on the intelligence.

3359. Which look as if they were stupid?—They may make the animal appear stupid, or produce a state of dementia, as I should call it; but I cannot recollect anything I said which could be construed into making such condition the subject of a joke, certainly not if the animal had exhibited the slightest suffering.

3360. I have been told that in the same lecture you stated that you were afraid to say on how many cats you had made the same experiment, and that that statement caused laughter in the audience. You have already said, I think, that you saw no laughter in the audience; but do you remember whether you did make that statement, that you were afraid to say on how many cats you had repeated the experiment?—I certainly should not be afraid to say it. I have no recollection of making any such statement. As far as I remember, when I described my experiments at the British Association at Bradford, I stated distinctly the number of experiments which I had made.

3361. How many were they?—About that time altogether I stated that I had experimented on about 100 animals, taking them all in all; and I do not think that I should be afraid to state at the London Institution what I stated publicly before the British Association.

3362. And you were not aware that any laughter was caused by that, or any other statement?—I observed none.

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3363. I have before me an article of yours on "Experimental Researches in Cerebral Physiology and "Pathology in the West Riding Lunatic Asylum "Medical Reports;" you say in the beginning of this article, "It may be mentioned here, once for all, that "before and throughout all the following experiments "æther or chloroform was administered;" but then there are many traces throughout the article that you yourself believed that the animal was not under anæsthesia in a great many of these experiments?—In regard to the administration of chloroform, I may remark that chloroform must be administered intermittently; the continuous administration of chloroform must inevitably cause death. Chloroform is being administered throughout the whole of the experimentation, but it is kept up in such a manner as not to endanger the life of the animal; sufficient chloroform is given, however, to keep it narcotised to such an extent as to abolish all consciousness of pain.

3364. However, I suppose you not only admit, but maintain, that in a great many of these experiments a great deal of pain was caused, owing to the interruption of the chloroform?—I think I saw no indications of the animal suffering pain.

3365. But you have expressed yourself so in this article as to many cases. At page 79, for instance, I see you say, "In order to determine whether the "combined movements were conditioned by the "voluntary impulse of the left hemisphere, I next "proceeded, two hours after the removal of the right "hemisphere, to expose the sigmoid gyrus of the "superior external convolution of the left hemisphere. Having ascertained by electrisation that I "could induce the usual movements of the right fore "leg by stimulation of its centres here situated, I cut "away the greater part of this gyrus, checking the "hæmorrhage with cotton wool steeped in perchloride of iron. After this the animal ceased to "struggle, and lay in whatever position it was placed. "Pinching the toes caused reflex movements in all "the four limbs, and at the same time the animal "barked energetically and howled when pinched. "Pinching of the tail especially caused the animal to "bark. This condition continued for several hours, "barking being always elicited and some reflex movements of the legs, but not to any great extent. The "barking may also have been a reflex phenomenon, "but from the fact that barking alone was sometimes "induced without any marked reflex movements of "the limbs, I was rather inclined to attribute the "phenomena to retention of consciousness and "distinct sense of pain. Ultimately (five hours after "the first operation) no barking was caused, but only "reflex of the limbs and trunk when the legs or tail "were pinched. The dog survived for eight hours "after the removal of the hemisphere." In that case clearly you did believe that the animal was suffering?—My last answer was with reference to the experiments on the electrical excitation of the brain, and that one which you have now read I had not in my mind; and even there I had very considerable difficulty in determining whether the animal was conscious or not.

3366. Still your opinion was that it was conscious?—That was the opinion to which I came in the end.

3367. Then at page 36 you say, "After 40 seconds "the right lip began to twitch; in 55 seconds, "twitching in the right forepaw; in 1 minute 10 "seconds, right lip twitching violently and jaws "working; in 1 minute 30 seconds, head drawn "over right shoulder; in 1 minute 46 seconds, the "fit stopped suddenly, and animal appeared to wake "up from a state of stupor." Then, I suppose, the pain began again, did it not?—No, certainly not; not even during that convulsion did the animal experience the slightest pain.

3368. Not even when it "woke up from a state of stupor?"—It falls into the epileptic coma, and you see a resuscitation after that, but not to pain.

3369. Then on page 38 I read, "The head was "immediately turned to the right, but it was doubtful "whether the movement was spasmodic or voluntary, "as the animal was only partially insensible." There, I suppose, there was a chance of pain?—It was sufficiently narcotised to abolish any real pain.

3370. That is hardly the impression that I think the language would give to anyone reading it for the first time?—I think that the phenomena known to all surgeons, which are manifested by human beings under chloroform, are quite consistent with these experiments being carried out altogether without any consciousness on the part of the animal.

3371. Here again, on page 42, it is stated, that "The animal exhibits signs of pain, screams, and "kicks with both hind legs, especially the left." Would you have used the expression "signs of pain" if you had simply meant reflex actions?—Yes, because I may stimulate the apparatus which gives the external manifestation of pain without inflicting pain on the animal.

3372. Here again, on page 44, I see this, "Restlessness, opening of the mouth, and long continued "cries, as if of rage or pain"?—There I put in the words "as if" because I did not believe that they were so in reality.

3373. That is why I assume that in the cases where you do not use that expression you did believe that pain existed?—If I did convey that impression it is certainly not the impression that I wished to convey in my remarks.

3374. However, in that first case that I read the account of, you did believe that the animal was suffering apparently?—Yes; that was an experiment which was performed in the only possible way of ascertaining the function of a certain part of the brain; and it is an experiment which does not require to be repeated when the fact is once established.

3375. (*Chairman.*) Is it the case that in severe surgical operations performed upon the human subject there are not unfrequently all the usual demonstrations of pain in cases in which, when the patient recovers, he or she says that they have experienced no pain?—That is a matter of every day occurrence, and quite familiar to all surgeons; and particularly it is the case in the administration of chloroform for obstetric purposes; in these, it is not carried to complete anæsthesia, but sufficient to annul any sense of pain; so that I have seen patients in this condition of anæsthesia manifest the signs of pain, screaming, groaning, and bodily contortions, who afterwards have told me that they have had a pleasant dream all the time, and never experienced the slightest pain during the whole period.

3376. (*Mr. Hutton.*) What particularly impressed me in that experiment of yours which I read first was a part of it which I did not read just now. At the beginning, page 78, you say, "It retained the power "of opening both eyes and of howling and barking in "a very vigorous manner. Apparently it remained "quite conscious, for, when called to, it would "struggle to get up, and would sometimes regain its "feet, and even succeed in walking a few steps, "when it would fall over in a helpless manner." That belongs to the first experiment which I read. Now did not that indicate complete consciousness so far as the experiment went?—It would indicate consciousness, but I think there is nothing there to indicate that the animal was in suffering.

3377. But if there was consciousness I suppose that anything which would ordinarily cause suffering would cause suffering then?—Yes, but I saw no indication then that the animal was in pain. We know that men may have part of the skull removed and yet not complain of the slightest pain.

3378. In experiment three on a strong cat you state that "the animal was only partially narcotised," and you speak of the experiment being "with and without chloroform?"—My attention has been called to that expression, "with and without chloroform;" and you will see, if you read the sentence, that it stands "with and without chloroform no effect was

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produced," no pain or indication of pain being manifested; and it was necessary, in reference to the question of the excitability of the cerebellum, to ascertain whether the absence of effect was due to anæsthesia or not; you will see that there is complete proof that the animal was not suffering in the slightest degree.

3379. Not this dog on which the experiment was tried?—This dog I believe was not suffering.

3380. Surely you express your view that it is, at the end, do you not?—I made an experiment there to ascertain if the animal was conscious.

3381. You also said you would incline yourself to the belief that there was a sense of pain?—I pinched its tail in order to ascertain whether it was conscious, just as I might pinch a man's ear; but when I obtain a proof of the consciousness I inflict no further pain upon the animal.

3382. All this is mere conjecture, is it not; it leads you to hope that it is not suffering, but you cannot tell whether it is suffering or not?—I think we can; I think we have distinct facts to show that in well known cases of injury to the brain.

3383. (*Mr. Forster.*) Did you administer chloroform with the intention of preventing these animals from suffering during these operations?—Certainly, always.

3384. You have been in the habit of administering chloroform to human patients?—Yes.

3385. Did you in these cases administer it in the same quantity, and in the same careful manner, as you would have done if you had been performing an operation on a human being?—Certainly, and my difficulty was to avoid giving the animal too much, for I frequently, in seeking to keep the animal unconscious, lost the animal, it died from an overdose.

3386. That means that you gave them more chloroform than you would have done with a human being, because with a human being you would have been afraid of death following?—Yes, and one requires to be exceedingly careful in its administration; and I have frequently had, in the middle of my operation, to resuscitate the animal from the effects of poisoning by chloroform, from administering the chloroform too continuously. It requires to be given with exceeding care, so that the respiration may not become paralysed.

3387. (*Mr. Hutton.*) As to the results of all these observations, is it not the fact that it is a question in dispute between you and the German physiologists, whether your results are correct?—I believe that all new doctrines are received with adverse criticism at first. I believe that the main results are accepted by all physiologists though there may be differences as regards interpretation.

3388. We were told not long ago that the functions of the brain are so much the same in the human beings and the lower animals that it is not necessary to verify, even for scientific purposes on human beings, results obtained on the brains of the lower animals. Is that your opinion or not?—I think before a result which is obtained by experimentation on the lower animals is to be accepted as applicable to human beings it ought to be determined whether it is in harmony with the facts of pathology, so far as we know them; but when the data of human pathology indicate the same facts as we obtain by experimentation on the lower animals, we are enabled to arrive at a rational explanation of them.

3389. I noticed that at the Edinburgh meeting lately very considerable doubts were thrown on your experiments, on the ground that they were not made upon beings near enough to man; I think it was M. Dupuy who expressed those doubts, was it not?—As far as I recollect it was by a man who has no standing at all, a practitioner in the country, not conversant with physiological subjects. It is a well known fact that the organization of the monkey's brain is almost identical with that of the human brain, and that what is true of the monkey is true of the human being.

3390. Is it not within your knowledge that the same experiment has been tried in America on a human being with the view of testing the applicability of these experiments to the human being?—I believe such an experiment was performed in the case of an Irish servant, who, from ulceration, had a large part of the brain exposed; and the American physician, thinking that irritation might be applied to the human brain with the same degree of safety as to the brains of the lower animals, applied electricity to ascertain whether similar movements would be induced in her as in monkeys; and he found that that was the case. The woman afterwards died, some said, from the result of his experiments; I believe, however, that the woman was in a very perilous condition at the time, and the post mortem examination showed that she had died from the results of the disease.

3391. I think the opinion was that he had hastened her death, was it not?—It may have been so; but without all the data we cannot pronounce definitively upon it.

3392. I mentioned that because you said that you entirely objected to anything which would restrict physiological research; of course you did not mean to include the rights of human beings in that statement; you would strongly object to physiological research on human beings?—Certainly.

3393. Do you not think that to a certain extent the sufferings of animals are considerable enough to be counted as deserving a certain proportion at least of the weight which you would attach to the sufferings of human beings; that there are in fact physiological results, which one would rather sacrifice than attain them by these exceedingly painful experiments; I gather you do not think that?—I think we are in need of every additional fact to advance the science of physiology, and physiology as a science can only be advanced by the accumulation of facts by different experimenters. Although the result obtained by any one individual may not appear to be of great importance, yet for the whole science it may prove of great importance, and it is only in this manner that science has reached its present state of development.

3394. The logic of that reply would lead you to make similar experiments on the human being; that you entirely disavow. I ask, do not you consider that the sufferings of animals are sufficiently important as compared with the sufferings of human beings, to make it often desirable rather to sacrifice a physiological truth than to inflict all these sufferings?—No, I do not see that the sufferings of animals are so very great as compared with human sufferings.

3395. I was particularly struck by your saying that you really would rather allow a psychologist, who might be a bad physiologist, to experiment in a way that might torture an animal extremely rather than sacrifice the results which he might gain as a psychologist by those experiments. Surely that is a sacrificing of animals altogether?—I did not suppose that a psychologist would be so ignorant of the subject as to go in a bungling way to work; because, so far as I know the psychologists of the present day, they are well acquainted with the practical results of physiology.

3396. One would be very sorry to trust a psychologist, who was not a good physiologist, with experimenting on one's own brain?—Well, I do not know any experiments on the human brain.

3397. He might, for instance, make, on the human brain, the experiments that you have made on the brains of animals?—I entirely disavow any experiments on human beings; and it is for the purpose of saving human beings suffering that we make experiments on lower animals.

3398. But you go further, and say that you would allow incompetent men to experiment on the lower animals, so long as they had psychological objects n view?—That is not my meaning.

3399. You objected to restriction, on the ground that it might prevent some psychologists from so experimenting?—I only said that I objected to re-

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striction, of such a kind as might interfere with men capable of research; and I thought that by taking the standard of medical qualification you would interfere with men capable of performing such experiments.

3400. But you would also interfere with men who were capable of giving a great deal of unnecessary pain?—Practically, we find so few people in the country who make experiments of this nature that I think we may trust the good sense and humanity of the great majority of mankind not to make experiments unless some good can be got by them.

3401. As to poisons, do you find it necessary to experiment for demonstration on almost all of the strongest poisons?—I do not experiment with all; but some of them are so characteristic in their effects, and it is so necessary that students should see them, that I make experiments with them. For instance, I make experiments with charcoal vapour, which sends the animals very quickly to sleep, and they die in a state of asphyxia; but there the condition of the animal after death is so characteristic that it cannot be described, it must be seen.

3402. That is not a painful experiment?—No. Then, too, I experiment with strychnia.

3403. That is a painful experiment, and it is one of the most painful deaths, is it not?—The convulsions are rapidly over. It is said to cause a considerable amount of pain during the convulsions; but we experiment chiefly on frogs with strychnia, and their consciousness of pain is not very highly developed I think.

3404. You do not experiment then on any mammals with that poison?—Yes, with rats and mice, and sometimes with birds, and sometimes with rabbits and guinea-pigs.

3405. You do not use more painful poisons than any of these in demonstrating experiments?—No, I only use those which rapidly kill the animal.

3406. (*Mr. Huxley.*) In some of your experiments, after describing your operation upon the anæsthetised animal, you go on to say that on a certain stimulus being applied the animal showed signs of pain. I apprehend from some of your answers that what you meant to say was that the animal exhibited the same kind of motion as it would exhibit if it were in pain? —Exactly, that is what I meant.

3407. But I apprehend, also, that it may be possible (I should like to know whether in your judgment it is possible) that in some of these cases, during the application of the stimulus, there actually was pain?—Well, I believe that there really was no pain.

3408. But supposing the extreme case which I now understand you to doubt, supposing that during the application of the stimulus itself, there was pain so long as the stimulus lasted, that must be by no means taken to imply that the animal was suffering pain before that strong stimulus was applied?—Certainly not.

3409. So that it would be a very erroneous conclusion to draw from your statement that when the animal was pinched it showed signs of pain, that therefore the animal was conscious of pain during the whole experiment?—Certainly it would.

3410. In fact the case is very parallel to that of a man who is in a dead slumber, whom you may rub gently in various parts of his body without disturbing him, but if you pinched that man, he would show pain, and feel it?—Yes.

3411. But it would be a strange conclusion to draw from that that he was suffering the whole time that he was asleep?—Yes.

3412. The two cases are parallel in your view?—Quite.

3413. With respect to the barking mentioned in one of your experiments, I would ask you whether the description which I am about to give of a very well known experiment is not correct: a frog is deprived of its cerebral hemispheres so as to be absolutely in the position of a machine incapable of suffering pain; and if under those circumstances, after the operation is recovered from, and the frog is perfectly well again, you take it up, and gently stroke it along the middle of the back, the frog gives a violent croak?—Yes; I know the experiment perfectly.

3414. Now that croak is exactly analogous to a dog's bark, is it not?—Yes, quite of the same nature.

3415. And you may treat that frog as you would a repeater; every time you stroke it on the back it croaks, and then it sinks back again into a state of absolute insensibility?—Yes.

3416. But inasmuch as that is not the least evidence of pain in the frog, it is quite clear that the corresponding action of the dog, which is a bark, cannot be taken in itself to be an evidence of pain?—Just so.

3417. A dog unconscious may bark just in the same way as a frog may croak?—Yes.

3418. So that you may have really very strong experimental evidence to ground your conclusion on that under the circumstances which you indicate, the dog did not really feel?—Yes, that the dog was quite insensible to any pain.

3419. So, again, even to take the most singularly combined actions, those actions which appear to show consciousness in the highest degree, inasmuch as they show rational adaptation of means to ends, I daresay you are familiar with the phenomena of mesmerism?—Yes, I have seen them.

3420. You may have possibly seen cases where a person being tested by yourself was absolutely insensible to physical pain?—Yes.

3421. Yet that person will answer a question rationally, and will do various and sundry complicated actions?—Yes, and surgical operations may be performed under that state without the individual suffering the slightest pain. It has been employed as a means for producing anæsthesia for surgical operations.

3422. I apprehend that you would say that the condition of the brain of a person in the mesmeric state doing these things, you have every reason to believe is of an analogous kind to the condition of the brain of the dog upon which you are performing your operation?—Yes.

3423. And as you are quite clear in the one case that there is no sensibility to pain, so you have reason to believe with moral certainty that it is so in the other case?—Yes.

3424. Then I observed that you answered with very great caution the question whether you would interfere with persons who were performing experiments which appeared to be unimportant, or trifling. I presume that one ground for your caution in answering was your knowledge, common to all of us who have followed the progress of science at all, that the observation of facts, and seemingly facts of the most insignificant character, has, in many cases, resulted in discoveries of the very greatest importance. I would merely mention, as an example, the very well known case of Galvani's observation of the frog's legs, which twitched when, under certain conditions, they were touched by different metals. Now, it is the case, is it not, that a very large proportion of our knowledge of the nerves and the physiology of the nervous system, has grown absolutely out of that observation, insignificant and trifling as it may seem? —Yes, the whole theory of animal electricity being founded on it.

3425. So that, with such facts as are before you, you would not unnaturally feel a very great hesitation in interfering with research, because to a man's contemporaries it may appear trifling?—Yes; I should say there would be great danger in subjecting any inquirer to the inspection of another person who was not able to enter into all the conditions of the problem which he had set before him.

3426. But I understand that, at the same time, you would view with the greatest moral abhorrence any man who should undertake a series of painful experiments, without being perfectly convinced in his own mind, whether rightly or wrongly, that they would bring forth good results?—I should.

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3427. (*Mr. Erichsen.*) With regard to consciousness under anæsthetics, have you observed in surgical operations that a patient will wake up from the anæsthetic and be perfectly conscious, and ask whether the operation has been done, and be incredulous that it has been performed, and remain in that condition of perfect consciousness for some minutes without feeling the slightest pain from the operation which has been performed ?—Frequently I have seen that.

3428. That is a common matter, of every day observation ?—It is.

3429. And it is a common matter also, in those cases in which a surgeon wishes to keep up chloroform for a period of 10 or 12 hours, say in the compression of an artery for aneurism, that the patient is allowed to recover, and the chloroform is only given intermittently, at lengthened periods, periods of 10 minutes or a quarter of an hour; that it is interrupted and given again ?—Yes; but sufficient completely to lull sensibility.

3430. Although the person is more or less conscious throughout ?—Yes, apparently conscious, but absolutely unconscious to pain.

3431. (*Chairman.*) Is it sometimes the case that a patient who has been under chloroform can describe what has been happening during the period of the operation, and yet will say that he has not suffered any pain ?—I have heard of such cases ; I think they are rare, but still I have heard of them.

3432. Is there anything further that you wish to say ?—No, I think not.

The witness withdrew.

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GEORGE HOGGAN, M.B., called in and examined.

3433. (*Chairman.*) I believe you were formerly in Her Majesty's Indian Navy ?—Yes.

3434. Have you paid particular attention to the subject which has been referred to us ?—Yes, I have paid particular attention to it. I have studied and acted as an assistant in a laboratory.

3435. I think you have taken a great interest in the subject from a desire to diminish the sufferings of animals ?—From a desire to diminish the sufferings of animals, purely.

3436. Now do you advocate the total abolition of the practice of trying experiments upon living animals ?—No, I am not prepared at present to advocate the total abolition of vivisection.

3437. Do you think then that it ought to be permitted under certain conditions ?—I do.

3438. Have you considered what those conditions ought to be ?—I have.

3439. Will you be so good as to state the result of your consideration ?—The conditions which I should like to see if it continued would be those in which all that was done might be made amenable to public opinion, so that all that was good in vivisection might be utilised, and all the abuses connected with it might be put a stop to.

3440. Have you considered in detail how those objects might be attained ?—I have ; I have considered it and discussed it with other men, and the result of what I thought would be best, and what many others who belong to the same party that I do, that is, not wishing to have total abolition but restriction, agreed with me in, I am prepared to state.

3441. Will you have the goodness to state to us in detail what you would recommend should be done ? —If vivisection is to be permitted under conditions for the prevention of abuses in connexion with it, it ought only to be allowed under circumstances where public opinion might be brought to bear upon it. With this intention the following suggestions are offered. First, all secret or private physiological experiments involving the infliction of pain or wounds upon animals to be absolutely forbidden by law under heavy penalties, involving imprisonment. Secondly, all physiological experiments necessitating wounding or infliction of pain upon animals to be conducted in a suitable hall fitted with the necessary tables and apparatus for practising vivisection. The area of the hall thus provided to be overlooked by a gallery, or galleries, into which the public could have unrestrained access by separate doors. This would offer the following advantages. (*a.*) All experimenters claiming to be humane in their practice of vivisection would thus enable the public to form a correct estimate of their pretensions. (*b.*) As the public would not be admitted into the area, there could be no inconvenience to experimenters from crowding round tables or acts of mistaken zeal or feeling. (*c.*) The public mind and conscience accused at present of exaggerations would thus be afforded an opportunity of calming and rectifying itself, if no cruelty or other abuses were to be witnessed. (*d.*) Any abuse or act of cruelty might be witnessed by the public and the guilty party prosecuted and punished at their instance. (*e.*) The animals on which experiments were being tried would be protected from cruelty by the presence of the public. (*f.*) Experimenters would be forced to be on their guard against allowing any cruelty or even the appearance of it. They would thus be rendered more thoughtful of the sentient being under experiment, and would therefore resort to vivisection only when such seemed necessary or advisable for the elucidation or proof of some important fact. (*g.*) Any person might have the power of experimenting there, without special license or qualification, under the charge of the superintendent of the institution, who, except on the proof of a conviction for cruelty, could refuse admission to none while any table or place remained vacant or unused. (*h.*) Any student of medicine wishing to make a speciality of physiology could acquire a useful knowledge of this science from the gallery, whence he could watch every step in the operation of preparing for and performing the experiment. This is an advantage which cannot be obtained in the best organised classes in the world, and is of primary importance. (*i.*) Any of the students seen in the gallery might be asked to assis. in the area at any experiment by any of the operators The number of such assistants to be limited (say to five), so as not to inconvenience neghbouring experimenters, or permit the place to be converted into a class room for witnessing demonstrations upon animals. This measure would benefit the three parties, as an extra assistant might enable the anæsthesia to be kept perfect, while by closer observance the student would learn better, and the operator would have his hands set free for his more special part. (*j.*) For a like reason, the number of persons in the gallery ought also to be limited, say to 10, of whom only five might be medical students ; all admission therein to be by tickets issued gratis by the superintendent to all respectable persons above the age of 18 who might apply for them, something like a reading ticket for the British Museum. (*k.*) For the benefit of the students, or the information of the visitor, a placard containing notes of the case to be hung in the gallery opposite the experiment to which it refers. The anæsthetic, the course of operation, and the design aimed at, to be fully stated therein by the operator. This card to be held as the card giving admission to the operator into the area, and to be retained and filed afterwards by the superintendent ; like the card case hung over a patient's bed in some hospitals. (*l.*) It has been objected that no discoveries in science could be prosecuted in such a place, as they would become known before the discoverer published them, and thus he would lose the coveted fame ; and that consequently such discoverers would be discouraged from using the institution. This is possibly the best argument that could

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be offered in favour of such a plan, as, while habitual or dilletante vivisectors would be discouraged, all earnest workers for the good of humanity would not hesitate to work openly in it at any experiment necessary to establish some important or beneficial truth. (*m.*) Animals on which it is intended to study the course of disease shall after innoculation, or operation in the hall, be retained in premises connected with and attached to the institution. These shall also be freely accessible to the public. No animal suffering from very painful wounds to be allowed to live, and if in the course of any induced disease the animal appears to suffer greatly, it shall be destroyed at once; and any neglect to do so shall constitute an act of cruelty for which the owner primarily,—failing whom the superintendent,—shall be held responsible. (*n.*) Any person or corporate body to be at liberty to form or build such an establishment, for which, after it has been inspected by an officer of Government, a license or permit gratis, or at a nominal fee, shall be issued, giving power to keep open the same under charge of a superintendent, who shall be held responsible for any irregularity or cruelty which may occur, along with but after the person committing the offence. He shall grant by the afore-mentioned card, admittance to use the area to any respectable person, and retain the same afterwards as a record of the operation. Admission to be granted on payment of certain stipulated fees for hourly, daily, or weekly use of the place. (*o.*) In this establishment all wounding of animals calculated to cause much pain under normal conditions to be effected only while the animal is fully under the influence of some anæsthetic or narcotic, and thereby rendered insensible to pain.

You will observe that this does not include the whole of an experiment, but only that portion in which pain is being inflicted by some preliminary operation.

(*p.*) Curari, or its supposed physiological analogues, is never to be used during wounding of, or upon wounded animals during any experiment. (*q.*) Lastly. Experiments should be strictly limited to the less sensitive and intelligent animals and it should be prohibited to vivisect horses, monkeys, dogs, and cats.

These are the general conditions that I have to propose. You will see that they aim principally at making vivisection as easy as possible when it is conducted in a proper way. While it prevents all secret vivisecting, it lays the path open and free for anyone to pursue vivisection under those proper restrictions. I recommend no special alteration in the law regarding Cruelty to Animals. That law could be applied there in each special case. In fact, it often would be very hard to say when an experiment was justifiable and when it was not. The circumstances of each special case would probably be the best guide upon which to rely. No hindrance is by this proposal placed in the way, in regard to license, or profession, or age above a certain point, but everything is done in order to render public experiments, with public opinion to bear upon them, as easy as possible.

3442. You mentioned having communicated on the subject with several other persons. Are we to take that which you have just put before us as representing your own individual opinion only, or as representing any society or body of persons?—It represents no society, but it represents the opinion of many who kept up this agitation for restriction before ever any society was instituted by writing and otherwise.

3443. Are you authorised to mention any names?—I am not authorised to do so; I have not asked the question. I have no doubt the parties would be quite willing.

3444. We are to consider you then as representing your individual opinion upon the subject?—You may.

3445. (*Lord Winmarleigh.*) Are the parties that you represent here, at whose suggestion you have come here, unanimous in the recommendation of those regulations, or have they been discussed by them?—They have been discussed by them.

3446. And are they the result of any resolutions passed by them?—They were first drafted out, read to different parties, their opinions got upon them, and alterations made where they wished it, until we seemed all agreed upon the point.

3447. And were those resolutions passed with the express view of your coming here to represent them?—Yes.

3448. Is that long since?—No, very lately.

3449. Since the appointment of this Commission?—Since the appointment of this Commission.

3450. May I ask if it is a large number of persons?—No, I have not made it an official communication.

3451. (*Chairman.*) Are you prepared to put in a list of any considerable number of persons who would adhere to this suggestion of yours?—I am.

3452. Will you have the goodness to do so?—I shall endeavour to do my best for that purpose.

3453. (*Mr. Forster.*) You said that you had been an assistant when experiments were performed as I understood you?—Yes.

3454. Will you describe to us what kind of experiments they were?—Yes, and I am prepared under a separate heading to state what I consider some of the abuses in vivisection; and I am prepared to illustrate by experiments which I myself have witnessed and assisted at. First, I consider one of the abuses of vivisection to be the repetition of fundamental experiments already classic, by experts, by way of showing their skill or showing an interesting point to a visitor in the laboratory, as the case may be. As an example of that sort I choose the action of the various nerves upon the secretion and the circulation of the sub-maxillary glands. The operation was first, at all events, best described by a professor whose name I do not know that I am liberty to mention.

3455. Is that experiment to which you allude an experiment mentioned in the 240th page of the first volume of a journal of physiology published under the direction of Dr. Brown-Séquard?—Yes, it is an experiment mentioned in that and the next page.

3456. (*Chairman.*) Have you witnessed the exhibition of this experiment yourself?—I have.

3457. Is it one which involves great suffering to the animal which is the subject of it?—It involves great suffering,, and it is impossible almost to give any narcotic in that case, because it would interfere with the course of the experiment.

3458. Now, have you seen this experiment performed in this country?—I have not.

3459. Do you know at all whether the practice in regard to experiments in this country is accompanied with the same pain to animals as elsewhere?—Certainly, there can be no difference. Vivisection is the same all the world over.

3460. Have you seen vivisection in this country?—I have seen a little; I have done some myself.

3461. Does it consist with your knowledge whether the experiments performed in this country are so severe as you say you have seen them on the Continent, and of which you have just given us an instance?—The course of the experiment must be exactly the same. Every step as it is described by a foreign experimentor must be followed step by step in any repetition of that experiment here; there must be no addition of narcotic or anæsthetic where such is not pointed out, or else the result will not be the same. The experiments are necessarily the same wherever they are performed.

3462. Would you have the kindness to tell me where you have seen experiments in this country?—I have done them myself. I have seen animals experimented on with strychnine in this country. What I have seen in a class has been very little. The thing is altered now, as I know from documentary evidence which I have here, from what it was at the physiological class of the University of Edinburgh five or six years ago. The only thing I saw there was on a frog. There is evidence from the professor himself to show that it is very different now.

3463. Can you put in evidence to show that severe

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experiments are performed in Edinburgh in a class, and without anæsthetics ?—No, I cannot say that; but I know that experiments are performed in Edinburgh without anæsthetics, and I have documentary evidence for that.

3464. Will you be so good as to produce it ?—This which I hold in my hand is an account which came into my hand only on Saturday from the British Medical Journal. My mere opinion upon these experiments is that they were very cruel, very painful, and as far as I can see they were useless, and not to be depended upon as far as application to man was concerned. Animals, namely dogs, were kept fasting, in the first place, for 18 or 19 hours, a thing that would never be attempted upon the human being upon which cholagogues were being administered. Curari was given, a substance the effect of which on the liver has not yet been examined thoroughly, but we know this, that in almost all glands it increases the secretion very much, and would throw matters into an abnormal condition. The animal has been kept under curari when there was no anæsthetic, no narcotic given; no narcotic, indeed, could be given, for there it would interfere, as a separate drug, with the experiment. Therefore, those animals, from the time that they were placed under curari, were kept under curari eight, seven, six, and five hours, suffering pain in consequence of an operation being performed which opened their abdomen, an operation made to find out the bile duct, and separate it from the other structures which lie with it in the gastro-hepatic omentum. A glass cannula is then tied in the bile duct, and the bile drops by means of a tube. All that human beings know is the pain there is when gall stones are passing down the bile duct, and that is known to give excessive torture. Merely a little bit of fat passing down gives us intense pain, and we can form an opinion that to take out the duct, to disturb all these parts, and manipulate it, as has been done, would cause more intense pain. And in that condition the animals were kept conscious and fully sensitive (I have any amount of evidence to prove this if there is any doubt about it), while the experiments were being tried upon them. I say that the conditions were abnormal to such a degree that they could never be applied to men; and that the pain was excessive; and that the experiments were uncalled for, and cruel in the extreme; and I put in a paper by Dr. Rutherford himself, in the British Medical Journal of October 23rd, as evidence of that point. This view of Dr. Rutherford's only forms another of the numerous opposing views on the same question; agreeing on one point only with the committee who sat in the same university, and the professor who was in the same chair before him a few years ago, (and under whom I received my tuition,) namely, that mercury had no effect on dogs. Nearly the whole medical profession agree that it has a great effect on human beings. So that the only point on which these people agree, after all their cruel experiments, is, that what is applicable to the dog is *not* applicable to man.

3465. (*Mr. Forster.*) I propose now to ask you with regard to the experiments in England that you have actually seen. Would you kindly tell me the experiment implying the greatest amount of suffering to any animal that you can recollect that you have ever seen in England ?—The only experiments that I can speak to in England (seeing that I went to France to acquire a full knowledge of vivisection, and that when I had seen it, I did not care about coming over here to practice it), were little things that as a student I did myself, and have seen others do. I have seen experiments for instance connected with strychnine.

3466. Will you kindly describe the sort of experiment ?—Simply this; a person thought he had discovered a cure for strychnine. It was a silly thing altogether, but he invited me to witness it. He had already experimented on a large number of dogs.

3467. Was this in a laboratory ?—No, it was not in a laboratory, it was in a private cellar, and not by a medical man; but he supposed that he had a knowledge of medicine, he had attended some of the classes. The strychnine was given, and the cure was to be to use the galvanic battery upon it. It was simply useless, the animal suffered greatly from convulsions caused by strychnine, and died as one would expect, and as many others had done before it.

3468. Upon how many animals was that experiment tried in your presence ?—It was tried on one in my presence, and another was lying dead.

3469. And how long would that animal be under the influence of strychnine before death ?—I did not exactly watch the time, but I suppose about a quarter of an hour or 20 minutes. The dose I may say was not very strong in order to give the antidote a chance of working, and therefore lengthened out the experiment.

3470. Can you give me any other case that you have witnessed ?—None except my own.

3471. Do you object to telling us what you have done yourself ?—No. I would simply say this, that I never got further than administering the anæsthetic. I have done that with three or four animals, and they died under my hands. I wished to save them all pain. I was strongly urged to try experiments by my teacher, and by others, on a subject that had already been published; I refused to do so, and tried quite by myself whether I could administer the anæsthetic and go through it. I found I could not do it, and therefore I refused to go further into the question until I had learned how vivisection was done.

3472. Am I to understand that you were advised by the lecturer, whose lectures you attended, to try experiments yourself ?—I was advised by nearly everyone of my own friends to do so, and the lecturer, my own teacher, offered to associate himself with me, and put his own rooms at my disposal for the purpose.

3473. (*Lord Winmarleigh.*) Was that in England or abroad ?—In Scotland.

3474. (*Mr. Forster.*) It was when you were studying medicine in Scotland ?—When I was studying medicine in Scotland. I was demonstrator of anatomy at the time.

3475. At what place ?—At Edinburgh.

3476. Were you recommended to try these experiments not merely as a student of medicine, but as being in the position of a demonstrator of anatomy ?—I was a student of medicine while I was demonstrator of anatomy; and this theory which I had devised, and which was hotly debated in Edinburgh for some time, was a new mechanical theory of respiration, upon which I wrote a dissertation, which was read before the Royal Medical Society there. That was a part of it.

3477. Then I would ask you this question, Was the advice that was given to you to try experiments advice that was given to you merely as a student of medicine like other students, or was it advice that was given to you in order that you should investigate the particular theory that you were advancing ?—It might have been either, it might have been both; that is to say, whether I was a practitioner or a student, if I devised a question that required proof, I should have been equally advised in either case to try experiments.

3478. Do you think that, judging from your own experience at Edinburgh, the lecturers generally advise the students to try experiments for themselves, independently of the experiments that were tried in the lecture room or in the laboratory ?—I certainly can speak for myself, and say that I was advised by many; by several of my teachers, by many medical men, also by all the students about me, who all said, "You "have laid it out beautifully in theory, but you "must show by experiments how it is done; you "must try experiments on animals, and publish your "theory along with those experiments."

3479. Do you think that a similar advice was given to other students ?—I have no doubt of it whatever, because several students have told me; one told me, who is now dead, that he had performed 10 or 15 experiments on cats shortly before then, and recommended me strongly, as what he had seen was so very clear, to get cats and dogs and examine the action of the various muscles in those animals that I had contended in my dissertation had not the use that was assigned to them.

3480. When this advice was given to you to try experiments, was it accompanied with any advice to subject the animals to as much anæsthesia as possible?—There was no question of anæsthesia at all.

3481. Do you imagine that in consequence of this kind of advice there was much private experimenting by the students?—I do.

3482. I think I must ask you what was the name of the professor who advised you to try these experiments?—Dr. Handyside, lecturer on anatomy.

3483. Is he a lecturer now?—Yes, and he offered to associate himself with me in the whole theory, and place his rooms at my disposal in order to elucidate the theory, or rather to illustrate it, by means of experiments.

3484. Then you give this as a fact that you desire us to take upon your own personal knowledge, that at the time that you were studying medicine at Edinburgh, it was common for the medical students to try private experiments, and that with the knowledge and concurrence and approval of the lecturer?—I do.

3485. What year was that?—About 1871–2, I think.

3486. Do you imagine that these private experiments were generally performed with anæsthetics when that could have been done without marring the success of the experiments?—No. My reason for thinking so was the fact of one gentleman now in practise advising me not to have cats, because they made such a squalling and scratching when you began to cut them.

3487. Was it a man senior to yourself?—He was senior to myself.

3488. Had he any official position in the university?—He was demonstrator of anatomy before me.

3489. I think I must ask you his name also?—Dr. Alexander Sinclair, now in practise in Edinburgh.

3490. (*Mr. Huxley.*) At the time that you speak of, when you were a student advised to perform these experiments, how long had you been a student?—I got advice continually from the time when I had been about only a year and a half a student up till the time that I finished there.

3491. I wish to refer particularly to the advice given to you by the responsible person whose name you have mentioned. You have stated that Dr. Handyside advised you to perform certain experiments. At what time in your career was that, how long had you been a student when he gave you that advice?—I may say three years. The advice was often given, but I give that as one term.

3492. I am now asking about this particular case, and will not trouble you to tell me anything about the others. Am I right in supposing that at the time that Dr. Handyside gave you this advice you had been a medical student for three years?—Yes.

3493. You were, I presume, then looking forward before long to take your degree?—Well, I cannot give a direct answer to that.

3494. Did you take your degree?—I did take my degree.

3495. In what year did you take it?—I took it in 1872.

3496. How long was that after you commenced your medical studies?—I commenced my medical studies in the session of 1868–9. You have asked if I was looking forward to taking any degree. All the time of my student life I had no specific looking forward of the kind. I merely went to medicine by way of filling up my time. I had retired from the service, and made medicine a sort of a pastime; so you will understand now why I was not looking forward to my degree. I did not care much whether I got it or not.

3497. May I take it then that you commenced your medical studies in 1868–9, and, as a matter of fact, did take your degree in 1872?—I did.

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3498. Then, I presume, that this advice given by Dr. Handyside was given to you shortly before the time of your taking your degree?—About a year before.

3499. Under these circumstances, you had passed through, I presume, at least two winters' instruction in anatomy. Is that not so?—Two winters' instruction in anatomy.

3500. You had also passed through two sessions, or, at any rate, one session of instruction in physiology?—One.

3501. You had also passed through your chemical courses?—Yes; one.

3502. And, I may say, that you had received all the preliminary instruction in medical science which it is customary to give in the university, that is to say, all that instruction upon abstract science as apart from medicine and surgery which is usual?—I had had no instruction in practical physiology.

3503. But you had received all the instruction that you were likely to get in anatomy and physiology, all that it was necessary for you to have in order to obtain your degree?—Yes.

3504. So that when this case is brought before us as an illustration of a student being urged to perform vivisections, when we come to look into the matter, the real fact is that that so-called student had at that time received all the instruction in anatomy and physiology that the university had to give him?—That is relating to this one occasion; but I told you there were several of the same kind from the same teachers.

3505. If you will allow me, I will take this specific case, because it affects the reputation of a gentleman in the university who has been named, and until that case has been disposed of it may be convenient not to deal with anything else. At the time this advice was given to you, so far from being a raw or inexperienced person, you had received all the instruction in anatomy and physiology that you were likely to get, and you had all the skill in anatomy and physiology that you were likely to possess. That is so, is it not?—On this one occasion I ought probably to correct that answer; I did not know whether that was all that I was likely to get, because I felt that I was incompetent at the time; and it was because I was incompetent that I refused to do it.

3506. You refused to do it because you felt that you were incompetent?—I did.

3507. May I ask you if it is not very much the custom at the University of Edinburgh for a student who takes his degree to present on the occasion of his taking his degree what is called a thesis?—The custom is the opposite; that is to say, it is not so much the custom as it is not the custom.

3508. Let me ask, does it frequently happen that students who are going to take their degree in the University of Edinburgh, present these on the occasion?—No, it does not.

3509. It does not frequently happen do you say?—It does not frequently happen I believe. A student leaves the university as a student for two years at least before he has to present a thesis, and it is supposed that during that time, after he has left as a student and taken his degree as a Bachelor of Medicine, he will employ himself in preparing a thesis for M.D.

3510. The degree of medicine to which I was referring was the Doctor of Medicine. I will put my question again more distinctly. It is commonly the case, is it not, that a student, before taking his degree as a Doctor of Medicine, presents a thesis?—The student of medicine does not take a degree of Doctor of Medicine now in the University of Edinburgh unless he began 10 or 12 years ago. The student of medicine in the University of Edinburgh first gets his M.B. degree, and remains outside the university, or at all events in practice for another two years before he can present himself with a thesis for M.D.

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3511. I think that this difficulty turns upon the technical sense of the word "student." Does a person who has taken his Bachelor of Medicine degree, and intends to proceed to be a Doctor of Medicine, in any proper sense of the word in any but a technical sense, cease to be a student?—He ceases to be a student in a technical sense; he ceases to be a student when he gets his M.B. degree.

3512. Then what you referred to just now was that he was not technically a student?—That he was not technically a student.

3513. In order to make my question accurate, will you allow me to ask whether it is not the custom and a frequent practice for a person proceeding for examination for his degree of Doctor of Medicine to present what is called a thesis?—No gentleman goes to examination for an M.D., there is no examination for M.D., all your medical examination is closed at M.B.; anything medical after that is confined to the presentation of a thesis, and it is for that purpose that you are supposed to remain two or three years after obtaining your M.B. degree.

3514. I am quite content with that. Let me put my question in another shape. Is it not usual or common before a person obtains his Doctor of Medicine degree for him to present a thesis?—Yes; it is imperative.

3515. Now, among the best students it is not uncommon for them, when they attain the condition of persons seeking to obtain a medical degree, to take for the subject of their thesis something requiring original research; that is so, is it not?—It is, unfortunately, true.

3516. But whether fortunate or unfortunate, it has for many years been the practice of the university, and it is considered a certain distinction, whether rightly or wrongly, on the part of the person proceeding for his medical degree, to present a thesis containing the results of some original research, that is so, is it not?—It is a custom for them to make researches; but I am not prepared to say that it is considered as an additional qualification that they should do so.

3517. I did not say an additional qualification; I said a distinction to have done so?—It is something to have done it; I would not say a distinction.

3518. I did not ask your opinion as to whether it was a distinction or not, but whether the authorities of the university considered it a distinction or not; whether, for example, they do not award gold medals for some of those theses; that is, within your knowledge?—Yes, it is within my knowledge that they award gold medals.

3519. Now, whether they do rightly or wrongly, I will ask you, is it not within your knowledge that the professors and teachers in the University of Edinburgh think it their duty to encourage any young man who shows a certain amount of scientific aptitude to carry out original research, and to embody that original scientific research in the thesis which he presents?—You put a question to me that I think ought to be put to the authorities of the University of Edinburgh. I cannot speak for those authorities.

3520. I ask you whether you know as a matter of common notoriety, having been in the University of Edinburgh yourself, and knowing a great deal about its affairs, that that is the case or not?—I do not pass any opinion upon that.

3521. You do not know it?—I do not pass an opinion upon it.

3522. You say that you know nothing one way or the other?—Simply that I have not been advised by any of the authorities to put in a thesis to the University of Edinburgh.

3523. And you do not know that that is the practice in the University of Edinburgh?—I know that the question of putting in a thesis is often spoken about with a sneer. I can only say that much; I could not speak for the authorities themselves.

3524. I then understand you to say that you do not know that the professors and teachers in the University of Edinburgh encourage young men to do this?—I have no special personal knowledge on that point.

3525. You are not aware that a professor in the university would think that he had failed in his duty if he did not encourage a young man who showed aptitude for original research to follow it?—I have not so very high an opinion of all the authorities in the university.

3526. I did not ask your opinion about the authorities in the university; I asked whether it was in your knowledge or not that they do a certain thing. You have been in the university, you have lived in Edinburgh, you have been a teacher in a school, which occupies itself in preparing persons for examination, and I ask you this simple question, whether you, having those opportunities of being conversant with the practice of the university, do or do not know that it is the habit of the professors of the university to encourage young men who show aptitude for original research?—No, I do not know it; I have never experienced it.

3527. You have never yourself you say been the subject of such a recommendation?—No; it is a question of favouritism in general that brings on young men in the university.

The witness withdrew.

Adjourned to to-morrow at 2 o'clock.

Thursday, 28th October 1875.

PRESENT:

THE RIGHT HON. VISCOUNT CARDWELL, IN THE CHAIR.

The Right Hon. LORD WINMARLEIGH.
The Right Hon. W. E. FORSTER, M.P.
Sir J. B. KARSLAKE, M.P.

JOHN ERIC ERICHSEN, Esq.
RICHARD HOLT HUTTON, Esq.
N. BAKER, Esq., Secretary.

Mr. E. Klein, M.D.

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Mr. EMANUEL KLEIN, M.D., called in and examined.

With respect to this evidence see Report, p. 1, and Appendix, Part II. § 2.

3528. (*Chairman.*) Are you Assistant Professor at the Laboratory of the Brown Institution?—Yes.

3529. Do you hold any other public appointment?—I am Lecturer on Histology at the Medical School of St. Bartholomew's Hospital.

3530. Are you the author of the first section of this book, which is known as a handbook for the physiological laboratory?—Yes.

3531. Now are any of the experiments which you describe in that book experiments of a painful character?—Many of them are.

3532. Necessarily so?—Necessarily so.

3533. What was the intention in your view of the book as regards the practice of those experiments; are there any limitations to that practice?—It is a

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guide for students working in a laboratory under the supervision of some person who understands and has to direct them how to do it; that they should not do it anyhow, but according to certain principles accepted in every laboratory.

3534. You have drawn in that book, I think, no distinction as to where anæsthetics are to be used, and where they are not?—No.

3535. Is that purposely on your part?—It is understood that the experiments mentioned there are done under anæsthetics, except where it is expressly mentioned that they are not so.

3536. But there is no general indication in that book; I think that such is the case?—No, I do not think there is.

3537. Do you think there ought to have been?—Well, as I said before, it is to be a guide in a laboratory, not for private persons or amateurs; and there exist in every laboratory certain rules from which no one is allowed to deviate.

3538. What is your own practice with regard to the use of anæsthetics in experiments that are otherwise painful?—Except for teaching purposes, for demonstration, I never use anæsthetics, where it is not necessary for convenience. If I demonstrate, I use anæsthetics. If I do experiments for my inquiries in pathological research, except for convenience sake, as for instance on dogs and cats, I do not use them. On frogs and the lower animals I never use them.

3539. When you say that you only use them for convenience sake, do you mean that you have no regard at all to the sufferings of the animals?—No regard at all.

3540. You are prepared to establish that as a principle which you approve?—I think that with regard to an experimenter, a man who conducts special research, and performs an experiment, he has no time, so to speak, for thinking what will the animal feel or suffer. His only purpose is to perform the experiment, to learn as much from it as possible, and to do it as quickly as possible.

3541. Then for your own purposes you disregard entirely the question of the suffering of the animal in performing a painful experiment?—I do.

3542. Why do you regard it then when it is for a demonstration?—Because I know that there is a great deal of feeling against it in this country, and when it is not necessary, one should not perhaps act against the opinion or the belief of certain individuals of the auditorium. One must take regard of the feelings and opinions of those people before whom one does the experiment.

3543. Then am I wrong in attributing to you that you separate yourself entirely from the feeling which you observe to prevail in this country in regard to humanity to animals?—I separate myself as an investigator from myself as a teacher.

3544. But in regard to your proceedings as an investigator, you are prepared to acknowledge that you hold as entirely indifferent the sufferings of the animal which is subjected to your investigation?—Yes.

3545. (*Lord Winmarleigh.*) Had you practised before coming to England?—Yes, in Vienna.

3546. Do you believe that that is a general practice on the Continent, to disregard altogether the feelings of the animals?—I believe so.

3547. Have you, since you have come to this country, had any proof of what you state now with regard to the different feeling that pervades the inhabitants of England with regard to the feelings of the animals on which you operate? Have you had any instances of the contrary feeling to that which you have just mentioned, on the part of Englishmen, since you have come to this country?—Yes, there is a great deal of difference.

3548. You have seen it exhibited?—Yes.

3549. Would you give the Commission an instance in which any such feeling has been exhibited?—I mean with regard to the journals; the outcry and agitation carried on in the different journals against the practice of vivisection. There is no such thing abroad; there the general public takes no view, does not claim to pronounce any criticism or any judgment about scientific teaching or physiology in general.

3550. But with regard to the pupils before whom you lecture, have you ever seen any indication on their part of a disinclination to inflict unnecessary pain on animals?—If I do perform a painful experiment where chloroform cannot be used, because it is against the purpose of the experiment, then I ask those before whom I do the experiment, whether they object to it.

3551. And what have you found?—If some of them do object then I do not do it before them.

3552. Have they objected?—Very seldom. With the exception of one or two it has never happened that they have objected.

3553. But you believe that generally speaking there is a very different feeling in England?—Not amongst the physiologists; I do not think there is.

3554. But amongst the people of England do you think there is a very different feeling from what exists upon the Continent on this subject?—Yes, I think so.

3555. Seeing that there is that feeling, have you found that in yielding to that feeling in your lectures your experiments have not been so effective as they would have been if you had acted without anæsthetics?—Well, really I could not say.

3556. How long have you practised in England?—Four years and a half now.

3557. Always in London?—Always in London.

3558. You have never given lectures in the country?—No. I never do experiments in my lectures; I only do experiments for teaching purposes in my private class.

3559. To demonstrate?—To demonstrate.

3560. Could you tell the Commission any particular experiment in which you think it is absolutely necessary to act in demonstration without anæsthetics?—Supposing, for instance, one intends to bleed a young dog to death by opening a vessel. With young dogs it is almost impossible to produce narcosis quickly, it takes always a considerable time; it is much sooner done by an assistant fixing the dog; at one cut the carotid may be exposed, and the vessel may be opened in less than half a minute, whereas with chloroform it would take a much longer time; the animal would howl, and would, I have no doubt, suffer a great deal more than when it was done without anæsthetics.

3561. Would the dog howl and exhibit pain while under anæsthetics?—While being chloroformed it would.

3562. Could you explain more in detail why you think it necessary, besides the point of time, to abstain from the use of anæsthetics in cases of research?—I fancy the whole attention of the investigator is drawn to the purpose of the experiment. He has made clear to himself what he is going to do, how he is going to do it, and what he is going to learn by it. He generally chloroforms a dog when he experiments on a dog for convenience sake, in order not to be disturbed by the howling and the resistance; and so with cats. He does not do it with frogs. I do not think we have any right to regard the sensibility and feeling of a frog as being of a very high degree. And just as little as a sportsman or a cook goes inquiring into the detail of the whole business while the sportsman is hunting or the cook putting a lobster into boiling water, just as little as one may expect these persons to go inquiring into the detail of the feeling of the animal, just as little can the physiologist or the investigator be expected to devote time and thought to inquiring what this animal will feel while he is doing the experiment. His whole attention is only directed to the making the experiment, how to do it quickly, and to learn the most that he can from it.

3563. But do you think that where it is only a question of time a professor of physiology is not bound to consult humanitarian feelings?—I must again draw a distinction between an investigator and a professor

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of physiology. I understand a professor of physiology is a man who teaches, and there I think it is quite right before a class that when one performs an experiment one should use anæsthetics, but an investigator has no time. I myself, when I am going to make an experiment for pathological research, have no time really with regard what the animal will feel.

3564. Is that really the only reason that you can give for not using anæsthetics?—It is to a great extent; it is the chief reason I should say; there is no place for considering that point.

3565. (*Mr. Forster.*) You mentioned, I think, that there were certain rules in laboratories; what are those rules?—That no one is allowed to make an experiment except a professor or the assistant professor is present and tells him to do it, and makes him acquainted with the purpose of the experiment, doing it perhaps the first time before the man who is going to repeat it.

3566. Do those rules in the English laboratories say anything about anæsthetics?—Well, generally it is understood, where it is not against the purpose of the experiment, that they are used; it is a thing understood by itself.

3567. Is that a written rule?—There is no written rule that I know of any kind.

3568. Have you found much progress in physiological science lately in this country?—I think so.

3569. You find more attention given to it than when you first came?—Yes, considerably more.

3570. And several students, I suppose, that attend your lectures take great interest in it?—Yes.

3571. Do you think that many of them are carrying on private investigations?—No; I do not believe that any private person carries on investigations except in a laboratory.

3572. Do they come to your laboratory and try experiments?—Yes; those who carry on private experiments in connexion with the Brown Institution all come to the laboratory, and do it under me or Dr. Sanderson.

3573. What is your connexion with the Brown Institution?—I have nothing to do with it, but I am only in the laboratory, which is part of the Brown Institution. My official position is with Mr. Simon.

3574. Your laboratory is in the rooms connected with the Brown Institution?—Yes, on the same ground.

3575. How do you get the animals that are practised upon in the Brown Institution?—We buy them.

3576. And do you buy them with funds that come out of the Brown Institution?—No; those used for pathological researches are not brought from the Brown Institution. These researches that I am conducting myself are for the medical officer of the Privy Council, and the expenses are defrayed by the medical officer.

3577. (*Chairman.*) By what medical officer?—Of the Privy Council, Mr. Simon; the expenses of the pathological experiments I refer to. The experiments have been very few in the last few years for special research, but those that have been made have been always in connexion with scientific investigation carried out for Mr. Simon.

3578. (*Mr. Forster.*) And have the animals upon which you have been experimenting in your researches and investigations been furnished by Mr. Simon?—I am speaking of those. Those for teaching purposes are bought quite privately.

3579. I want to know with regard to your own private investigations, are the animals that are obtained for that purpose, and upon which you try experiments, obtained with the assistance of Mr. Simon's grant out of the Privy Council?—I do not do any private experiments except for pathological researches for Mr. Simon.

3580. Have you done any other?—No.

3581. What are the experiments in pathological researches?—For instance, inducing artificial tuberculosis, or for instance, trying to communicate typhoid fever to the lower animals, or for instance, in the experiments on small-pox in sheep, or pyemia.

3582. You spoke about your own personal experience in what may be strictly called vivisection; that, I suppose, is not so much in this country as abroad?—To a great extent.

3583. Can you at all recollect what was the vivisectional experiment in this country with which you had to do which caused the greatest pain without anæsthetics?—I should think cauterising the cornea of a frog, for instance; it is mentioned at page 38 of the handbook.

3584. Would you be good enough to tell me the time that that experiment would last from the period at which you would first be obliged to give pain to the frog?—It greatly depends upon what kind of frog it is. If it is a large vigorous frog it takes a much shorter time than a small frog, for instance, a large frog from the Continent.

3585. This is not the frog which you get here, I suppose?—We get them from Holland.

3586. (*Chairman.*) You import them for the purpose?—Yes.

3587. (*Mr. Forster.*) Take one of those frogs, then, and tell me how long the experiment would last?—At present I do not practise it so long as the passage in the book would imply; I do not allow it stay an hour for the cauterisation. When doing it on living animals it takes say 10 or 20 minutes after cauterisation, the cauterisation itself takes little time.

3588. And what becomes of the frog after that time?—It is killed.

3589. (*Lord Winmarleigh.*) Always?—Generally killed; always killed.

3590. (*Mr. Forster.*) Have you performed any experiments in England upon dogs or cats; upon dogs, for instance?—Yes, I have.

3591. What sort of experiments?—I have injected pyemic fluids into the veins of dogs to make them pyemic, to produce pyemia, or septicæmia. So with cats. Then I have used dogs for producing tuberculosis, and I have used dogs and cats for producing typhoid fever.

3592. Have you cut up any dogs or cats by vivisection?—If I inject into the vein, I must cut; you cannot do it without cutting.

3593. Those are what you call pathological experiments?—Yes, for pathological purposes.

3594. I am now speaking of the other kind of experiments on living animals, which would be more strictly perhaps called vivisection; have you done them to dogs or cats in England?—No, not for the special purpose of demonstration.

3595. But have you for private investigation?—As I mentioned just now for experiments on pyemia, typhoid fever, septicæmia, and tuberculosis; not for others.

3596. Have you, for private investigation, tried any experiments on dogs or cats, not pathological, but physiological experiments?—No, not directly for physiological experiments. I may have used a dog which has been used at the same time for pathological purposes, also for physiological.

3597. (*Chairman.*) When you use a dog for physiological purposes, do you adopt any particular mode of fastening that dog?—No, it is fastened on a large board, the four limbs are fastened.

3598. And how are they fastened?—With broad bands.

3599. (*Mr. Forster.*) You were stating that you considered it was so important to be absorbed upon the object of the experiment, that you could think of nothing else, and therefore could not really, in your opinion, afford time to consider the feelings of the animal; do you not sometimes find an inconvenient interruption from the cries of the animal?—Only then I do use chloroform, that is what I said; I use anæsthetics for convenience sake.

3600. Do you ever use curari for that purpose, to stop the cries?—Curari involves other difficulties, the animal ceases to breathe because it paralyses the movement for breathing, and that might be against the object of the experiment. The animal, for instance, is to be kept alive after the experiment was over for some time; that could not be done after artificial respiration has been used.

3601. But practically, has not the howling of the dogs interfered with experiments?—Dogs do howl also when you chloroform them.

3602. Do you try experiments with any animals that do not signify pain so loudly?—Rabbits.

3603. They do not howl, I suppose?—They do not.

3604. Then of course the same motive would not induce you to use chloroform in their case?—No.

3605. In fact, I suppose with rabbits you would not use chloroform?—I use choral hydrate; but, as a general rule, for my scientific investigations, I do not use chloroform, or any other anæsthetic, except for convenience sake, in dogs and cats, and for no other animals as a general rule. There may be exceptions perhaps, but as a general rule, I think I am safe in saying I do not use it.

3606. You gave it as your opinion, that your views on the subject, although not shared by the British public generally, were the views of the British physiologists?—I would not say that distinctly, but I know a few of them, and I think that is the view held by them.

3607. What is your precise connexion with the Government department of the medical branch of the Privy Council?—In carrying out pathological investigations for Mr. Simon.

3608. That is to say, that Mr. Simon asks you to give him your assistance in a particular matter; there is no arrangement with you by which you may be considered an officer of the Privy Council, is there?—I do not think there is.

3609. (*Chairman.*) But the animals that are used for these purposes are paid for by the public money?—For my pathological inquiries only. I must mention that I make extremely few pathological inquiries. I have not made any other experiments of that nature, except those on artificial tuberculosis, on small-pox, and on typhoid fever. I understood that I was asked about my opinion, and I gave it, but it is another thing whether I actually do these things. If I were to be called upon to do experiments of these kinds, I would do them.

3610. (*Mr. Forster.*) I understand you to say, that as regards physiological experiments, you have not tried physiological experiments to any extent in England?—Very few.

3611. Have you done so at all in connexion with your employment by Mr. Simon?—No, none whatever.

3612. But what experiments you have tried have been tried in the Brown Institution at your own cost, I suppose?—In my private room. Those that I do for teaching purposes, physiological purposes, I do in my private room; I live there.

3613. Can you at all give us any idea of the number of animals that you find it necessary to obtain for experiments for medical investigations connected with the Privy Council?—At my work I do not think there have been more than 14 or 15 animals used in the whole of last year.

3614. (*Lord Winmarleigh.*) What were they?—There were three monkeys; of course, I would not call that vivisection; that is simply feeding them on peculiar stuffs, which has nothing to do with vivisection. I will leave out these therefore. Then there were three dogs and two cats, and a few mice, four white mice, I think.

3615. (*Chairman.*) Now in regard to the Brown Institution, what is your precise relation to the Brown Institution, do you receive any remuneration from it?—None whatsoever.

3616. You live in the building?—Yes.

3617. And this private room of which you speak is a private room belonging to the Brown Institution?—It belongs to the laboratory of the Brown Institution.

3618. That is to say, it is a part of the property held in trust by the London University?—I believe so.

3619. And you reside in it?—Yes.

3620. What is your particular duty to the London University?—I am ashamed to say I do not know; it has never been made clear to me what my position is there. A title has been given to me, that of assistant professor, and I am working in the laboratory, conducting pathological researches; those gentlemen who come there to undertake special researches work under my direction.

3621. And are these pathological researches for the benefit of the Brown Institution?—I do not know that it has ever been put down in that way.

3622. The pathological researches are made upon animals?—Yes.

3623. Are those animals furnished to you by the Brown Institution?—No; I make pathological experiments only for the medical officer of the Privy Council.

3624. Then for the London University you make no pathological experiments?—None.

3625. Then no animals are furnished to you by the London University?—None.

3626. Are these pupils of yours in any way connected with the London University?—No; they are pupils from different hospitals who wish to enter more closely into microscopical or histological anatomy.

3627. And in the teaching of those pupils you draw no distinction between painful experiments and non-painful experiments if the students themselves raise no objection to see the animal subjected to pain?—Yes; I think that would be quite what I expressed before.

3628. Therefore any students who come there, so far as your teaching and influence are concerned, adopt, I presume, the principle that you have adopted?—Yes.

3629. And consider that a physiological inquirer has too much to do to think about the sufferings of the animals?—Yes; may I be allowed to state, with regard to experiments for teaching purposes, that there are extremely few that I really do.

3630. But I presume that the experiments which you perform for original research are, many of them, of a painful nature?—I have performed very painful experiments with reference to artificial tuberculosis, because I regard it as very painful to produce a disease coming on very slowly in an animal by injecting into its abdominal cavity a foreign material; but I do not regard it as a painful experiment to inject typhoid fever into an animal, seeing that the material has no effect whatsoever.

3631. But now coming to vivisection proper, you do perform in this laboratory operations which involved a great deal of pain to the animal?—Yes, we do occasionally; of course they are very few.

3632. And without any question of employing anæsthetics, unless it happens to be for your own convenience to do so?—Yes.

3633. And that principle, so far as your influence goes, is derived from you by the pupils who come to benefit by your teaching?—That applies only to that part of the experiments which is for teaching purposes.

3634. (*Mr. Forster.*) With regard to the pathological experiments which you tried in connexion with Mr. Simon, you mentioned that there was one that is particularly painful, the one connected with tuberculosis?—Yes, tuberculosis and pyemia.

3635. And that is done by injecting?—Yes, by injecting a certain fluid into the abdominal cavity, or a vein.

3636. I suppose the operation of the injection is painful?—No, the injection itself is nothing, the consequences of the experiment are painful.

3637. Is there no operation which is painful?—I daresay injecting into the veins of a dog is a painful operation, the making a cut into the skin, but that is the whole thing.

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3638. How long would that last?—Not half a minute.

3639. You give no chloroform, I suppose, when that is done?—No.

3640. Are there any other of these experiments which require what may be called operations in doing them?—No, except injecting into veins and injecting into the abdominal cavities. I would not regard that as a painful operation any more than injecting into a man. Besides those I do not remember any.

3641. When you take hold of an animal for this purpose, what is done with it; do you bind it up while you are making this injection?—If it is a large vigorous animal, as a dog, we do bind it and fasten it. A cat we generally must chloroform.

3642. Why do you not chloroform a dog?—We chloroform a cat because we are afraid of being scratched.

3643. Why not a dog?—If it is a small dog there is no fear of being bitten by the dog.

3644. I hardly need ask you, with your earnestness in investigation, you would run the risk of a scratch if you thought that giving chloroform would spoil the experiment?—Yes, if I had to open an artery to insert a cannula into it (I am talking only of conditions which may be; I do not say it has happened with me), I should chloroform it, not being scratched, and the cannula being torn out.

3645. Have you had any directions from Mr. Simon with regard to pathological investigations in consequence of any minute which I wrote when I was in the Privy Council?—Yes, I remember.

3646. Do you remember what they were?—Yes, I think I remember that it was your express desire to use animals for pathological experiments as little as possible, and to use poison producing as little pain as possible: those were the contents.

3647. Do you recollect anything definite in those instructions, or in Mr. Simon's instructions to you upon those instructions, with regard to the use of chloroform?—The fact is that I do not remember that we used any animal strictly for vivisection, except injecting into the veins and injecting into the abdominal cavities; for one could not call it vivisection injecting lymph of small-pox into the skin of a sheep, any more than one could injecting vaccine into the arm of a child.

3648. Have you ever had instructions from Mr. Simon, that with regard to experiments, pathological or physiological, conducted by you, chloroform was to be used in the case of any severely painful operation?—We had no severely painful operations.

3649. Would you inform him beforehand of what you were going to do in the way of vivisection?—No. I think Mr. Simon knows too much himself about scientific investigation to interfere with an investigator.

3650. I want to know whether he has ever told you, as a general rule, for your guidance, that in any severely painful operation chloroform is to be administered for any experiment connected with the Privy Council?—The only direction which Mr. Simon gives is, that he gives the thema to do this and that investigation; but to say how we are to do it, or to criticise the results we obtain, I do not think he ever undertakes.

3651. (*Lord Winmarleigh.*) He never gives directions under those circumstances, whether it should be done under anæsthetics or not; is that what you mean?—There is no occasion for him to give them.

3652. (*Sir J. B. Karslake.*) Have you given us now the most painful experiments that you have ever performed by Mr. Simon's instructions?—I have given you a list comprising injection of pyæmic matter or tuberculous matter into the abdominal cavities of animals, or into the jugular vein, injecting lymph in the skin of sheep, and feeding animals on typhoid dejecta.

3653. Then it has not fallen to your lot at present to perform what you would call a more severe experiment with the knife than that of opening the jugular vein of an animal?—No other.

3654. Now is there any pain caused to a dog in the administration of chloroform?—I think there is; at least we see a great deal of struggling going on in the animal; the animal struggles very hard.

3655. Before it takes effect you mean?—Before it takes effect.

3656. In each of those cases which you have mentioned to me in which you performed the experiments by the direction of Mr. Simon, was it necessary to keep the animal alive after the actual incision or actual injection?—Yes, always.

3657. For how long a time?—Sometimes for weeks, sometimes for days.

3658. Then after the original injection or incision, the pain would be caused by the development of the disease?—Yes.

3659. In those cases, I suppose chloroform would be useless?—Quite so.

3660. As I understand you, if you were directed to perform an operation for the purpose of ascertaining some fact, or supposed fact, with reference to the nerves of a dog, and it became necessary to cut the back of the dog severely for the purpose of exposing the dog's nerves, for the sake of saving yourself inconvenience, you would at once perform that without the use of anæsthetics?—Yes.

3661. And it is only because the dog might howl, or get into contortions, that you would use anæsthetics at all?—Yes.

3662. But it so happens that for Mr. Simon's purposes, at all events, you have never been called upon to perform such an experiment?—Never.

3663. Do I understand you to say, that you have any public class, in contradistinction to the private class you have spoken of, of individuals who attend the laboratory?—No public class.

3664. Then the experiments you perform involving pain to animals are in the presence of certain gentlemen who come to the laboratory for the purpose of learning?—Yes, and altogether there are only three or four such experiments.

3665. How long have you been performing those experiments before a private class?—I may say three years.

3666. Can you tell me in the course of each year how many experiments you have performed before that class involving pain to animals?—Two.

3667. Two in each year?—Yes: sometimes two, and sometimes three.

3668. Will you give us the three which you have performed?—The cauterisation of the cornea of a frog; then the injection into the blood vessels of a frog; then the cauterisation of the cornea of a kitten, but that is done under hydrate of chloral.

3669. That is an anæsthetic?—Yes.

3670. And was that done in that way to satisfy the pupils, or for your own convenience; I am now upon the experiments for teaching purposes?—That was also done for keeping the animal quiet while doing it, and for the pupils convenience, and for mine.

3671. Have you ever enunciated to these private pupils who come before you your view that a physiologist engaged in the research should pay no attention to the pain of animals, but should avoid the use of anæsthetics?—I have never had occasion to speak to pupils of private research, of special research.

3672. As I understand you the case has occurred in your private laboratory? — Not remonstrance exactly. I remember it only once; I said, "I am "going to perform this and that experiment."

3673. Just tell us what it was?—The cauterisation of the cornea of a frog. I said, "Do you object to "that," and only on one occasion I remember one said, "Yes, I think it is useless."

3674. How many pupils were present at that time? I have never more than five or six at a time.

3675. Did you continue to perform the experiment notwithstanding that remonstrance?—No, I did not

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do it; I do not generally do these experiments for all the pupils at once; because my pupils are not as in a class carried through instruction in the same subject, but every one works in a special subject; and so I do the experiment for one.

3676. But as I understand, the whole number of experiments before every pupil in the class, or every individual, does not exceed three?—I mean three kinds; but the experiment may repeat itself several times.

3677. How many animals are sacrificed, do you suppose, in the course of the year for the purpose of these experiments for teaching your private pupils?—I can say only in a rough estimate. I should not think more than 10 or 12.

3678. And of those, what is the proportion that would be of the lower animals—frogs, for instance?—Frogs are the greatest quantity; one kitten, perhaps, among the whole lot.

3679. You say one kitten; have you only destroyed one kitten in these experiments?—No; I have only made one experiment on a kitten. I have cut off the heads of many kittens, but have made an experiment on a living kitten for teaching purposes only once.

3680. Have you ever made an experiment on a living dog for teaching purposes?—No; I think not.

3681. In the case of frogs, you never take out any part of the brain before you perform these experiments, do you?—No.

3682. That is a short process, is it not?—Yes.

3683. But you think it unnecessary, because you say that a physiologist has a right to do as he likes with the animal?—Yes.

3684. Now in your own private research, what animals have you chiefly used since you have been in England?—Guinea-pigs, rabbits, rats, mice, frogs, dogs, cats, monkeys, and sheep.

3685. But in the case of monkeys, as I understand, that was only for the purpose of ascertaining the effect that food would produce upon them?—Yes, or feeding them with some typhoid stuff.

3686. Can you give me what, in your judgment, was the most painful experiment that in your private research you have tried since you have been in England, the one causing the most pain to the animal; I refer to the actual pain inflicted in the course of cutting the animal?—I do not think I have done any more severe experiment than cutting the skin, and injecting into the jugular vein.

3687. That is the most painful experiment you have performed in England, you think?—That is the most painful experiment I have performed in England in my special research.

3688. I think you have said that you have been engaged in this Brown Institute a little more than four years?—Yes.

3689. Did you come direct from Germany?—Yes, from Vienna.

3690. Have you studied in Germany?—Yes, in Vienna.

3691. What laboratory did you come from?—Professor Stricker's, in Vienna.

3692. How many years had you studied under him?—I had studied only pathology and histology under him about four years.

3693. How many lectures of his did you attend in the course of the year?—No lectures.

3694. Or how many demonstrations?—His laboratory was a laboratory where private researches were carried out, and I being his private assistant was more or less connected with all the experiments that were carried out there either directly or indirectly.

3695. And might I ask you, in the course of the last year that you were engaged as assistant to Dr. Stricker, about how many experiments were performed in his laboratory?—I could not say.

3696. Was the number in hundreds?—No, far less.

3697. What animals did he chiefly use?—Rabbits and dogs.

3698. Not frogs?—Yes, frogs also; that is the physiologist's animal.

3699. However the largest animals he used were dogs and rabbits?—Yes.

3700. Now there was one answer which you gave in the course of the questions put to you in which you said that other physiologists in England take the same view of the subject that you do. Do you know any physiologist that works in his private laboratory except yourself in England?—I think that is more a matter of private talk, is it not.

3701. You have given an answer; I want to know whether you have any accurate information which enables you to say that other physiologists in England take the same view of the subject that you do?—I have no accurate knowledge about it. I only expressed an opinion, a belief. I believe it is so; I could not prove it, and I do not know for certain whether it is so; but I believe that there are other physiologists who take the same view that I do.

3702. (*Mr. Erichsen.*) You make a distinction, I suppose, between physiologists and practical physicians?—Yes.

3703. Of late years there has been a great change, has there not, in that respect in the scientific world in England. Formerly experiments were usually made by practising physicians and surgeons in order to determine some special point in practice?—Yes.

3704. For instance, a man ligatured an artery in order to see what the effect was?—Yes.

3705. But of late years a class has grown up in the medical profession, biologists and physiologists, who do not practice medicine in any way whatever, who may or may not have been educated for the medical profession, but who devote the whole of their time to the study of the laws of life upon animals?—Yes.

3706. And it was to them that your remarks were applicable?—Yes.

3707. (*Mr. Hutton.*) Did I rightly understand you to say that the frogs were always killed after this cornea operation, or only sometimes?—As a general rule they are killed.

3708. You have to excise the cornea, have you not?—Yes.

3709. And if they are not killed what is done with them?—At that time they are generally killed—at least to my knowledge always—but there may be one or two occasions where I have not time to look after them, and I do not know whether my orders are carried out or not.

3710. (*Chairman.*) Do you mean when you say "killed" that the operation killed them?—Before the cornea is cut out the animal is generally decapitated, and then the cornea is cut out from the head.

3711. (*Mr. Hutton.*) Whether that is always so done you are not sure?—I cannot speak with certainty about any one thing, but as far as my knowledge goes it is so.

3712. I take it that the operation of scraping the cornea which precedes, I believe, the injecting into it would be a very painful one to the human being?—Yes.

3713. You suppose that a frog does not suffer as a man in that operation?—Yes, I think it does not.

3714. But you say it would be a painful one; and the frog is killed at the end of an hour, or what period?—I would say 20 minutes.

3715. Is that the English or German frog?—The German frog.

3716. And how long would the English frog be kept alive?—Twice as long.

3717. Have you ever performed in England this operation described in your part of this handbook, at page 108, on the mesentery of a frog?—I have done it.

3718. It would be a much more painful one, would it not?—That is done always under curari.

3719. But it is quite an open question whether curari is, or is not, an anæsthetic for a frog?—I think

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it is; Schiff made some experiments to prove that it is.

3720. Claude Bernard thinks it is not, I believe ?—Those that believe in experiments will, I think, agree that it is.

3721. But supposing it is not an anæsthetic, this experiment on the mesentery would be much more painful, would it not, than the cornea experiment ?—Well, there is another question necessarily involved in this, namely, how much less would a frog feel if he does not breathe ? The fact is, that the frog does not breathe with his lungs after the application of curari, but breathes only through the large vein of the skin.

3722. And you think that might diminish the suffering ?—I have no doubt it does.

2723. Have you performed this experiment on the mesentery of a frog before your private class, or not ?—I have done it perhaps twice, not before a class, but before persons who were there for teaching purposes. I have done it several times for the purpose of pathological investigation.

3724. You have done it always under curari ?—Yes; it could not be done without it.

3725. You have to keep the creature still, you mean ?—Yes.

3726. And that is the reason of your using it ?—Yes; and even if curari were not an anæsthetic I believe that the frog would undoubtedly feel much less when the breathing is reduced to that very small amount which takes place through the vein of the skin than it does when curari is not applied.

3727. And have you ever done this experiment at page 98, about the injection of small mammalian animals during life ?—I never did that before pupils.

3728. Have you done it in this country for private research ?—I have never done it myself in England; I have done it in Vienna.

2729. Have you ever had to make experiments on arterial pressure in England, in the course of your teaching ?—No.

3730. Can you give me the least idea of about the quantity of animals required for the Vienna laboratory in a year ?—No.

3731. Not even an approximate number ?—No.

3732. Is it a very large laboratory ?—Yes. Professor Striker's laboratory is; the room is small, but the amount of work done there is very great, or it was.

3733. And of course a considerable number of animals are used there ?—Yes, very considerable.

3734. Is it at all as large as Ludwig's laboratory at Leipsic ?—No.

3735. Have you ever studied there ?—No.

3736. You could not give any idea what the consumption of animals there is ?—Not in the least.

3737. Are there any other foreign laboratories that you have studied in ?—No. I have studied only in Professor Brücke's and Professor Striker's, in Vienna.

3738. And Professor Striker's is the larger of the two ?—Yes.

3739. And you think that the view of scientific men on the Continent is your view, that animal suffering is so entirely unimportant compared with scientific research that it should not be taken into account at all ?—Yes, except for convenience sake.

3740. Have you told us everything that is done in the Brown Institute; have you had no inquiries there but what you have mentioned in answer to Sir John Karslake ?—I believe I have not omitted anything.

3741. When regard to any experiments on dogs which you have made, have they been simply cutting the carotid artery or the jugular vein ?—Yes, I think that is all.

3742. There is nothing that has lasted then for any considerable time ?—No; I could not remember now anything else.

3743. I thought I understood you once to say that you had performed on dogs operations that had lasted half an hour ?—Half a minute, perhaps, but not half an hour.

3744. I thought you said operations that might take half an hour, but that they were never done before pupils ?—I do not remember anything of the kind except bleeding a puppy to death by opening the carotid, and that is a matter of half a minute.

3745. (*Sir John Karslake.*) What was that for ?—To draw out as much blood as possible, say for injection of the lymphatics of the lung. That can be done most conveniently, and with best success, by first bleeding the animal to death.

3746. (*Mr. Forster.*) Do you recollect whether Mr. Simon informed you that when I was in office I had said something to him about this, or did he give you a minute that I wrote ?—I think he spoke to me about it; but really it is so long ago that I could not be certain.

3747. You cannot recollect whether he gave you a minute ?—No.

3748. You do not recollect his giving you any words written by me, to this effect, "That no experiments on living animals should be conducted at the cost of the State without the employment of some anæsthetic in case of painful operation, and without a report from time to time by the gentleman conducting the experiments, explaining their object and showing their necessity for the purpose of discovery." Do you recollect seeing those words ?—No. May I be allowed to say this, that at that time I was not connected directly with Mr. Simon. I was at that time simply an assistant of Dr. Burdon Sanderson, so that Mr. Simon could not have occasion to give me that instruction in an official way.

3749. When you were put directly under him you had not that minute laid before you, as I understand you ?—No.

3750. (*Mr. Hutton.*) Have you ever discussed this question of the relation of animal pain to physiological inquiry with Dr. Sanderson ?—I think we did.

3751. And I think that he differs from you on that point ?—Yes; he does differ from me.

3752. But he understands your views on the subject ?—I do not think we have entered very minutely into it; but I remember I had repeatedly occasion to say, "I do not think it is a matter of necessity to administer chloroform in a general way."

3753. (*Lord Winmarleigh.*) You stated just now that you believed that curari was an anæsthetic for certain animals ?—Yes.

3754. What are your reasons for that belief ?—My reasons are chiefly the experiments given by Schiff, he made some experiment, and I think it proves that curari does not act always on the sensitive nerves. We know that it paralyses the motor nerves.

3755. But Claude Bernard says, does he not, that it does not destroy the sensitiveness ?—That was the general belief until these experiments of Schiff's were known.

3756. Have you read Claude Bernard's reasons for his view ?—No, I have not.

3757. (*Mr. Hutton.*) You are perhaps aware that Claude Bernard describes two experiments on man with curari ?—No.

3758. (*Lord Winmarleigh.*) Will you state, as shortly as you can, the exact reason for supposing that curari is an anæsthetic ?—First of all, the reason which I gave to Mr. Hutton, namely, that if you poison a frog with curari you stop the breathing through the lungs, it breathes only through the cutanean vein; and it is quite clear that it does not keep up its vitality, its sensitiveness, to such an extent by the small amount of oxygen it may get through the vein.

3759. Would that destroy sensitiveness ?—It would lower it.

3760. But not destroy it ?—I do not think it would destroy it, but it would destroy it undoubtedly. And then, secondly, I believe, with Schiff, that curari does act as an anæsthetic for this reason. If you ligature, for instance, one artery of the lower limb of a frog and then poison the animal with curari, you will find that the movement of that limb is not destroyed, because curari could not penetrate into the muscles

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of that region of which the artery has been ligatured. If now you pinch the other leg which is curarised the frog will show movement with the uncurarised limb, because the muscles are not paralysed. If then you paralyse that limb where the curari is not acting on the muscles it does not move the limb. That shows that where the muscles have been paralysed also the sensitive nerves have been paralysed, and that where the muscles have not been paralysed the sensitive nerves have not been paralysed.

3761. Is that movement indicative of pain?—Yes; we take it generally that when we pinch the frog and it does not move it does not feel.

3762. Dr. Schiff practises in Florence, does he not?—Yes.

3763. Is he a large experimenter in physiology?—Yes; from what he has written, and from his researches, I may say, perhaps, that he is one of the largest.

3674. Are you aware whether he uses anæsthetics?—I am not. I could not say.

The witness withdrew.

Mr. Edward Albert Schäfer, M.R.C.S., called in and examined.

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3765. (*Chairman.*) Are you assistant professor of physiology in University College?—Yes.

3766. Assistant to Dr. Burdon-Sanderson?—Assistant professor to Dr. Burdon-Sanderson.

3767. Now have you paid considerable attention to the subject which has been referred to us?—Yes, I think I may say so.

3768. You have seen such experiments of course as have been performed at University College?—I have seen a good many of them.

3769. Speaking generally, you know what is taking place at University College?—Yes.

3770. What is the practice there with regard to experiments which are painful in their nature, are anæsthetics always employed?—If possible.

3771. And to whatever extent possible, in cases where it is not possible to do it entirely?—Certainly so.

3772. Do I rightly understand you to say that at University College the treatment of animals is dictated by a sentiment of humanity?—Most strictly so; I am quite certain of it.

3773. And that that is the view both of the superior medical gentlemen, those with whom you are associated, and also of the pupils?—I believe it to be so.

3774. Is the proportion of experiments that are painful in their nature very large?—Very few experiments on the whole are done at all.

3775. The principal part of your teaching I suppose is upon the dead tissues?—My own is entirely upon the tissues.

3776. But I mean in the college the experiments are most of them upon the dead tissues, are not they?—The physiological experiments are not on dead tissues; so far as the chemistry is concerned they are entirely.

3777. The experiments in the physiological laboratory are very much upon the dead tissues?—They are certainly upon the tissues, but then those are histological for the most part, and appertain to the branch of the physiological department which has for its object the study of the dead tissues.

3778. What is the department in which experiments are carried on upon the living subject?—That of practical physiology, of which again there are two departments, one in which the professor himself does experiments upon living animals, and shows them to a class; and another department, which is only just springing up, in which advanced students or medical men come to the laboratory and perform experiments on living animals in conjunction with the professor or myself.

3779. Now are almost all those experiments performed under chloroform?—They are almost all performed under some anæsthetic, either chloroform, or ether, or opium.

3780. Is curari, or wurari, treated as an anæsthetic in your laboratory?—No; at the same time we are not fully convinced as to how far it may be an anæsthetic.

3781. And therefore while that question is unsettled, you do not treat it as being so?—Therefore we frequently give opium as well.

3782. Can you tell us what is the whole number of living animals consumed in your laboratory in a year?—I cannot tell you, but it is very small; that is, not including frogs. So far as rabbits and dogs are concerned; of dogs there are perhaps not half-a-dozen consumed in the year; of rabbits a greater number, but not very many. But it depends entirely upon what series of experiments are being performed.

3783. Then with regard to frogs, do you consume a large number of frogs?—Yes.

3784. Are those the English frogs, or frogs imported for the purpose?—Some of them are English frogs, and some of them of the larger kind.

3785. Is the larger kind preferable for all experiments?—For nearly all experiments.

3786. And why is it preferable?—Because the parts are larger for manipulation.

3787. Not because the animal is of greater endurance?—Certainly not; I do not think it is of greater endurance.

3788. Then are the experiments upon living frogs usually performed with or without anæsthetics?—Usually without.

3789. Is no measure taken to diminish the pain of the frog?—The only measure taken is great care in the performance of the experiments; no other measure.

3790. No anæsthetic of any sort is used?—Very seldom indeed.

3791. Is that because you have the opinion that the frog is not so sensitive as the higher animals?—That is my opinion.

3792. And that is the ground upon which the practice rests?—That is the ground.

3793. Have you received your education in England or abroad?—In England; I have studied abroad.

3794. (*Sir J. B. Karslake.*) I understand you to say that sometimes you do administer anæsthetics to frogs?—I have seen them administered; I never do it myself.

3795. Are the experiments which you make upon frogs painful in their character?—They would be if the frog had an amount of sensibility in proportion.

3796. Some of these experiments involve cutting the frog open, do they not, and exposing the lungs, and so forth?—Certainly.

3797. Would the anæsthetic in any way interfere with the experiment in a great number of instances?—We do not know what anæsthetic to apply to frogs; we do not know that the anæsthetic would not give as much pain as the operation itself.

3798. Do you remove the brain of the frog, or partially remove it, before performing the experiment?—Yes, that is very often the case; the spinal marrow is cut across.

3799. Is that done in all cases where it can be done without injury to the experiment, or in most cases?—It is done chiefly in those experiments in which it facilitates the object of the experiment.

3800. Then it is not done from a motive of humanity?—I think not.

3801. Then may I take it that there are a great number of experiments which, supposing a frog to be a sensitive animal, must cause a vast deal of pain, which are not done under chloroform?—There is no doubt about it.

3802. And there is no precaution taken to diminish pain, if it suffers pain?—I think I may say no special precaution.

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3803. How long have you been lecturer at University College ?—This is my second year.

3804. Did you study at the college before you became a professor ?—I studied there eight years.

3805. Was this the practice ever since you remember anything connected with the college ?—Yes.

3806. That frogs were as a practice not subjected to narcotics or anæsthetics of any sort ?—Yes.

3807. Now as regards the other animals that are used, are they invariably put under anæsthetics, except where necessarily the operation cannot be performed with the assistance of anæsthetics ?—Quite invariably ; I do not think I have ever done experiments myself in this country on one of the higher animals without anæsthetics.

3808. Is that from feelings of humanity ?—Strictly so.

3809. (*Lord Winmarleigh.*) As to those experiments on frogs, do I rightly understand you to say that they were made in your lectures, or in examinations for research ?—In examinations for research.

3810. And you do not practice upon the frog, as you have described now, in your lectures ?—No.

3811. Does anybody ?—I think Dr. Sanderson may in his *practical* course, but I do not assist him in that, and cannot, therefore, speak positively on the point.

3812. (*Mr. Forster.*) With regard to these experiments which you have made in your own research, were they original or new experiments, or were they repetitions of others ?—Entirely new.

3813. Can you tell me what would be your impression with regard to repeating experiments ; when you would consider it necessary to do it ?—I think it is not necessary just for the sake of repetition ; but if you doubted the result then it would be justifiable to repeat experiments.

3814. What do you think is the practice of physiological investigators ; do they think it desirable to repeat the experiments generally themselves, or do they take them for granted ?—They generally take them for granted, if they have already been corroborated by somebody else, and if the names attached to the experiment are of high rank in physiology. For instance, if a series of experiments have been performed by Professor Ludwig of Leipsic, it would generally be taken for granted that the result is a correct one, I mean to say if a definite result is obtained at all.

3815. If he has published that experiment as satisfying his mind that the result has been obtained, physiological investigators in this country would not think it worth while to repeat it ?—Certainly they would not, I think.

3816. (*Mr. Erichsen.*) I think I understood you to say that the reason why you did not give chloroform to frogs was that you thought that it might give more pain, or as much pain, as the experiment ?—I think it would be a great irritant to the skin of a frog.

3817. You probably know as much as anyone in London of what is going on amongst the younger class of physiologists of the day, and I suppose I am correct in saying that the most active physiological life is amongst those young men of the present day with the exception of a few men, whose names are well known ? —I think so, but I think the younger men as a rule work under the older men.

3818. I was going to ask you whether, knowing well the general habits of the younger physiologists in London, the more active physiologists, you are aware that any of them practice vivisection or experiments on animals for scientific purposes in other places than in recognized physiological laboratories ?—I never saw or heard of such a thing.

3819. I mean in their own houses or in private laboratories fitted up for the purpose, out of medical schools ?—I do not know of such.

3820. The number of physiological laboratories in London is not very large, is it ?—Very limited.

3821. I believe the largest are those of University College and Guy's Hospital ?—And King's College.

3822. And the Brown Institute ?—Yes.

3823. Then it is very easily known, or it could be easily ascertained, how many men are working practically at physiology, how many physiologists in London are working at any given time ?—Very easily indeed, I think.

3824. But then there is another class of men who can scarcely be called physiologists, the medical students still in statu pupillari not qualified practitioners. Are you aware whether they are in the habit of practicing experiments in their own rooms, either singly or associating together for the purpose, or elsewhere than in physiological laboratories ?—I never heard of such a thing.

3825. And you have been for many years as a student and as a teacher connected with one of the largest schools of medicine in London ?—Yes.

3826. And a school of medicine in which physiology probably has been more actively cultivated than in any other school in England ?—I think it has.

3827. So that if it were the case that medical students were in the habit of experimenting either singly or in parties, and in their own rooms or elsewhere than in physiological laboratories, it could scarcely have escaped your knowledge ?—I think it is hardly possible it should have.

3828. In the experiments that are made before students, it is usual to give an anæsthetic ?—I think it is always done at our laboratory.

3829. Have you ever seen an experiment performed before a body of students to which the term "frightful" could be applied with propriety ?—Not one to which I should apply the term " frightful." I have seen formidable experiments, but not without anæsthesia.

3830. I will ask you distinctly, have those experiments been under anæsthetics or not ?—Always.

3831. So that the animal suffered no pain ?—Not the least.

3832. Is it your opinion, from what you know of medical students, that " the more frightful the experiment, the more attractive it is " to the student ?—I have never noticed such a thing.

3833. We were told by an eminent teacher of physiology who has lectured for a great number of years in London, that if he were to perform a painful experiment without anæsthetics before his class, it would probably be repugnant to the feelings of the class ; they might remonstrate, and might leave the room ?—Yes.

3834. Is that your experience also, or would that be your opinion also, if such a thing were done ?—I should not be surprised if some left the room.

3835. Do you think that a lecturer on physiology in one of the large London schools could venture to experiment on animals, performing painful experiments without using anæsthesia, without the risk of a commotion in his class ?—I think there would be such a risk.

3836. But you do not know whether it would be so or not positively, inasmuch as, if I understand you right, you have never seen class experiments of a painful character performed without anæsthesia ?—Not upon the higher animals.

3837. With regard to the frogs, are they frequently pithed, or do they have the brain removed ?—Yes, for certain experiments.

3838. (*Mr. Hutton.*) I understood you to say, that not knowing whether curari was an anæsthetic or not, you frequently gave opium as well. Do you mean that sometimes you do not, that you only give curari in some cases ?—Sometimes we do not, when the opium would act prejudicially to the object of the experiment.

3839. Can you describe to us any experiment in which you would give curari and not opium ?—One would give curari and not opium in certain experiments which might be necessary to make in researches upon the nervous system ; for instance, in researches upon the nerves which govern the blood vessels.

3840. Now in experiments on arterial pressure would you give curari and not opium ?—In some experiments on arterial pressure we should.

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3841. And those, I suppose, would be very painful experiments if curari is not an anæsthetic?—Not necessarily so.

3842. I mean they would last a long time?—They might last a few minutes, or they might last an hour or two.

3843. Can you give me any account of the most painful experiment that you have seen used with curari and no other anæsthetic?—I have not had much experience in that way, but I suppose it would be such an experiment as I mentioned just now, an experiment to expose and stimulate the nerves of the blood vessels.

3844. And, I suppose, in stimulating the nerves, there would be a good deal of pain, would there not?—As a rule not, because these are special nerves of the blood vessels, and not nerves of sensation.

3845. Are they motory nerves?—Motory nerves for the blood vessels. In certain cases sensory nerves might also be included.

3846. Now have you ever seen the experiment of bringing the mesentery of a frog out upon a glass plate to show the circulation?—I have often done it.

3847. Would you give only curari for that?—Yes, I think so.

3848. And supposing the frog to be a sensitive animal, that would be a very painful experiment I suppose?—I think the beginning of the experiment would be a little painful, but if done carefully there would be no special pain during the experiment; I doubt if the mere exposure of a serous membrane induces pain in the lower animals.

3849. Do not the experiments on arterial pressure last a long time, many hours?—They may do so.

3850. Did I rightly understand you that you have been studying in Ludwig's Laboratory in Leipsic?—I was there this summer.

3851. But not for any length of time?—I was there for about three months.

3852. Can you give the Commission any idea of the number of animals that would be used in the course of the year in such a laboratory as that?—I cannot give an idea, but it would be a very large number.

3853. Hundreds, for instance?—Hundreds, if you take animals of all kinds; of rabbits and dogs, I suppose more than a hundred.

3854. In the course of a year?—Yes.

3855. And that without counting "the physiologists' animal," the frog?—Without counting the frog.

3856. A witness that we have had lately gave us the impression that amongst medical men abroad there would be a certain amount of contempt for attaching so much importance to the sufferings of animals when any scientific object was in view. Is that your impression or not?—I cannot say that it is my impression; I am perfectly certain that abroad they also think about the feelings of the animal that they operate upon.

3857. They try to economise the pain as much as possible, you think?—I am quite certain of that.

3858. As regards demonstration, are a much larger number of demonstrational experiments used abroad than are used in England that are likely to give pain?—There are not a very large number performed abroad of demonstrational experiments that are likely to give pain, but I think more than in England.

3859. On what do you ground your idea that the frog is not a particularly sensitive animal; have you any scientific reason for that?—One is perfectly certain that the frog at least cannot reason about the sensations that he has; and on that account in itself any actual sensations that the frog feels cannot be looked at in the same light as the pain which the higher animals, which undoubtedly have a certain amount of reasoning power, would feel.

3860. One of our witnesses, a veterinary witness, said that he had observed that frogs were quite as sensitive to parasites as even the higher animals, and appeared to suffer as much from parasites as higher animals, and he seemed to think that was a reason for supposing that frogs did suffer as much as the higher animals. Would you think that that was a reason of no weight?—I myself do not see the weight of the reason; I think it is of no weight.

3861. You think the restlessness and the irritation shown is not any measure of the suffering?—Not necessarily so; for this reason, that a frog will show sometimes more restlessness after its brain is removed than before.

3862. But that surely applies equally to human beings if the spine has been divided?—Unquestionably there is sometimes more reflex action when the man feels nothing than there was before.

3863. So that that rather breaks down the argument in the case of the frog, does it not?—Not altogether; because since there is ordinarily a large amount of reflex action in the case of the frog, it shows that the brain has less power in controlling those reflex actions in the lower animals than it has in the higher animals, and therefore is probably less active in other respects also, in receiving impressions of pain amongst other things.

3864. (*Chairman.*) Then at University College you carry the conviction of the comparative insensibility of the frog to pain so far, that you do not think it necessary to take any precautions to mitigate the suffering that you inflict upon frogs?—We do not take any precautions in the way of anæsthetics. The precautions we take are to so arrange the experiment that it shall involve the least amount of injury to the animal, consistently with the success of the experiment.

3865. But do you remove the brain, or even the head, of a frog in all cases in which it is possible to do so, without destroying the effect of the experiment?—That we certainly do. I may remark that we principally use the frog for three purposes; for experiments upon the heart and circulation, for experiments upon the muscular system, and for experiments upon the nervous system. In the first-named experiments, those upon the heart and circulation, it would in many instances not be allowable to remove the brain, because the removal would cause too great a shock to the animal; but in both the other cases, the experiments on the nervous system and on the muscular system, the frog is almost in all cases killed before the experiment; and frequently the tissues are taken from the dead animal.

3866. Did your answer about the frogs apply to the classes and to demonstration, or to physiological research?—My answer as to the frog with regard to anæsthetics applied to both.

3867. Then you mean that a considerable number of frogs are used in demonstration in the class in the course of a year which are not deprived of insensibility by any proceedings, either the use of anæsthetics or the severing of the connexion between the brain and the part operated upon?—A *few* such are employed for demonstration in the class; a considerable number are employed for research.

The witness withdrew.

JOHN GRAY M'KENDRICK, M.D., called in and examined.

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3868. (*Chairman.*) At the present moment you are in private practice as a physician in Edinburgh?—Yes.

3869. You are also a lecturer upon physiology in Edinburgh?—Yes.

3870. Not, I think, in the university itself?—No.

3871. But in a school which is in some way connected with the university?—Yes.

3872. Will you be so good as to explain to us the precise connection of that institution with the uni-

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versity?—The Extra Academical School of Edinburgh consists of a body of lecturers upon various subjects included in the medical curriculum, whose lectures qualify for graduation at the university, and also for the various examining boards throughout the kingdom.

3873. You were before you occupied this position assistant to the late Dr. Hughes Bennett, were you not?—Yes.

3874. And you have held an appointment within the university itself?—Yes.

3875. What was that appointment?—Assistant to the Professor of Institutes of Medicine.

3876. You have some connection with the Veterinary College, I think?—Yes. I also lecture upon physiology in connection with the Edinburgh Royal Veterinary College.

3877. In those capacities you have had considerable acquaintance with the subject which is referred to us?—Yes.

3878. Will you be so good as to give us the advantage of your experience upon the subject referred to us; namely, how far experiments upon living animals are in your opinion necessary for scientific purposes; and next, what is the mode in which those experiments, as far as you know, are practised in this country?—Probably the better way of giving you that information is to refer you in the first place to a very short paper which I wrote for the British Medical Journal of January 9th, 1875, entitled, "What has "Vivisection done for Humanity?" in which I have attempted to answer the question, what has vivisection done for medical science, by enumerating a number of discoveries which I believe were made by this mode of investigation. These discoveries have regard to the physiology of the nervous system, to the circulation of the blood, to respiration, to the functions of various nerves, and to certain points connected with the physiology of the senses. All of the facts which were discovered by these investigations now form, as it were, the groundwork of the knowledge of all medical men in the detection and treatment of disease; and without, for example, the knowledge which has been acquired by this mode of investigation with regard to the functions of the nervous system, I do not see how it could be possible at the present time for a medical man to make an accurate diagnosis of certain cases of disease. For instance, I may allude here to the discoveries of Sir Charles Bell and Brown-Séquard regarding the functions of the various parts of the spinal cord, and more especially the connection of the upper part of the spinal cord with the base of the brain. Anatomy so far can give information regarding the conditions of certain diseases of the nervous system; but I do not think it could possibly explain the whole phenomena. If, for instance, a man was paralysed on one side of his body, say the right side, without a knowledge of the exact arrangement of the fibres in the spinal cord, and how they cross over to the other side of the brain at the upper part of the spinal cord, I do not see how we could arrive at the knowledge that this paralysis was caused by a clot in the opposite side of the brain from the paralysed side.

3879. One would have supposed that the crossing of the fibres might have been discovered by anatomy?—The practical fact is, that it is extremely difficult, I should say almost impossible, to trace accurately the course of the fibres in the softer parts of the central nervous system. One certainly in the medulla can see indications both to the naked eye, and still better in microscopical sections, of a decussation or crossing from one side to the other; but without actual experimental inquiry, I think that the appearances are so indefinite that we should not be entitled to say that we thoroughly understood the cause or the conditions of the affection. Then with regard to the discoveries more particularly of Brown-Séquard, it is well known that that physiologist made out very distinctly that the posterior fibres of the spinal cord decussated all down the back of that part of the nervous system; and he showed that by making sections, say, into one part of one side of a posterior column, there was no paralysis of motion, but paralysis of sensation upon the opposite side; and on cutting the anterior column he found paralysis of motion upon the same side. Now, what I contend for is, that mere anatomical investigation would not have discovered the exact course of these fibres in the spinal cord. Certainly it is right to state this, that after or about the same time as Brown-Séquard made these investigations, Mr. Lockhart Clarke, of London, made a number of sections of the spinal cord, prepared for observation under the microscope, and pointed out certain decussating fibres. But the whole truth would not have been ascertained by an examination of these sections alone. The experimental evidence had to be conjoined with the evidence derived from microscopical examination. Then I might give somewhat similar instances with regard to the physiology of circulation and of respiration. There is one interesting experiment, for example, which at once gave the physician an intelligent comprehension of the cause of cardiac murmurs sometimes heard over the heart when you apply the stethoscope over that region. Certain experiments were made by the late Dr. Hope, in which he investigated the causes of the sounds of the heart, and he showed that by interfering with certain of the valves experimentally murmurs were produced; indeed he imitated artificially, in a manner, conditions which we know frequently occur in disease. Another remarkable example, I think, of the value of the experimental method in discovering the physiology of a certain part of the nervous system we have in the well-known researches of Dr. John Reid. No doubt there are certain points still not thoroughly cleared up regarding the functions of certain of the cranial nerves; but anyone who reads books on physiology written before his time can see that there was then no correct information regarding the functions of the great nerves issuing from the base of the brain. As an example of bad treatment in consequence of deficient knowledge of the functions of these nerves, we have the well-known instance of section of the seventh nerve, which we now know to be the motor nerve of the face. It was supposed to be a sensory nerve—indeed at one time the distinction between motor and sensory nerves was not thoroughly understood; and the consequence was that for neuralgic affections of the side of the head the seventh nerve was frequently cut by surgeons instead of severing the nerve which conveyed the impressions to the brain and gave rise to the pain. Then, without going through these cases now (I have given a large number of instances in the paper), I may point out a research in which I was personally connected, in which I scarcely think we could have derived any information without experimental inquiry, and that is regarding the problem of the action of light upon the retina,—a problem which is not limited merely to the action of the stimulus upon this single organ, but no doubt has a bearing upon our knowledge of the action of all the senses. This investigation, which was conducted by my friend Professor Dewar, of Cambridge, who recently was a colleague with me in the Veterinary College, and myself, brought out the fact that light produces a change in the electrical condition of the retina, and that this change is transmitted to the brain. I have brought with me a few papers which give a short account of this research. It is a good example, I think, of a purely experimental research—a research of which the results could not have been arrived at either by observation of disease or by anatomical observations. I have another series of experiments to allude to; (I think it better, perhaps, to give as evidence what I am personally acquainted with myself;) I have given in my paper the list of what others have done. I allude now to researches which are contained in the volume I have before me, a volume entitled "Researches "into the Antagonism of Medicines, being the Report "of the Edinburgh Committee of the British Medical

"Association, by John Hughes Bennett." I may say that I did nearly the whole of that work. The object we had in view was to find out whether or not certain active substances exercised a counteracting influence within the body; so that in the first place we might be able perhaps to discover real antidotes for poisons, and in the second place (and I consider this the most important of all) that we might perhaps be able to get upon a track which would lead us to counteracting the effect of the zymotic poisons which are believed to produce the various fevers. This research orginated, I may mention, chiefly in a great desire which Dr. Hughes Bennett had to find out an antidote for opium. Opium poisoning is of course pretty frequent, and at present there is no apparent efficient system of treating the case. A great many observations were made upon this point, without however, I am sorry to say, our being able to find out an efficient antidote for opium. We, however, ascertained an antidote for a very powerful poison, namely, strychnine. Previous to our work this had been discovered by a German, Oscar Liebreich, of Berlin. Here we have an instance of a distinct physiological antagonism, so distinct that if I were called upon to see a man in tetanic convulsions from strychnia poison, I should not hesitate for one moment in at once putting him powerfully under the influence of the hydrate of chloral. I know of one instance which occurred in the practice of a friend of mine, Dr. Angus M'Donald, of Edinburgh: he had given a patient strychnine as an ordinary remedy for a nervous affection; the man had unfortunately taken too much,—he had not taken an excessive dose apparently, but probably there was a predisposition to the remedy having a more powerful effect upon him than upon others; he was brought in a cab to my friend's door in a partially rigid condition; he was passing into the rigid tetanic convulsions characteristic of strychnia poisoning; Dr. M'Donald had seen the experiment in the physiological laboratory of the University of Edinburgh of rabbits being submitted to the action of both poisons, and he was so convinced of the powerful efficacy of hydrate of chloral that he took the man into his study and administered the substance in various doses; and kept him there for six or eight hours, when the man went home tolerably well. I give this as an instance of a practical effect resulting from purely physiological investigation. This is one of the most striking instances, I think, that I could possibly give of the value of experimental research. You may give a rabbit of, say, three or four pounds weight the 60th part of a grain or the 40th part of a grain of strychnine; it will die probably in one tetanic convulsion in the course of perhaps 10 or 15 minutes. Give another rabbit of equal weight and apparently of equal strength the same dose, followed by a dose of from 15 to 20 grains of the hydrate of chloral, it will lie quite quiet, breathing calmly; perhaps there may be a few convulsive twitches, but there is a very strong probability that it will recover entirely. In this investigation, too, we also directed our attention to other antagonisms. We repeated, for instance, some of the remarkable experiments of Dr. Thomas Frazer, now of Knutsford in Cheshire, upon the antagonism between sulphate of atropia and the Calabar bean; and we found that he was perfectly correct in stating that there is a true physiological antagonism between these two active substances. So that here again in a case of poisoning by Calabar bean, which sometimes has happened, I should have no hesitation in ordering sulphate of atropine. We found also that hydrate of chloral was an antagonist to the Calabar bean, even to a greater extent than sulphate of atropia. The other part of the investigation was certainly not so satisfactory. We devoted a great deal of time and trouble to finding an antagonist for opium, but in that we did not arrive at very good results.

3880. You give these instances to show that important results are frequently attained by experiments on living animals?—Yes.

3881. Now were these experiments largely tried on living animals?—Yes, they were tried to a considerable extent.

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3882. Was this experiment upon the eye you speak of a very painful experiment?—No, it was not.

3883. Were the experiments you tried upon the rabbits by poison very painful?—In some cases I have no doubt they were painful.

3884. (*Mr. Forster.*) How long would the pain last?—In the case of strychnia poisoning, for instance, I would never allow an animal to have a series of spasms from strychnine; I would kill it and not allow it to suffer pain for a lengthened period; but I have no doubt in giving strychnine that there is a powerful tetanic convulsion in which there is a time of pain.

3885. How long?—Sometimes a few minutes. In the case of strychnia poisoning, the symptoms do not make their appearance until almost immediately before the spasmodic attack; the animal may be running about or eating its food, and suddenly it becomes convulsed, and dies usually at once.

3886. Now for the purpose of showing the operation of strychnine proper, is it necessary to have any more experiments?—No, I do not think so.

3887. That is established?—Yes.

3888. The object of these particular experiments was to try whether hydrate of chloral would not prove an antagonist to strychnine?—Yes.

3889. Which you have established?—Yes.

3890. And you do not consider it necessary to repeat that?—No.

3391. In the school in which you are now lecturer do you repeat these experiments for the purpose of demonstration to the pupils?—I did so on one occasion; I should not consider it necessary to do so now.

3892. And if not necessary, I presume not justifiable?—In certain circumstances I might repeat some of the experiments. For example, take an instance of this kind: if I were a Professor of Physiology, and I wished a student to investigate, say, such a disease as tetanus, I should consider it my duty in the first place to let him see tetanic convulsions in a rabbit, but I would not, as a rule, show it to a large class.

3893. Do you in the University of Edinburgh encourage students to perform experiments themselves?—No.

3894. Then if any student performs an experiment of this kind, it is under the direct control and superintendence of the principal lecturer?—It is.

3895. Do I rightly understand that the practice in Edinburgh is to regard the infliction of pain upon animals as a great evil?—I personally regard the infliction of pain upon animals and pain generally as a great evil.

3896. An evil only to be compensated, in your opinion by some great advantage?—Exactly.

3897. In most of the experiments that are performed for the purpose of demonstration we have been told that anæsthesia may always be induced without any prejudice to the success of the experiment?—In my own experience, I may say that I have performed very few experiments for purposes of demonstration in which anæsthetics are required. I do not wish to lay it down as a rule for my brother physiologists, but I am content with showing students, for instance, the physiology of the nerves and muscles, the physiology of the heart, certain facts regarding respiration and the action of the heart upon a frog after the animal has been deprived entirely of sensation by the removal of the head, which is done instantly. I also demonstrate the circulation of the blood in the same animal under the microscope.

3898. Then you confirm the opinion that for purposes of demonstration to pupils, by one mode or another, any experiments upon living animals may be performed under anæsthetics?—Yes.

3899. Now for original research is it often necessary to resort to experiments which cannot be performed under anæsthetics?—No, I do not think so, in my own experience, with the exception of researches

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into the actions of drugs. It is quite evident that there it would not do to complicate matters by administering another substance.

3900. But where you have tried experiments upon such poisons as strychnine, I understand you that there is no prolonged torture of the animal?—No; there might be; it would depend, of course, upon the humanity of the investigator.

3901. But with a proper feeling on the part of the investigator, there would not be?—No.

3902. And that proper feeling, in your opinion, ought, I presume, to be observed?—I think it ought.

3903. And there would be no loss to science if that feeling were always observed?—I do not think there would be any.

3904. Then in other experiments (I am now speaking of original research), where pain cannot be altogether avoided, it may be very much mitigated?—It may.

3905. And it may be that in an experiment the whole of which cannot be altogether free from pain, the most painful portion can be done while the animal is insensible to pain?—I should say in most cases. In most of the researches in which I have been engaged that is quite so. For example, in those researches upon the eye we never experimented upon any warm-blooded animal except under the influence of anæsthetics; and in our earliest experiments we first worked with the eye of the frog after the animal had been killed. And I may mention the following fact as a striking example of how, when science advances, vivisectional experiments may become less required than they are at present. We thought at first that it was necessary to remove the eye of the frog and place it upon our apparatus in order to detect the action of light upon it by means of a delicate galvanometer. After having found that out, we had next to convince ourselves that this impression was carried from the retina to the brain,—that required certain vivisections which were performed under anæsthetics. After that had been done we investigated the transmission of this effect to various parts of the brain,—that was also done under anæsthetics. But latterly, within these last six or eight months, we have been able to prove this remarkable fact, that if we place our apparatus in connexion with the eye of the animal uninjured, part in contact with the surface of the cornea, the other part of the apparatus in contact with a small abraded surface of the skin, (the least thing possible, where the hair is merely stripped off, for example,) we still can obtain the effect.

3906. So that in that case nothing to be called pain is inflicted?—Nothing; but that result was obtained after and in consequence of our previous experiments.

I may also be allowed to mention another research in connexion with my own work. Various anæsthetics are used by physiologists; chloroform by some, hydrate of chloral by others, and many use woorara, regarding which there is a difference of opinion as to whether or not it has any anæsthetic properties. Professor Dewar and I entered upon an investigation upon the physiological action of a number of substances which are made by the action of potash upon quinine or strychnine. These substances are known to chemists as the chinoline bases. We have here a series of bases which differ from each other by slight chemical differences, increments of carbon and hydrogen; and we proceeded to investigate the physiological action of these substances from the simpler ones to the more complicated ones, in the hope that we might possibly come upon some of the higher ones that would have an action upon a living body analogous to that of sulphate of quinine, which no doubt would be a very important discovery. During this investigation we ascertained that one of the substances, termed chinoline, has a very remarkable effect upon the living body of the mammal. It differs from woorara in not paralysing the nerves or nerve-terminations distributed through the body, neither sensory nor motor, I believe, but it acts principally upon the nerve-centres, the various ganglia of the brain; and it is remarkable that it does not act to a great extent upon the respiratory centres. So that where it is administered to a rabbit, say three grains to an animal of four pounds weight, the animal becomes completely unconscious, and still respiration goes on: the animal is fit for any kind of observations that may be necessary. I am convinced that in that case there is no feeling of pain.

3907. Now has this substance ever been tried upon the human being at all?—It has; I tried it upon myself by swallowing some.

3908. Were you insensible to pain?—I may explain that the difficulty with reference to the higher animals, and what deprives it no doubt of its value in the meantime, is that it at first excites nausea and vomiting. If it did not excite severe nausea and vomiting, I have no doubt it would be a most important substance; chinoline, I believe, will ultimately replace woorara in physiological laboratories in many experiments.

3909. But you did try it upon yourself?—Yes, I did.

3910. Was any pain inflicted upon you while under it?—I was not able to take sufficient to produce the anæsthetic effect because I got so sick; but we attempted it upon a dog and upon a monkey. It was quite sufficient to do it on those two; and we saw at once that there was a great practical difficulty with regard to its application in the higher animals.

3911. (*Mr. Forster.*) Was it injected?—Yes, a small quantity injected.

3911*a*. You have not tried injecting it on a human being?—No, I swallowed the doses administered to myself.

3912. Would there be a difficulty in injecting it?—There would be a difficulty in getting it in a sufficiently concentrated solution.

3913. (*Sir J. B. Karslake.*) It caused vomiting in the monkey, did it?—It caused vomiting in the monkey although injected, and it is quite evident it was not applicable; it is a remarkable fact that where we had the more highly developed brain it caused this action, but in the rabbit, the guinea pig, the hedge-hog, the bird, the mouse, the rat, the frog, and the fish it had the effect I have just mentioned of an anæsthetic, without interfering with respiration.

3914. What is it?—An artificial production, which may be made by the action of caustic potash upon quinine or strychnine. But I mentioned this research not on account of its intrinsic importance, but by way of showing the practical aim which we had in working, namely, to try if possible to work up to a high base like sulphate of quinine, which would no doubt be very valuable; because chinoline can be made from substances which exist in coal tar.

3915. (*Chairman.*) Then I collect that in your experiments on frogs you treat them as animals that have sensibility, and you do not operate upon them under the hypothesis that they have no sensibility?—No, certainly not. I can show a great many phenomena connected with the muscular system, the nervous system, the action of the heart, and the action of the skin, without subjecting the animal to anything more than instantaneous pain,—killing it. There is one fact that I should like to mention to you. It has been stated, or rather there was a suspicion abroad, that students have engaged largely in vivisection. During all the time that I have been connected with the Edinburgh Medical School only one instance has come under my own observation. That was the case of a student in the infirmary who had been performing certain experiments upon a cat. I heard of it, and I considered it my duty to go to him and tell him that he should not do it, that he was working at random, and that if he had any proper ideas that might be of value he might come over to the laboratory and speak to me about it; and I succeeded in stopping his experiments. That is the only instance I ever knew, with the exception of students studying the circulation in the web of a frog's foot under the microscope.

3916. Will you have the goodness to put in the

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paper to which you have already referred as having been written by you in the British Medical Journal?—Yes. It is as follows: "What has Vivisection " done for Humanity?—Recent circumstances render " it desirable that some attempt should be made to " answer the question whether or not the practice of " making experiments on living animals has materially " aided the progress of medical science. To answer " this question with completeness would involve an en- " cyclopædic investigation of the sources and history of " our present knowledge. It would be a work into " which a great fund must be brought of patience, " time, and labour. We shall, however, endeavour " to present here at once and hastily some leading " data, such as may be gathered from a cursory review " of the subject. We offer them as *mémoires pour* " *servir*, and shall hope to be able to finish the picture " by filling these rough outlines as time and circum- " stances will permit. We invite assistance and criti- " cism from physicians, surgeons, and physiologists. " We present to-day a first contribution in the follow- " ing skeleton sketch. What has vivisection done for " medical science? A. *It has succeeded in advancing* " *our knowledge of physiology, by*, (1.) Discovery of " the two classes of nerves, sensory and motor, by Sir " Charles Bell. (2.) Discovery of the functions " (motor) of the *portio dura* of the seventh pair, by " Sir Charles Bell. Previously to this discovery, the " *portio dura* was often cut by surgeons for the cure " of neuralgia! (3.) Discovery of the functions of " the anterior and posterior roots of the spinal nerves, " by Sir Charles Bell. (4.) Discovery of the func- " tions of the anterior and posterior columns of the " spinal cord, by Brown-Séquard and others. (5.) " Discovery of one of the functions of the cerebellum " in co-ordinating muscular movements, by Flourens " and others. (6.) Discovery of the functions of the " grey matter on the surface of the cerebral hemi- " spheres as connected with sensation and volition, by " Flourens, Magendie, &c. (7.) Discovery of the " motor functions of the grey matter covering certain " convolutions in the anterior part of the cerebral " hemispheres, by Hitzig, Fritsch, Ferrier, Gudden, " and Nothnagel. (8.) Demonstration of the cir- " culation of the blood, by Harvey. (9.) Measure- " ment of the static force of the heat and discovery " of other hydraulic phenomena of the circulation, " of Stephen Hales, Ludwig, &c. (10.) Discovery " that atmospheric air is necessary to the maintenance " of life, and that, when stupefied by its withdrawal, " animals may be resuscitated by readmitting it, by " Robert Boyle in 1670. (11.) Discovery that atmo- " spheric air by continued breathing becomes vitiated " and unfit for respiration, by Boyle. (12.) Dis- " covery that the air was not only vitiated, but also " diminished in volume, by the respiration of animals, " by Mayo in 1674. (13.) Discovery of the relation, " as regards respiration, between animal and vegetable " life, by Priestley in 1722. (14.) Great discoveries " by Lavoisier on the physiology of respiration, from '1775 to 1780; namely, that respiration acts only on " the respirable portion of the air, or oxygen, while " the remainder, nitrogen, is entirely passive in the " process; secondly, that when animals are confined " in a limited space they die when they have absorbed, " or converted into carbonic acid, the greater part of " the oxygen, and so reduced the air to the state of " an irrespirable gas. (15.) Numerous facts in the " physiology of digestion, observed by Blondlot, " Schwame, Bernard, Lehmann, and others, by ex- " periments on animals. (16.) The discovery of the " functions of the lacteals, by Colin, Bernard, Ludwig, " and others. (17.) The discovery of the functions " of the eight pair of nerves in relation to deglutition, " phonation, respiration, and cardiac action, by John " Reid and others. (18.) The discovery of the func- " tions of the sympathetic system of nerves, by Pour- " four du Petit in 1727, Dupuy in 1816, Brachet in " 1837, John Reid and Brown-Séquard. (19.) The " discovery of the phenomena of diastaltic or reflex " action, by Marshall Hall. (20.) The discovery of the " action of light on the retina, by Holmgren, Dewar, and " M'Kendrick. (21.) The discovery of the glyco- " genic function of the liver, by Bernard, Macdonnell, " Pavy, &c. (22.) The discoveries of the whole series " of facts in the domain of electro-physiology, by " Matteucci, Du Bois-Reymond, Pflüger, and many " others. These discoveries have important practical " bearings. B. *In aiding medicine and surgery.* " (1.) The transfusion of blood and introduction " directly into blood of medicines; first proposed by " Robert Boyle in 1665. In 1665, Sower trans- " fused blood from vessels of one animal into " those of another. First done in human being by " Dennis and Emmerets in France in 1666. Blun- " dell's celebrated experiments on animals in 1818, " since done by many others,–Dumas, Milne-Edwards, " Dieffenbach, Bischoff, Doubleday, Brigham, Waller, " Burton Brown, Klett, Lane, Lavy, Berard, &c. " (2.) Artificial respiration. Vesalius showed that " by blowing up the lungs with air, after the chest " was opened, stoppage of the heart's action might be " delayed for some time. Hook in 1664 first demon- " strated the possibility of artificial respiration. " Brodie, Hope, Le Gallois, Wilson, Philip Marshall " Hall, and Silvester have practised it on human " beings. (3.) The causes of the cardiac sounds have " been determined entirely by vivisectional experi- " ments. (4.) Phenomena of the circulation within " the cranium examined experimentally by Kelly, " Burrows, Reid, &c. (5.) Hunter's operation for " aneurism was first demonstrated and tried on living " animals. This he did in 1785. He also found by " experiments on animals that in many cases the " arterial coats were diseased immediately above the " aneurism, and that consequently it was necessary, in " order to avoid secondary hæmorrhage, to place the " ligature higher up. (6.) The office of the perios- " teum in regeneration of bone has been demon- " strated experimentally by Du Hamel in 1740, " Hunter in 1772, Syme in 1837, Wagner in 1853, " and Leopold Ollier in 1858. The practical im- " portance of these observations is recognised by all " surgeons who have had much to do with diseases of " bones and joints. (7.) The researches of Redfern " into disease of cartilage. (8.) The researches of " Stricker, Cohnheim, Von Recklinghausen, and " many others on inflammation, more especially of " cornea and serous membranes. (9.) Without vivi- " section experiments, we would know almost nothing " of the phenomena of inflammation. (10.) Experi- " mental inquiries into many zymotic diseases, showing " occurrence of micrococci. C. *In advancing thera-* " *peutics, relief of pain, &c.* (1.) Use of ether. " (2.) Use of chloroform. (3.) Chloral, discovered " experimentally by Liebreich. (4.) The actions of " all remedies are only definitely ascertained by ex- " periments on animals. (5.) Action of Calabar bean, " by Fraser. (6.) Antagonism between active sub- " stances and the study of antidotes, many observers. " The above are simply examples which have readily " occurred to the mind. To record all the facts given " to physiology by experiments on animals, would " simply be to write the history of the science. Thera- " peutics is yet in its infancy; but nearly all the facts " definitely known regarding the actions of remedies " have been gained by experiments on animals. To " stop experiments on animals would as surely arrest " the progress of physiology, pathology, and thera- " peutics, as an edict preventing the chemist from " the use of the retort, test-tube, acids, and alkalies " would arrest the progress of chemistry."

3917. (*Lord Winmarleigh.*) I see you divide the subject into three heads, and in the first head, relating to the advancement of knowledge of physiology by means of vivisection, there are 22 instances of the benefits derived to human beings?—Yes.

3918. I presume that does not include the whole number?—No.

3919. Then are there other experiments of great importance which are not included in that list?—I think that I have mentioned the most important

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which I remember. I prepared that list with very great care at the time, and none besides occur to me at this moment. There may be some that I have omitted, but I think I have got in my list the more important facts that you will find in any work on physiology.

3920. Do you make the same observation with regard to the advancement of therapeutics and the relief of pain ?—No. I have no doubt there are a great many more there. I mention at the end of my paper, I think—at all events I should do so now—that the correct knowledge which we have of the action of almost any drug in use and practice has been derived by work of this kind.

3921. Almost all the instances were derived by vivisection, do you mean ?—Almost all derived by experiments on animals.

3922. May I ask you with regard to this new anæsthetic, the hydrochlorate of chinoline ; you have stated the successful experiments that have been made upon that ; have those experiments been recognized by the profession generally ?—They were published in the Proceedings of the Royal Society some months ago, and specimens of the substance have been given to a number of physiologists. I have no doubt they will be making up their minds about it. It has certainly not yet come into general use.

3923. When was the experiment made ?—It would be about the end of last year.

3924. What is the time that is usually occupied before a new discovery of this kind does come practically into operation in the medical world ?—That depends upon various circumstances. I should say that this substance has not commended itself to the profession, because unfortunately it has been found at present, inapplicable in the case of a human being inasmuch as it excited nausea and vomiting. If we had been able to say, "Here is something which has "these effects, without exciting nausea and vomiting," I have no doubt whatever it would at once have excited great attention. It has interested physiologists, both in this country and abroad.

3925. We should not therefore in this Commission be justified in saying that that is a discovery of a substance which for the future can be used as an anæsthetic in the experiments made upon frogs and the lower animals ?—I should think that you would be quite justified in stating that it is serviceable in experimental inquiries upon all animals under the grade, say, of the rabbit,—rabbits, pigeons, rats, mice, frogs.

3926. But still we should not be justified in saying that that is a material recognised by the profession ?—No, I cannot say that.

3927. You merely look upon it as a physiologist ?—I look upon it merely from my practical experience of it.

3928. And you do not look upon it that the results are perfect at the present moment ?—I am certain of the results as regards these lower animals, but it must be probably conjoined with other substances before it can be of use in the case of man and the higher animals. This is still for experimental enquiry. A precaution which I think should always be taken in the case of observations of this kind is, that after any point has been ascertained clearly, it should be shown to a few competent observers, so as to be stamped with authenticity, by their names. This was done, to give a notable example, almost invariably by Dr. John Reid. You will find in the record of many of his experiments that he said, "So and so ; "Professor Alison and Professor Syme were present."

3929. But this is a discovery that has been made very recently ?—Very recently.

3930. And do you believe that discoveries of equal importance to that have been more frequent in these latter times than they were formerly ? — I believe so. I believe we are coming upon a time just now when a great many discoveries will be made.

3931. Your reason for saying that is, that you have witnessed the rapid progress that has been made in these particular instances ?—Yes, and I also have been watching with great interest what Professor Burdon-Sanderson and others have been doing with reference to experimental pathology ; I regard their work as of the highest importance.

3932. I understood you to say to Lord Cardwell that you believed the greater part of these experiments could be made without the infliction of pain upon the animals ?—Yes. There are certain experiments in which you cannot entirely avoid a little pain ; for example, injecting a little of something underneath the skin ; one would not give an anæsthetic to an animal before doing that.

3933. Are you enabled to state to the Commission that you believe that the great majority of the experiments which have been stated to inflict the greatest pain upon the animal may be made under anæsthetics, and without cruelty to the animal ?—I think myself that the majority of the experiments necessary for demonstrating physiological facts can be done with the animal in anæsthesia ; and also in many cases for investigation. For example, in the case of some of those experiments upon the eye, I would have considered myself quite unjustified in performing these experiments upon an animal not anæsthetised unless there had been a great probability of success, judging from results obtained in anæsthetised animals ; that is my feeling. I do not experiment on living animals with or without anæsthetics at random.

3934. We have had some evidence that it is very difficult to perform experiments upon the liver and kidneys without pain to animals ; is that your opinion ?—I have no practical experience regarding that, but I have seen it stated that anæsthetics so affect the function of the liver itself as to vitiate the conclusions. But with regard to that view of the matter I may be allowed to say that the great art in experimenting is to notice the prominent conditions ; the problem of what is the precise effect of an experiment on a living animal is extremely complicated ; there are many factors in it. In some cases I believe one could make a certain allowance for an anæsthetic effect ; that is to say,—supposing you are experimenting upon the liver of an animal : it is a very difficult and serious operation ; in the first instance, no doubt it would affect the nervous and other functions of the animal, and one would not be entitled to say, perhaps, that suchand such effects were entirely due to such and such a substance introduced into the liver ; one would require to take all the facts into consideration, not simply the introduction of this one substance ; therefore I say that in some of these cases (though the particular instance would require to be left to the judgment of the physiologists engaged in the research) one might make allowance for the use of an anæsthetic, and thus save the animal from pain.

3935. (*Chairman.*) In performing such an experiment as that, as I understand you, you must disturb the animal one way or the other ?—Yes.

3936. If you give it great pain you disturb it in that way ?—Yes.

3937. And if you take away pain by an anæsthetic, you cause another disturbance ?—Yes.

3938. So that it may be that in some cases, by not administering an anæsthetic, the animal is not only unnecessarily troubled, but the scientific result is not equal to what it otherwise would have been ?—Yes, in some cases, but that must be left to the judgment of the physiologist investigating it,

3939. (*Lord Winmarleigh.*) This paper of yours I see was published in January ?—Yes.

3940. Since that period have there been any criticisms upon the paper you have put forward, in any of the public journals in Edinburgh or elsewhere ?—I have not seen them in any journal myself ; some one told me that he had seen a criticism or some observation about it somewhere, but I have no distinct recollection of it. I certainly did not see it.

3941. (*Mr. Forster.*) Taking these instances of the advantage of vivisection, I do not ask you whether the majority of those discoveries were made with the administration of anæsthetics, because I am aware that a good many of them would have been made before anæsthetics were introduced; but do you think that if they had to be made now the majority of these results could be obtained with the administration of anæsthetics?—Certain of those facts regarding the nervous system I do not think could have been ascertained with the use of anæsthetics.

3942. But that is only one branch?—That is only one branch.

3943. (*Chairman.*) Those discoveries relating to the nervous system were due chiefly to Sir Charles Bell, were they not?—Chiefly to Sir Charles Bell.

3944. Was he not one of the most humane of investigators?—He was. With regard, for instance, to determining the sensibility of a particular nerve, that could not be ascertained if the animal were completely anæsthetised, but it might be ascertained though the animal was not *completely* anæsthetised: it might be brought under the influence of an anæsthetic to a certain extent. With reference to the eighth pair of nerves, what I say as to investigations of that kind is this, that those investigations no doubt caused a great deal of pain and suffering; but it was done so carefully, so elaborately, by John Reid, and the results were shown to so many skilful men, as to collect such an enormous number of facts that I do not consider it necessary to repeat those experiments.

3945. (*Mr. Forster.*) I see that you have put under the head of the necessity of vivisection for therapeutics, the use of ether and the use of chloroform?—Yes.

3946. Could you tell us how far it was necessary to try experiments in their case—take chloroform?—With regard to chloroform, Sir James Simpson searched about for some time for an anæsthetic—he tried a great many substances on animals, on his assistants, and on himself; he got chloroform I believe from a chemist in Liverpool, and discovered its properties by experimenting in that way on himself and others, and on animals too.

3947. Do you think that he could have got at his results if he had been unable to try it on animals?—Yes, it is possible he might; but I do not think a man would be justified in trying various substances upon himself first until he has tried them several times upon animals.

3948. But would there not have been this danger, that in trying them upon a human being he would have been obliged to limit the extent of his experiment to a greater extent than in the case of an animal?—Yes.

3949. Supposing there had been a prevention of trying experiments upon animals, do you, or do you not, think that the discovery of chloroform would have been delayed?—I think it would probably have been delayed.

3950. And I suppose that those experiments did require some pain to be inflicted, because you had to search about and find out whether the animal showed a sensation of pain?—Yes. No doubt pinching, pricking, and various irritations of that kind; and there was all the disagreeable nausea that often follows inhalation of chloroform. I never had a conversation with any one immediately concerned with these investigations of chloroform; but I think that Simpson performed many experiments upon animals too. Dr. Matthews Duncan, of Edinburgh, was his assistant at the time, and I believe he took chloroform.

3951. (*Lord Winmarleigh.*) Was laughing gas amongst the number?—Yes; he tried a great number of substances which might be expected to produce anæsthesia. Sir James was on several occasions very ill indeed from his experiments, as I have been informed.

3952. (*Chairman.*) But these experiments which you are now speaking of were not of an agonising kind?—No.

3953. (*Mr. Forster.*) I understand that you have before stated in your answers to questions, that you would not try any experiments upon an animal that would give pain without anæsthesia, if you could do it in that way without spoiling the success of the experiment?—Exactly.

3954. And that you would not repeat a necessarily painful experiment unless you thought it was absolutely necessary for verification?—Yes.

3955. Do you see any objection to such legislation as would cause other inquirers to conduct their investigations in the same humane manner as you do your own?—I scarcely see how legislation would have any distinct beneficial effect. I have thought over that matter; various forms of legislative recommendations have come up to my mind, but all of them have certain difficulties about them. For example, having experiments done only in licensed places. Well, there is one objection to that, that it necessitates an observer or inquirer—one who wishes to do something to advance science—to go to that place, and to communicate his ideas and plans to the men in charge of that institution; and one who is earnest in investigating truth should not be subjected, I think, to having to do that.

3956. Have you had the opportunity of reading a Bill brought in by Dr. Playfair in the last session?—Yes.

3957. That, as you are aware, contemplated the licensing of persons rather than of places?—Yes; I think that would meet my view—licensing of persons, not of special places, or of special investigations.

3958. (*Chairman.*) But to licensing persons you would not object?—No, I do not see any great objection to that.

3959. If you knew that there were persons practising in this country who openly avowed that the sufferings of animals were no sort of concern to a physiologist, you would think it reasonable that such people as those should be restrained?—From my point of view I certainly think so; I do not take that view of it at all.

3960. (*Sir J. B. Karslake.*) Have you a private laboratory yourself?—Yes, I have.

3961. And do you find that it is a matter of convenience to you to be able to perform these experiments in your private laboratory?—I do.

3962. Do you think that any legislation interfering with that course would be objectionable?—In what way interfering?

3963. Which should compel every person engaged in physiological research to go to some public and licensed laboratory for the purpose of making his experiments?—I certainly would object to that.

3964. You find it necessary to perform your experiments at odd times, and at convenient moments to yourself?—Yes.

3965. You are a practising physician, as I understand, in addition to lecturing?—Yes.

3966. Now you have spoken about certain experiments that are made before classes; do I rightly understand you to say that for purposes of demonstration before students, no experiments are ever made on a living animal for the purpose of showing the effects of poison by you?—I have shown such experiments. I have shown students the action of hydrate of chloral and strychnine. I must say that my feeling is (I would not run very strongly against other people's feelings in such a matter), that it is an important matter to show a number of students the antagonistic influence, for instance, of two such substances as strychnine and chloral. The thing is so marked, and produces such an impression on the mind, that it could not fail in prompting them to give hydrate of chloral in cases of strychnine poisoning, or cases of tetanus.

3967. As I understand you, you do show the effect of strychnia to students of the higher class in private classes?—Yes.

3968. Now do you think it necessary to show the higher class of student the effect of strychnine on the body of the living animal, in order to assist him, and make him perfect in his learning upon that subject?

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—Yes, I think so; I think in the case of strychnia poisoning, he should see it.

3969. That a tetanic convulsion is so peculiar that it cannot be explained by reasoning, but must be actually seen?—I think so.

3970. You stated that you had something to do with the Veterinary School in Edinburgh?—Yes.

3971. Are experiments on living animals made for the benefit of students in that school?—Not at all.

3972. Have you engaged in physiological research at all with a distinct view to the remedies to be applied to animals?—No; I cannot say so, except that with regard to the investigations I have mentioned, so far as they may benefit the animals, they are useful in that way. I have only been connected with the Veterinary School during one year. I had relief of pain and the cure of disease in view in most of my physiological work, with the exception of the research on the eye which is a purely scientific investigation.

3973. As far as you know, have these experiments on living animals ever been exhibited in the Veterinary School?—So far as I know, they have not. I have heard it spoken of, but I have seen nothing of it; and I can certainly say it is not done at present,—not since I have been connected with it.

3974. I dare say you have a very large number of students under your control in the course of the year, either in public or private clases?—Yes.

3975. Has more than one case occurred in which you have had to remonstrate with a student for the practice of carrying on vivisection in his own house?—No, only one.

3976. Was it stopped on that occasion?—It was.

3977. Have you as good means of knowing as any man can have, whether that practice is carried out at Edinburgh?—I think I have.

3978. And in your judgment it is not resorted to by students?—I do not think it is resorted to by students to any extent. I think if it were I should hear of it, as I heard of that case.

3979. You have stated that that is so with regard to students; now, in your opinion, are young medical men, who have passed beyond the period of studentship, in the habit of practising these vivisections?—No; not in the habit of doing so.

3980. Then you think that it is confined practically to the laboratories, where students are admitted sometimes to take part in the experiments, under the supervision of the experimentor?—Yes, it is practically limited to that.

3981. (*Mr. Forster.*) Do you think it is the habit of students to try those experiments while preparing the theses which they write?—I think they always do so, so far as my own experience goes, under the supervision of the teacher.

3982. (*Sir J. B. Karslake.*) Is it necessary to some extent to have a laboratory fitted up for the purpose of carrying out these experiments?—It is.

3983. And would it be difficult for any ordinary medical man or student to carry them out in his own room without having a laboratory?—It would; the apparatus is very expensive, and during the last 10 or 12 years experimental methods have been introduced quite different from those employed hitherto; the expensive apparatus of the laboratory is necessary for most of these. I could imagine certain investigations being carried on at home, but very few.

3984. Am I right in thinking that in your profession where a person does engage in physiological research, if he discovers anything he is very quick in giving his discovery to the profession?—Yes; as a rule he is anxious to bring it before the notice of the profession as quickly as possible.

3985. (*Mr. Erichsen.*) Your attention has chiefly been directed, I think, to experiments of a toxicological and therapeutical character?—Yes, chiefly to them; but I regard my principal work as being in connexion with the eye investigations.

3986. And they were in the domain of pure science?—Pure science.

3987. Am I right in concluding, from what you have stated to us, that you think we shall not advance very much further in our knowledge of therapeutics and in toxicological inquiries except through the medium of experimentation?—I certainly think so.

3988. That we have arrived at a sort of finality in the old method of investigation of the action of drugs and poisons?—Yes.

3989. That very important results, directly beneficial to humanity, have already flowed from the experimental method of investigation?—I think so.

3990. And that more may be reasonably expected to follow?—Yes.

3991. Is it your opinion also that the same remarks hold good with regard to pathological investigation?—Yes, I hold that opinion very strongly indeed. In a lecture which I delivered yesterday to the Veterinary College, I pointed out prominently that I thought pathological anatomy—the mere description of diseased organs, their form, and size, and colour, and so on,—has been done so frequently that little more is to be gained in that way; that it teaches little regarding the origin of diseases; and that investigations into pathology conducted on the experimental method are likely to produce very great results.

3992. Both in the prevention of diseases and in a knowledge of the true nature of diseases?—Yes.

3993. And consequently, in all probability, in the cure of those diseases?—Yes.

3994. We may take it, I suppose, that the more we know of the true nature of a disease, the more likely we shall be able to cure it?—Yes.

3995. And with regard to physiological investigations of a purely scientific character, am I right also in thinking that you are of opinion that those investigations are sometimes productive of direct practical benefit, although at the time of their performance no direct practical application may have been anticipated by the experimentor?—I quite think so. As an example of that, you may take many of those researches into the physiology of the nervous system. Those investigations into the physiology of vision have received a great deal of attention from psychologists, because it is the first attempt to trace the direct action of an external agency upon a sensory organ, and the influence carried to the brain.

3996. The Physiological School of Edinburgh has always been an extremely active one?—It has.

3997. I suppose I may fairly say that it has been the most active in Great Britain as a whole?—Yes, as a whole; recently Dr. Burdon-Sanderson has been carrying on a very large number of investigations with the aid of many able coadjutors.

3998. And yet, in the centre of so much physiological activity, you think that physiological experiments, at all events that vivisections are not carried on out of the physiological laboratory to any extent?—That is my opinion.

3999. And the students are not encouraged by their teachers to practise in their own rooms or out of the laboratory?—Certainly not.

4000. (*Mr. Hutton.*) Were you associated with Dr. Hughes Bennett in the experiments that were made, which have been recently revised by Professor Rutherford?—No, I was not, except on one or two final experiments. I think I performed only one, perhaps two experiments in connection with that enquiry, and they were not of the same nature exactly as the previous experiments.

4001. And can you tell me how those experiments were performed; they were on dogs, I believe?—Yes. They were as to the action of mercury on the liver.

4002. Were they performed under anæsthetics or not?—I really cannot say. I do not remember what was said in the report; I should fancy the dog would be anæsthetised during the time that the fistulous opening was made.

4003. Have you read the account of Professor Rutherford's own experiments published in the last Medical Journal?—No, I have not carefully read it;

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I heard him give an account of them at the British Medical Association Meeting.

4004. You are aware that the dogs that were experimented on were only put under curari, and that then the effect of the various drugs upon them was tried?—Yes.

4005. What would be your impression of the amount of pain inflicted by that class of experiments? —My impression is that the animals would suffer a considerable amount of pain.

4006. And do you think, allowing, as you suggested should be done, for the effects of the anæsthetic by separate observations, that those experiments might have been done with equal scientific results under anæsthetics?—I am not prepared to answer that. Professor Rutherford has directed special attention to that subject, and has had more experience in it perhaps than anyone in Europe; and I really would not take it upon me to say that the same accurate results would have been obtained if the animal in these instances had been under the influence of an anæsthetic.

4007. But do not you suppose that curari itself might vitiate the result, almost as much as the anæsthetic?—I recollect that Dr. Rutherford pointed out that he had first tried the influence of curari alone upon the secretion of bile, and then, as I understood him, he made an allowance in his further experiments for its influence.

4008. Why should not that experiment have succeeded as well if it had been an anæsthetic instead of curari?—That is what I cannot answer, because I do not know how an anæsthetic exactly would affect the secretion of bile; that was for Professor Rutherford to investigate.

4009. I was struck with the expression which you used that you would not answer for your brother physiologists. I conclude that meant that there is a very considerable variety of opinion amongst physiologists as to the amount of pain which it is allowable to inflict for scientific purposes?—Yes, there is a considerable latitude of opinion.

4010. Now with respect to any kind of restrictive measure, do you not conceive that any man ought to be a competent physiologist who is allowed to make these experiments at all?—At all events he should be under the guidance of some one connected with physiological work.

4011. Supposing there were any psychologist making these experiments without any thorough physiological education, would you object to his being restrained?—If I heard of any one personally performing experiments of that kind, and I thought he was not a sufficiently competent person, I think I would try to point out to him that he should not do it; but I am afraid that any stringent legislative restriction would have the effect of rather preventing the advancement of physiological science in this country.

4012. (*Lord Winmarleigh.*) Would you say including the issue of a license?—I do not object to the issue of a license to persons.

4013. (*Mr. Hutton.*) But even supposing a certain amount of research was prevented in that way, would you not say it was more than compensated by the protection given to animals by such a restriction?—If I thought there was a great abuse of the power of vivisection in the country, I would take that view, but I do not think there is.

4014. (*Lord Winmarleigh.*) You do not dread its creeping in from abroad?—I do not think so. My own impression is, that this investigation by this Commission will have a most beneficial effect, apart from legislative interference at all.

4015. (*Mr. Hutton.*) You said that you would not have made some of the experiments on the eye except under anæsthetics; I suppose you meant, it being in your power to make them under anæsthetics; you did not mean that you would have sacrificed the result altogether rather than give the pain that would have been involved in making those experiments without anæsthetics?—If we had found, for example, that there was no effect of light upon the retina in a deeply anæsthetised animal, we should have tried one experiment to see the effect upon an animal without its having received an anæsthetic; but we found the effect at once. We saw there was no necessity for doing it otherwise. These experiments, indeed, were of such a character that they could not have been done *without* anæsthetics.

4016. But supposing that had been the case, and you had found that it gave great agony to the animal, should you have desisted altogether, or thought it your duty to go on?—I think I should have felt it my duty to go on. I should have done it once, and done it with every precaution possible; but really that case is very rare.

4017. I suppose you can hardly say it rarely happens; probably in these experiments of Professor Rutherford's very great pain has been inflicted?—Yes, I fancy so.

4018. So that really it is a balance of the one good result, of the scientific result, against the bad result of the pain to the animal, after all, is it not?—No doubt the two aspects are to be taken into account.

4019. I take it that your general opinion is of the strongest kind, that any practical measure which should not severely restrict research, and which should ensure humanity, ought to be adopted, if such should seem necessary? — That would depend upon the character of the measure. I do not see any objection, as I have said, to a licence granted to competent persons to enter upon investigations of this kind, something in the same way as a licence granted for anatomical work, although certainly the two cases are not exactly similar.

4020. (*Chairman.*) Your connexion with veterinary practice will enable you to tell me whether you think that the general advance of physiological science will tend to mitigate sufferings in horses, for example, and the animals which are now attended by veterinary surgeons?—I have no doubt it will.

4021. Therefore if the course of practice pursued tends to elevate altogether the scientific knowledge of the country, it will on the whole greatly diminish the sufferings of animals?—No doubt of it. I pointed that out in a lecture yesterday.

The witness withdrew.

Adjourned to Saturday next, at 12 o'clock.

Saturday, 30th October 1875.

PRESENT:

THE RIGHT HON. VISCOUNT CARDWELL, IN THE CHAIR.

The Right Hon. Lord WINMARLEIGH.
The Right Hon. W. E. FORSTER, M.P.
Sir J. B. KARSLAKE, M.P.
THOMAS HENRY HUXLEY, Esq.
JOHN ERIC ERICHSEN, Esq.
RICHARD HOLT HUTTON, Esq.

N. BAKER, Esq., Secretary.

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GEORGE HOGGAN, M.B., recalled, and further examined.

4022. (*Mr. Huxley.*) I think you stated that Dr. Handyside advised you to conduct this experimental inquiry because you had stated to him that you had new views upon respiration?—I had done so during two medical years, and I constantly had the same advice from Dr. Handyside.

4023. Did the testing of these new views of yours require experiments upon living animals?—Whenever I had a discussion with any medical person on this question, when they could no longer contradict the views I held, they always said, "I will believe that "when you show me it on animals, and if you wish "this to be accepted by the profession you must have "it attested by experiments on animals;" that was the motive always given.

4024. Will you be so kind as to tell me what was the nature of the experiment suggested?—With regard to the action especially of the diaphragm, and the action of the intercostals. In fact, my theory did away with the necessity of muscles of inspiration. I denied the action of them as they stood, and I said that the motions of the others would be different if they acted as they were supposed to act.

4025. Then the persons with whom you discussed this matter said that that was a question which could only be decided, in their judgment, by experiments upon living animals?—Yes.

4026. Was the question ever raised as to whether such experiments could be made under anæsthetics or not?—I dare say that it has been said to me, though I could not say now who said it, but in reply to my objection that I would not give animals useless pain in proving what everybody seemed to prove beforehand, whatever their views were, the answer was sometimes made to me, "If you do not want to "give animals pain, cannot you give them anæs-"thetics?"

4027. It is a matter of fact, is it not, that the respiratory movements go on perfectly well in an anæsthetised animal?—I am not prepared to say that they do go on the same under anæsthetics as without them.

4028. They go on in such a manner, however, that all the great classes of respiratory actions are performed as they would be in the normal state, is it not so?—No; I am not prepared to assent to that proposition. We find that a little thing alters the conditions; an individual standing up or lying down alters, to a certain extent, the movements of respiration.

4029. But the problem which you propounded is one which relates, from what you just told us, to the action of one of the principal of those muscles which are commonly regarded as respiratory?—Yes.

4030. And one the action of which I think you would admit, if you had examined anæsthetised animals, was not seriously interfered with during anæsthesia?—I quite agree that it is not seriously interfered with, and I was quite prepared, and did begin to do the experiment with anæsthetics.

4031. So that, in point of fact, the recommendation to you to make these experimetns did not involve a accommendation that you should make experiments which, while they lasted, were painful to the animal?—I can say this, that the question of trying experiments on animals was put clearly to me by itself, without any consideration of anæsthetics at all; and it was only when I gave my objection to torture animals, as I considered uselessly, that it was said to me by way of overcoming my scruples, "But cannot you do it "under chloroform?"

4032. I only want to get out the facts clearly; and its results, from what you have stated, that the recommendation made to you, although there may have been no explicit statement about anæsthetics one way or the other, to perform these experiments did not necessarily imply that you were to perform experiments of a painful character?—There was the implication that the experiments were of a painful character, or I would not have given the answer that I did.

4033. But if the animal had been anæsthetised while they were being performed, the pain would have been removed, would it not?—Yes, if thoroughly anæsthetised.

4034. I think on the last occasion when you were here you brought forward as an example of an excessively cruel experiment one which is described in a work by Claude Bernard?—I did.

4035. Have you the details of that experiment here?—I have not the book with me, as I brought it down last time, and thought we had done with it; but I shall be happy to give it exactly.

4036. I think I can describe it to you sufficiently accurately for the purpose now. This experiment referred to the changes which take place in the blood in passing through a gland, did it not?—Yes.

4037. And it was further intended to show the influence of the irritation through a particular nerve, the chorda tympani upon the blood passing through the glands?—It was intended to show its action in causing the vessels to dilate.

4038. Now I think during that experiment the chorda tympani was laid bare?—Yes.

4039. And you will grant it once that that is a moderately severe operation?—Yes.

4040. And in the second place the chorda tympani was irritated by galvanic means, was it not?—Yes, generally it was tied and drawn out.

4041. Do you think that the irritation of the chorda tympani by galvanism is an excessively painful thing?—I should say that it was.

4042. Do you think that there is any evidence that the chorda tympani is a sensory nerve at all?—It is called a motor nerve as coming from the facial, but at the same time all motor nerves are found to have a large amount of sensation, reflex it is true it may be called, but still causing a great deal of pain when manipulated.

4043. But is it not a matter of fact that in looking up any work of authority on the subject, the chorda tympani would be spoken of with extreme doubt as a sensory nerve, if not its sensibility denied altogether?—Some may do so, but the professor to whom we allude is very distinct in his opinions as to motor nerves not being destitute of sensation.

4044. Has it not been made out as one of the most remarkable discoveries that have taken place in recent years, that the effect of the irritation of the chorda tympani is direct upon the secretory power of the glands? You are doubless acquainted with Ludwig's experiments?—Certainly, to irritate the chorda tympani causes an increase of secretion.

4045. Now the nerve having been discovered to have that very definite and very remarkable function, it might probably tend to increase any doubt that exists in your mind as to whether it is a sensory nerve at all ?—I believe that it is a motor nerve; but that motor nerves have pain just as other structures may have pain in which sensory nerves run; that is, the sensory nerves may be in some way connected with it. Whether the sensation is reflex or not I cannot say, but I would consider that galvanizing the chorda tympani, like galvanizing the anterior roots of the spinal nerves would cause a certain amount of pain.

4046. But you do not think it would cause more pain than galvanizing the anterior roots of the spinal nerve. You have no particular reason for thinking so ?—I have no particular reason to think so. It is a subject on which there can be many opinions.

4047. Then did I rightly understand you to say that this operation could not be carried out under the influence of any anæsthetic ?—No; at least if you understood me to say so, you have misunderstood me. I spoke of the operation, but I did not limit the operation to an irritation of the chorda tympani nerve by any means.

4048. I wish to get at this, whether this is an experiment that can be performed under the influence of anæsthetics or not; we are now speaking of anæsthetics of any kind ?—I have never seen it performed under anæsthetics. Opium would probably act prejudicially, and I cannot speak for chloroform. I have never seen the thing proved; and it is very possible that anæsthetics would have a bad action upon the secretory power of the glands; that is to say, that they would slightly alter the condition in some way or other.

4049. You have never seen Ludwig's experiment, for example, performed on an animal under the influence of opium ?—No, I have never seen it.

4050. Now, taking this experiment altogether, do you put it forward as one of the most terrible and cruel which you wish to bring before us ?—Not one of the most terrible and cruel, but a cruel experiment, and more especially cruel when performed by way of acquiring facilities in operating; as I have seen it done several times.

4051. Suppose the experiment were performed, as it was originally performed, for the purpose of determining two most important points of physiology, would you then think it an experiment to be condemned ?—No, it is a most valuable experiment, but, once proved, to repeat it is criminal I consider.

4052. I think I understood you to speak of the establishment of a biliary fistula as a very cruel experiment ?—I mentioned such as a cruel experiment performed under curari.

4053. Only under the condition of being performed under curari ?—It would not be a cruel experiment under anæsthetics thoroughly applied.

4054. I presume you have seen animals in which an experiment of that kind has been performed a few days after the operation ?—I never paid any particular attention to them.

4055. When the canula was in the duct ?—I never paid particular attention to that.

4056. You cannot tell us then, whether under these circumstances, the animal seemed perfectly comfortable and devoid of pain, or not ?—As a mere opinion, I would not suppose for a minute that it was destitute of pain. I have seen animals in all stages remaining after operations, and I was never in the slightest doubt, nor any one about me, but what they were suffering pain, although they were not continually giving evidence of it.

4057. Do you think it possible then that a dog on which that operation had been performed should, within 48 hours (as an outside limit) after the operation, run about, exhibit all its ordinary signs of health, feed with freedom, and in fact exhibit no signs of any remarkable disturbance ?—Yes.

4058. And during this time, as is necessary for the performance of the experiment, the canula is in ?—The canula is in; but if you will kindly permit me to notice, you are going on to another experiment different from the one that I spoke of. I spoke of an operation that lasted six, seven, or eight hours, a temporary operation—that is, there was no attempt at healing up the fistula. You are speaking of permanent fistula, if I understand you correctly.

4059. I have asked you those questions because I understood you to say that a canula placed within the gall duct must give rise to the same kind of intense agony as we very well know is caused in the human being by the passage of gall stones along the bile duct ?—I said I was not aware that any human being had any experience of the pain caused in tying the gall duct; all that they had experience of was in cases where gall stones pass down the bile duct, in which cases they describe the pain as being intense. And I say from their account we can form an opinion what would be the dissecting out of the bile duct from the structures with which it is included in the ligaments, cutting out and bringing it outside. It would be a most painful operation.

4060. I assume with you that the agony caused by the passage of gall stones down the bile duct is of the most acute possible character in the human subject; but then I take it if a dog with a canula in its bile duct exhibits no such manifestations of pain, shows not even any signs of inconvenience, it cannot be that the presence of that canula can give rise to pain in any respect comparable with that which is produced by the passage of a gall stone along the human bile duct ?—I am of opinion that the pain in the operation which I refer to would be much more intense than a gall stone passing along the bile duct in the human subject; but that pain (following up what I think is the intention of your question) would certainly get deadened as time passed.

4061. So that it does not follow, from the fact of a dog having a canula in its bile duct or gall duct, that it necessarily suffers the same sort of pain as we suffer when a biliary calculus passes along the same passage ?—During the acute stage, if I may use that word, I am of opinion that the suffering of a dog, in the experiment referred to, that is under curari for six or eight hours after the first making of the hole in the abdomen, would be more intense than in the case of a human being. I give that as my opinion. I may be right or wrong.

4062. I understand that you have no objection whatever to experiments for the sake of investigation, if carried out under what you consider to be proper precautions and restrictions ?—That is as near as possible my answer.

4063. You do not admit the principle which is urged by some, that experimental investigation ought to be put down at all costs and hazards whatever ?—I certainly do not. I do not wish to see it put down, if sufficient restriction would be accepted by physiologists for the prevention of abuses. But if there be a strong objection on the part of physiologists to allow any restrictions to be laid upon them, then I would take total abolition as the next best thing. But in the present condition of things I would not ask for total abolition.

4064. I understand you then to say that if physiologists object to restriction of any kind, then you would like to stop them altogether ?—That would be next best thing.

4065. But if they do not object to restriction, then you would be content with such restrictions as you have suggested ?—Yes.

4066. I understand that you think that all such experiments should be conducted in a hall with a gallery, to which the public might be admitted ?—Yes.

4067. The public being restricted, as I gather from you, to some five or ten students, and possibly as many persons who might be nominated and admitted by tickets ?—Yes.

4068. Now, leaving the students aside, what sort of people do you think it would be advisable to admit by ticket ?—Any respectable person above a certain age. I gave the idea of the British Museum reading-room ticket, as explaining what I meant.

4069. You would allow to enter into this laboratory

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any such persons as are ordinarily admitted to the British Museum?—Yes.

4070. Persons entirely without knowledge of physiology, and without any preliminary training?—Yes.

4071. Do you think that persons of that kind are competent to form a judgment as to the value, to take one point first, the scientific value, of the experiments performed?—It is possible. What I mean is, that they might be so or they might not. Very probably they would have an exaggerated idea, because I find an exaggerated idea of the importance of such experiments to be much more common than the opposite.

4072. We have had before us a very grave statement, made on the authority of a public body, respecting certain experiments, and the allegations made respecting these experiments have been contradicted very decidedly on further inquiry. Now do you not think that that sort of evil (for I think it is one), would be likely to arise very extensively if persons under no such restraint, for whom there was no such guarantee, were admitted to a laboratory of this kind?—On the contrary I think that it would have the opposite effect. There is a public body (if you have not used the name I presume I must not use it), who are speaking at present of vivisectors as if they were flourishing knives and red-hot irons over their victims. I do not believe in anything of that sort; and I believe if those same people had an opportunity of going into that gallery and seeing what was done, those erroneous impressions would be removed.

4073. So that in fact, on the whole, your purpose in suggesting this regulation is to prevent misrepresentation?—To prevent misrepresentation either of one side or the other.

4074. Now as regards the bearing of the presence of an audience of that kind upon investigation, I do not suppose that most investigators care much about publicity, because they publish the results of their investigations; but many a man would feel a certain uneasiness at being looked over and inspected if he were carrying on any form of inquiry in chemistry, we will say, would he not?—Yes.

4075. I mean to say, if you were to propose to the superintendent of a chemical laboratory to admit the public into a gallery which ran round it, I suppose he might very reasonably object on account of the distraction and the impossibility of carrying on work under those circumstances?—He might very reasonably object, seeing that he has only inert bodies to practice upon; and he assumes a right over them that we do not grant men to have over sentient animals.

4076. My use of the word "reasonable" there referred merely to its being reasonable to expect that he would be disturbed by that?—I am quite of your opinion that there would be an objection on the part of vivisectors to be over-looked.

4077. And that that objection on the ground I have stated would not be altogether unreasonable. I do not mean to ask you whether there are other considerations which outweigh that?—I do not think it is reasonable, looking at the whole thing in a just manner.

4078. That is to say that you think that the advantage which might result by the prevention of carelessness or useless cruelty would outweigh the disadvantage to the investigator?—It would.

4079. It never occurred to you to suggest, did it, that some similar regulation should be carried out in the operating theatres of the great hospitals and medical schools?—No, because the people operated upon are operated upon by consent.

4080. We have heard it stated that in some places, some countries, which shall be nameless, the surgeons are far more careless about their patients, far less gentle than English surgeons are; and supposing that their example was to be followed in England you would not think of suggesting that there should be any inspection for them, I apprehend?—I am not prepared to say that; I consider that there are many abuses even in hospitals, where tuition of students is sometimes placed before the feelings of the people operated upon, and in exceptional circumstances, such as those, what you have referred to might be advisable.

4081. You would be prepared, would you, to extend a system of that kind, in case of need being shown, to our great hospitals and theatres of operation?—In case of need being shown, certainly.

4082. I understand you to say that you would not propose to give a license to persons for physiological experimentation?—I did say so. I meant by that, that if a license was to be looked upon as a restriction, without being a protection to animals, that should be laid aside.

4083. (*Mr. Erichsen.*) I think I understood you in the early part of your examination to say that this proposal of having a public theatre or area for the display of experiments upon living animals did not emanate solely from yourself, but that it embodied the opinions of a number of persons who are interested in this question?—I did say so.

4084. Would you confine the operations in such an institution as this solely to experiments on living animals, or would you extend them to the general work that goes on in a pathological laboratory?—Simply in connexion with live animals; it would have no reference to dead matter; they might bring it there, but the place was specially to be adapted for living animals.

4085. Not for the general work of a pathological laboratory, the major part of which does not consist in experiments on live animals?—As far as I understand you you mean the examination of the organs of diseased persons and so on.

4086. Yes, or histological observations, or physico-chemical observations, being carried on there?—No, it was not intended for those purposes, though there would be no objection to have them there.

4087. If it were so intended would not exactly the same objection hold good with regard to that as would hold good as to the proceedings of a chemical laboratory being under general supervision?—With regard to pathological or physico-chemical experiments, if experiments upon animals were not in the question, then there would be no necessity for having them in a public place at all. I am quite conscious of the awkwardness of the arrangement, if I may so speak, but I looked upon it in this way, that just as the citizens of a country each give up a certain amount of personal liberty and personal pleasure for the benefit of the whole, in the same way physiologists ought to give up a little bit of their personal liberty to mangle as much as they choose, for the purpose of having cruelty and abuses corrected in general, and the animals themselves protected.

4088. This proposal, as I understand, does not emanate from you alone, but is the embodiment of the expression of opinion of a considerable number of people, as I understand you?—Yes.

4089. Then there was another point that struck me in regard to your evidence in thinking over the working of this scheme. Might not a public, uneducated in physiological research and observation as the majority would be, mistake reflex movements in a dead animal for voluntary movements indicative of pain in an animal that they might still believe to be alive?—It is quite possible.

4090. We were told the other day by Dr. Anthony that after a knacker had killed a horse, even though that horse's head were cut off, if he proceeded an hour or two afterwards to flay that animal, the reflex movements were so powerful that they might break the knacker's arm unless special precautions were taken. Now if the public in such a gallery were to see a great animal like a horse kicking so violently, might they not think that that animal was suffering intense agony, even though it was actually dead?—Well, I am not prepared to admit the truth of this, at least I do not understand the grounds of the experiment which gave such results.

4091. Dr. Anthony mentioned it as a fact that

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he himself had witnessed ?—I have not seen it; but it is possible that the public might witness conditions which they thought were most painful when they were nothing of the kind, just as it is probable and possible that they would see no movement at all when most intense suffering was being inflicted.

4092. But supposing that the public saw what they supposed to be evidences of intense suffering when the animal was absolutely dead, would there not be some fear of public disapprobation manifesting itself, perhaps in hissing, or in appeals to the experimentor, who was supposed to be outraging public feeling, and who was supposed to be inflicting horrible cruelty upon an animal that was known to him to be entirely beyond the reach of suffering, but that was still supposed by this public in the gallery to be a sentient creature ?—The proposal of the gallery itself removed the chance of all active interference with the operator, such as we have heard of in the Norwich Commission. The scheme proposed that a complaint might be made to the superintendent for interference if the case was considered grave by the public; the case could be made out, and if it failed there would be a warning against people interfering again. If it were justified it would be a justification of the complaint.

4093. But would it not embarrass the experimentor very much to have a series of groundless complaints made against him which he had to answer day by day. Such a thing might very possibly arise under the circumstances that I mentioned. I am speaking of reflex movements that appear to indicate pain in an animal that is dead, that is utterly incapable of sensation ?—The thing would happen now and then, as such things happen every day in every walk of life, but it would bring its own cure, and might be provided for afterwards. I should not expect that such a scheme was to be considered as correct as a commencement, but it would be a point from which to start, and to make improvements.

4094. You would not be afraid of any outward signs of disapprobation or repugnance, manifestations such as take place in popular assemblies when they are displeased at any thing that is going on before them ?—No; the thing might happen as I say at times, but we should find that many of those who appear most excited, and most ready to exaggerate would never go there. It is within my personal knowledge that many of the people who now talk so much against vivisection retain in their possession papers in which experiments are detailed, and they dare not look into them. If they could not look into a book I do not think that such sensitive individuals would go and place themselves in a gallery.

4095. But there might be individuals of a ruder nature, and less sensibility, and perhaps more likely to make outward manifestations ?—The arrangements of the place would not, I presume, allow people to behave as they chose. In the British Museum it is possible that any one could make a demonstration, but the regulations of the place I think would provide that it should not be repeated.

4096. Then there was another proposition that you made in connexion with this scheme, and it was this, that pathological experiments upon animals should be performed there, and that the animals should be open to the inspection of the public after the performance of these experiments ?—That they should be kept in places open to their inspection.

4097. Do not you think that that would entirely vitiate the experiment, that with regard to an experiment made for instance upon the inoculation of sheep pox, or any of these experiments that have been made under government supervision or direction in this country, the result must be entirely vitiated by the public going to visit these animals whilst in a state of disease?—By no means whatever.

4098. That they would disturb the animal, irritate it and frighten it ?—No.

4099. The sympathy exercised by the public towards a diseased animal might modify the results of the experiment, might it not ?—No; though I said that they should be kept in places where the public could see them, I did not mean places where the public could get hold of them or manipulate them.

4100. As this scheme emanates from a body of individuals, and not from a single individual, I was anxious to form some notion, if possible, in my own mind as to the working of this scheme if it were recommended ?—May I be allowed to make a correction. I wish to say that I have not obtained permission from any one to bring in their names in connexion with the scheme I have put forward. I had no authority when I made that statement, and I had not expected the question to be asked me. I drafted that out, and it has been submitted to many. I will only add that I am not authorised to give any names besides my own.

4101. I would ask you if you are acquainted in any way personally with the working of any physiological laboratory in London ?—I am not.

4102. You have never worked in any ?—I have never worked in any except my own.

4103. But I mean in any public laboratories in London. You know nothing of them from personal observation, or from having worked there ?—No.

4104. (*Mr. Hutton.*) I think this is a matter of the very greatest importance, and therefore I want to have a very explicit answer. I understood you to say that in Edinburgh the students are accustomed to make experiments of these kinds so often, or at least to propose them, that the public feeling among the class of students in Edinburgh would in no way prevent men who had not got their degree, who were not properly educated medical men, making this class of experiments ?—I should say it would not.

4105. By your own experience you could testify that that state of feeling did prevail amongst a large class of students in Edinburgh ?—It was felt that if anything could be proved or bettered by an experiment the experiment should be performed without any more ado.

4106. And that by students ?—That by students, certainly.

4107. Now you have expressed in letters that have appeared in the public press the opinion that anæsthetics on the whole have been rather curses than benefits to animals ?—I have.

4108. Will you give me the grounds of that opinion, as briefly as you can ?—Principally because, as I have explained in those letters (which I have put in before the Commission), the public have generally supposed that anæsthetics were used, and they did not feel called upon to make any demonstration to save animals from pain, and while the animals were suffering pain all the time the public really thought that nothing of the kind was going on, and consequently anæsthetics had served more to lull the public than the animals. Those are nearly my words. And the reasons given why anæsthetics were not so much used as they were supposed to be, were first, that anæsthetics if given to animals in many cases bring about a fatal result before the experiment can be concluded,—if given thoroughly, that is to say; in the second place, that anæsthetics cannot very well be given unless a special assistant is there for the purpose; and that these two things together cause so much annoyance to the experimentor that he does not take the trouble of thoroughly anæsthetising the animals. This leaves out of sight that great class of experiments where anæsthetics would interfere with the true result of the experiment; and these are very numerous.

4109. Now can you give us an example of what you would call the most painful class of experiments made without anæsthetics, and necessarily made without anæsthetics, from any authorised and written record. I have had a paper given me to-day in which an experiment is described which is made by M. Paul Bert (I think it is one that has been referred to before), a very eminent scientific man in Paris, who I see has just received a prize of 20,000 francs for some scientific inquiry; and if this be an authentic account of the experiment, I think it would be very material for

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the Commission to hear the general features of it?—I have also that account.

4110. (*Chairman.*) Is it from a published medical journal?—It is from a published medical work; it is a translation of Paul Bert's observations on a curarised dog, translated from "Archives de Physiologie," Volume II. of 1869, page 650. I may say that I myself have assisted at experiments somewhat similar to this.

4111. Have you got the French here?—Yes* (*producing it, and handing it to Mr. Huxley*). The translation is as follows:—"Under the skin of a "middle-sized, vigorous dog, six centigrammes of "curare in the solid state were inserted, and six "centigrammes of the same substance dissolved in "six grammes of water, were also injected. A few "minutes afterwards the animal staggered on its fore-"paws, walking on the tips of the toes; he then fell, "and presented all the signs of poisoning by curare; "he defecated, urinated, gave out a large quantity "of viscid saliva, and wept very much (*pleurait* "*abondamment*). I then opened the windpipe, and "inserted the nozzle of a bellows, to which an inter-"mittent movement was given by a machine worked "by water. (The machine fitted by M. Gréhant "in the laboratory of M. Claude Bernard at the "Collège of France). In this way an artificial "respiration of a very regular kind was kept up. "The pneumo-gastric nerve of the right side was "then laid bare and tied, and the same was done "with the sciatic nerve of the same side. After "some minutes the sciatic nerve could no longer "make the muscles contract. During ten hours the "action of the nerves and the condition of the pupils "were examined from hour to hour, and the following "facts ascertained, which it will be sufficient to state "once for all. The pupil of the right side was con-"tracted, that of the left side much dilated. The "galvanisation of the pneumo-gastric and sympathetic "of either side in the neck caused dilatation of the "pupil and protrusion of the eyeball. This effect was "most marked on the right side, but real also on the "left side. The galvanisation of the pneumogastric "nerve of either side completely arrested the action of "the heart. The iris of the healthy side (left side) "became contracted when light was allowed to fall upon "it. Moreover, and this is the point to which I par-"ticularly wish to call attention, galvanic excitation "of the central end of the tied sciatic nerve, of the "same in the median nerve, and of the same in "the infra-orbital nerve, caused a contraction of the "bladder, which (the sphincter of the urethra being "paralysed) made itself evident by the emission of a "small quantity of urine. This effect can only have "been obtained through the intervention of the "sensory nerves of animal life. Neither the pneumo-"gastric nor the sympathetic nerves in the neck, nor "the splanchnic nerves, gave any such result. After "ten hours of artificial respiration, the animal became "three or four degrees colder (temperature of the air "12°). Next morning it was dead, although the "artificial respiration machine still worked; perhaps "during the night there might have been some "irregularity. The urine found in the bladder, as "well as that previously collected, contained a great "deal of sugar. The viscid saliva, secreted in great "abundance, gave a slight but manifest yellow pre-"cipitate with the blue test fluid. This is the first "time, I believe, that sugar has been found in the "saliva in the course of induced diabetes. But let us "return to the action of the sensory nerves on the "contraction of the bladder. This result is interesting "in itself, since it explains certain known relations "between the bladder and the sensory nerves; such as "the desire to urinate which follows the sharp sensa-"tion of external cold on entering a cold bath, or a "very hot one. Moreover, in following out this "question, it will be possible to solve certain very in-"teresting physiological questions. On the one hand, "the intra-medullary origin and the place of exit "of the motor nerves of the bladder might be easily "be determined. We shall then discover by what "channel those centripetal disorders come; for ex-"ample, from the infra-orbital nerve passing along "the whole length of the spinal cord; if they belong "exclusively to the white columns, or if they spread "into the grey substance. The experiments of "Budge and Schiff might be thus usefully rendered "complete. However, this may be, here is a dog "under curare, upon whom, during ten hours, the "action of the pneumo-gastric and sympathetic "nerves remains intact; whether these be excited "directly by electricity or put in action by their natural excitor, that is, reflex action." I have written the following observations of my own:—"Remarks "on the above in plain English. In this experiment "a dog was first rendered helpless and incapable of "any movement, even of breathing, which function "was performed by a machine blowing through a "hole in its wind-pipe. All this time, however, 'its "'intelligence, its sensitiveness, and its will, re-"'mained intact; a condition accompanied by the "'most atrocious sufferings that the imagination "'of man can conceive' (*vide* Claude Bernard, in "Revue des Deux Mondes, 1st September 1864, "pp. 173, 182, 183, &c.) In this condition the side "of the face, the side of the neck, the side of the "fore-leg, the interior of the belly and the hip, were "dissected out, in order to lay bare respectively the "sciatic, the splanchnics, the median, the pneumo-"gastric and sympathetic, and the infra-orbital "nerves. These were excited by electricity for ten "consecutive hours; during which time the animal "must have suffered unutterable torment, unrelieved "even by a cry. The crowning discovery made, to "which the experimentor calls special attention, "being that, at times, when thus tortured, *it* "*urinated!* The inquisitors then left for their "homes, leaving the tortured victim alone with the "clanking engine working upon it, till death came "in the silence of the night, and set the sufferer free."

4112. (*Mr. Hutton.*) Have not you a little exaggerated the effect by stating that "these were excited by electricity ten consecutive hours." You meant to say with intervals; that the sufferings went on for ten consecutive hours, but not the sufferings as increased by electricity?—It was tried every hour during that time; but as there were a great number of nerves to excite and get at, it would probably be a consecutive operation throughout.

4113. You do not mean that the electricity was going on during the whole time exciting the sensitive nerves?—No; two nerves would not be excited at the time; after one was excited they would go to another.

4114. The French phrase is *pendant dix heures on examine d'heure en heure,*" that is to say, that hour after hour they were examined?—Yes, but in the interval some other nerve was being tried.

4115. Now your attention has been called very much, has it not, to the action of curari?—Very much; and I believe I was the first who, in this country, brought up the question of its abuse.

4116. You do not believe it to be an anæsthetic in any degree whatever, do you?—I am as positive on that point as I am on any point of physiology; and I have any amount of material to prove it from the best physiologists in the world.

4117. Did I rightly understand you to say that Claude Bernard's opinion is, that, so far from being an anæsthetic, it rather increases the sufferings of the creatures to which it is given?—I have no documentary evidence to state exactly the amount, but I can say that he expresses himself as if it was something horrible beyond all conception to be under curari.

4118. (*Mr. Forster.*) Where does he say that?—I have got the book here (*handing it in*), the *Revue des Deux Mondes* of the 1st of September 1864.

4119. (*Mr. Hutton.*) Can you put in any cases in which the effect on man has been described minutely?—I can. I have a translation here of the most im-

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* Appendix III., § 4.

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portant parts from that article, to which I have just alluded; it is a classic article on the subject. And I have also added some of those other experiments that you refer to. Liouville gives a very graphic description of a case where an overdose had been given to a patient, and artificial respiration had to be kept up till after he recovered. "The patient then related," says Liouville (*Bulletin Général Thérapeutique*, 1865, page 404), "all he had felt, the preservation of his in-" telligence, the annihilation of all power of movement, " of which he gave a clear account, witnessing all that " went on around him, without being able to take any " part in it, the fears freely expressed by some young " assistants present being by no means re-assuring to " him." That is one case, and Bernard also mentions at page 1117 of the *Revue Scientifique*, for 1874–5, that, " We have the accounts of individuals who had been " inoculated with curare, but to a degree which had " not stopped the motions of respiration, and conse-" quently permitted the individuals to return to life, " I mean to movement. These have then been able " to relate that during their paralysis they had never-" theless been fully conscious of their existence, and " of all the impressions which excited their senses." There is another account in the "*Revue Scientifique*," I cannot give the page, but I give the work (it is a very large work) in which he mentions that a patient had been operated upon under curari and suffered "*douleurs atroces*." I can repeat such opinions nearly word for word, from three or four of the greatest experimentors, in fact they are nearly all agreed upon it abroad.

4120. Has your attention been called to some experiments by Mr. Yule, in which he supposed that he had shown that, as regards the frog at least, curari was an anæsthetic?—It has.

4121. And what is your view of the success of those experiments?—My view of them, if I may be allowed to express it so strongly, is, that they are utterly unworthy of credit, as far as they were published in "*Nature*." I have heard reports about pinching, which do not appear in "*Nature*" at all, and which would alter the consideration of the question; but as they stand in "*Nature*," they are utterly unworthy of credit, and they are opposed in the most distinct manner to experiments of which I am prepared to read the account, performed by the greatest authorities to this subject, and that I myself have witnessed performed by them.

4122. Will you read us that?—It is said that in curari it is simply a question of pulling the animal's hind leg out, and because it does not draw it back, it is argued that it has lost volition and consciousness; and that is the point I will address myself to first, although the first three lines of the article I refer to, contain a grave mis-statement of the fact.

4123. You mean to say a grave mis-statement of the facts as you have understood them?—As all the scientific world, up to the time of the publication of my *exposé*, have understood them. Now I have here Claude Bernard's classic experiment, as it is considered, on the frog, and I am able to give the same opinion from Köllicker, it is the one, I think, that Mr. Yule refers to, and which is explained also in the work from which I have read before, as well as in the *Revue des Deux Mondes*.

4124. (*Mr. Huxley.*) I imagine you are going to read Bernard and Köllicker's evidence on which they arrived at their conclusions somewhere about 1863 or 1864, and it so happens that I experimented myself at the same time, and arrived at the same conclusions?—Principally. I have instances to give in 1875.

4125. (*Mr. Hutton.*) What is the date of this experiment?—It is a constant date. I saw it performed a year and a half ago several times.

4126. But the date of the book out of which yoy quote?—The book I quote from has this classic article in it. I will give you now the very latest published opinion on the subject that of Professor Vulpian.

4127. (*Chairman.*) It is the latest opinion of authority with which you are acquainted, I understand you?—Yes.

4128. (*Sir John Karslake.*) The latest which you recognise as an authority?—Yes.

4129. (*Chairman.*) And what is the date of it?—He states in "*Lecons sur l'appareil locomoteur*," at page 60, published in 1875, that curare does not act on the nerves of sensation. "Curare does not " act on the sensory nerves, or at least does not " abolish their function," and at page 661, "Curare " which abolishes voluntary motion in no way annuls " sensation, at least in ordinary doses." I hope it is understood that I am speaking of physiological doses for physiological experiments. In a large dose it destroys life as well as sensation.

4130. (*Mr. Hutton.*) Then it is your impression that any dose which really abolishes sensation also abolishes life?—That the one would come close on to the other; as near as has been ascertained. The action of curari follows a regular course over the body.

4131. Will you give us shortly your reasons for thinking Mr. Yule's experiments with regard to curari to be illusory?—I shall do so by pointing out that his best reason is simply this, that in a curarized frog, of which the hind legs are not curarized, on laying it down and extending out its hind leg it does not draw it up; he argues from that, I consider wrongly, that the leg being, as he considers, in an uncomfortable position, if it had consciousness and volition, it would draw it up; and because it does not draw it up from an uncomfortable position he says it has lost consciousness and volition.

4132. I want to ask you just one question as to the difference between narcotics and anæsthetics. I asked a very high scientific witness the other day whether it was true or not that narcotics only dulled pain, while anæsthetics removed it, and the answer was that it was directly contrary to the fact. Now have you any authority for saying that there is a distinct difference in kind, that narcotics only dull pain, while anæsthetics remove it?—I have given already an opinion on that point, in the words of Professor Claude Bernard.

4133 (*Chairman.*) At what date was that opinion given?—It was in 1870, "*Revue des Cours Scientifique*," volume 6, page 446:—"Morphia is not an " anæsthetic, but a narcotic (*stupéfiant*). When it has " taken effect on a dog he does not seek to escape; " he has lost the knowledge of where he is; he no " longer notices his master. Nevertheless sensibility " persists, for if we pinch the animal he moves and " cries. At the same time you see that morphia " plunges dogs into a state of immobility, which " permits us to place them on an experimenting " trough without tying or muzzling them." That gives a very good idea, and I have seen the thing done.

4134. (*Mr. Hutton.*) We have been told by some of the witnesses that by far the most painful of their experiments have been on the origin of tuberculosis, but also that they were by far the most important. Is there any difference amongst high authorities as to the importance of those experiments, as far as you know?—I have had my attention called to this in consequence of an article in the "Times" and elsewhere, and I can only say that according to the very latest information that we possess from the Continent, the experiments and the conclusions which are most relied on,—those of M. Villemin,—have been completely upset at the late Medical Congress in Brussels, by Professor Crocq. We have the whole thing in the "*Revue Scientifique*" for the 2nd of October 1875, where it appears that all the conclusions were upset, and special attention directed to the contradictory opinions now existing amongst nearly all experimentors on that subject.

4135. (*Chairman.*) Do you adduce that for the purpose of showing that no further experiments are desirable?—On the contrary, I simply bring it in to show that those experiments which were supposed to have justified all experiments on animals on this question have been upset by the very latest investigations upon the subject. Both Professor Crocq's

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reasoning and a letter subsequently from M. Villemin are to be found in the last two numbers of the "*Revue Scientifique;*" the one came last week and the other the week before. M. Villemin in his letter acknowledges that he has modified his ideas, and that the whole field still lies open for work.

4136. (*Mr. Hutton.*) Do you believe that the experiments of M. Villemin have been for the same object as those of Dr. Burdon-Sanderson, or have you no knowledge on that subject?—I have no special knowledge of Dr. Burdon-Sanderson's experiments. They seem to have gone about it in different ways, one by mechanical irritation, and the other by specific inoculation.

4137. (*Mr. Huxley.*) Now as respects this discussion between M. Villemin and the Reporter of the Brussels Congress, M. Crocq, I take it the whole difference between them lies in this, that M. Villemin says that tuberculosis is a specific disease requiring a certain kind of poisonous matter for its production, whereas M. Crocq denies that proposition?—Those two things are included certainly in it.

4138. But, on the other hand, M. Crocq has not denied, but on the contrary he has considerably extended, the statements made by M. Villemin as to the different modes of treatment by which tuberculosis can be artificially induced?—Professor Crocq has contradicted the ideas of M. Villemin, that true tubercle (if we know what that really is) cannot be produced except by being inoculated in a certain way, and that it is not hereditary. That was another part of M. Villemin's theory, which he has given up apparently.

4139. Nevertheless there is a vast body of experimental evidence common to the two, which is of great value in its bearing on the ætiology of tuberculosis?—It all proves what it is *not.* Nearly all the experiments have proved what tuberculosis is not; they have only contradicted each other. I am not aware that they have come really to a better knowledge of what tubercle is than they had 30 years ago.

4140. Now, I should like to ask you a question or two about this case which you have put into our hands—Paul Bert's observations on a curarized dog. In the first place, as I read this, it is stated that the various nerves here mentioned were all tied at starting?—It only mentions that two nerves were tied at starting.

4141. At the commencement of the experiment?—That the pneumo-gastric and sympathetic and sciatic nerves were tied at starting; it was a means of lifting them up out of the dissection.

4142. Then I do not find that M. Bert himself gives any opinion at all as to whether the animal was capable of sensation or not?—He says that those effects were through the sensory nerves of animal life.

4143. But that is an entirely different statement; as for example, supposing a frog to have its head off and I irritated its foot, the frog's leg will kick, and that result will be produced because a certain impression has been conveyed through the nerves of sensory life; and I might state that with perfect consistency, though I believed that the animal did not feel?—Yes.

4144. So that M. Bert's statement to the same effect comes to nothing?—No; we are nearly all agreed that the frog gives appearances in that way, which it is not correct to reason from in the higher animals.

4145. I will go back to the case which has been brought before your notice by Mr. Erichsen. He told you, on very good authority, that a horse, for some time after death, if the knacker begins to skin it, kicks out. Now it would be perfectly proper to describe that, even though the horse's head is cut off, as an effect produced upon the nervous system, through the nerves of sensory life, would it not?—I am not sure that it would. I am not prepared to pass an opinion on that matter. First, I am not certain of the thing; I have never seen anything like it; and again, there is this, that we can get it from the muscles themselves, apart from the nerves, as far as I can understand the question.

4146. Have you never seen in the case of an animal of the degree of organization of a horse, such as a dog, that after the destruction of the cerebral hemispheres, or such injury as entirely destroys consciousness, that animal exhibits reflex action, brought about by influences carried to its nerve-centres, through the nerves of sensory life?—I cannot fix on any occasion where I would be certain of that fact. I have seen such displays, which I put down to the action of the muscles themselves. We know that the muscles can act apart from the nerve-centres.

4147. Have you ever seen a case of paraplegia?—Yes, I think I have.

4148. Under those circumstances, where the lower half of a man's body is paralysed, and he is utterly unable to feel any stimulus applied to his feet, is it not a matter of notoriety that if you tickle the soles of the feet they will be drawn up?—Yes.

4149. And does not that impression conveyed to the soles of the feet act through the sensory nerves?—I am not quite prepared to give an opinion upon that point.

4150. You are not prepared to give an opinion upon one of the fundamental facts in every book of physiology?—I might give an opinion, but so hasty an opinion on a subject that requires thinking about, and explanation, might be wrong and inadvisable.

4151. Would you allow me to speak of this as a statement made probably by every physiological teacher in the world, and which has been made for the last 30 years in every physiological book. I am not putting to you a new and exceptional case, but one familiar to everybody who has paid the least attention to physiology?—I can say that it is considered to be owing to a reflex action going up to the cord and coming back from the cord again.

4152. And going up to the cord through the sensory nerves?—That is the explanation of the thing that is given.

4153. Have you any doubt that that is a correct explanation?—Well, I must say that I always felt a certain amount of doubt about the explanation of reflex action; that is merely as an opinion. I do not mean to say that I am prepared with another theory to explain it.

4154. Of course if these elementary, what are commonly considered, truths of physiology are doubted, it is no use our discussing the question any further; but supposing that anyone thought himself justified in believing that under these circumstances a person might exhibit movement, the result of stimuli to the sensory nerves, and yet not feel them, he then might think that all these things that have taken place in this curarized dog might have taken place without sensation, supposing he had reason to believe that curari is an anæsthetic?—I do not suppose for a minute that Paul Bert considered that curari was an anæsthetic.

4155. But if he had so considered it, then he might have used the phraseology which is here used, and yet have believed that the animal was not, in the proper sense of the word, sensitive?—To acknowledge that that is possible is all that I can do; it is scarcely within the bounds of probability.

4156. That, of course, is a question of opinion?—Yes.

4157. Now as to the scientific results of this experiment; what I find is, what to me is exceedingly interesting and very remarkable, that is to say, by this experiment it is established that the extremities of all the ordinary motor nerve are absolutely paralysed, but that nevertheless under a full dose of curari those branches of the sympathetic which terminate in muscles, and those branches of the pneumo-gastric which terminate in muscles, are not affected?—I do not suppose for a minute that he proves anything of the sort. It is pretty well known that while curari produces an effect on the sciatic nerve, the same dose does not affect the pneumo-gastric nerve, but that a greater dose will certainly affect the pneumo-gastric,

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and that it then fails to affect the heart when galvanized. He proves nothing of the sort there.

4158. All I am speaking of is the statement by Paul Bert which you have given us, "However this "may be, here is a dog under curari, upon whom "during ten hours the action of the pneumo-gastric "and sympathetic nerves remains intact." Now, either M. Bert was telling the truth in writing the account of his experiment, in which case his statement is accurate; or he was not, in which case you cannot build anything upon it. Do I undersand you to deny that his statement is true, that during ten hours the action of the sympathetic and pneumo-gastric nerves remained intact?—It is quite true, but to stop there would give an erroneous idea. It simply means that certain nerves are paralysed before certain other nerves, and it is certain that the sciatic is always paralysed by curari, and by a smaller dose and quicker, as a question of time, than the pneumo-gastric; the pneumo-gastric requires a much larger dose to affect it than the sciatic.

4159. Then that shows that there is something very singular about the action of curari, and something which by no means could have been imagined *à priori*. You are not acquainted with any *à priori* reason for supposing that the pneumo-gastric or sympathetic should not be affected when others are?—On the contrary that was the puzzle, why the pneumo-gastric having motor fibres should not be paralysed as quickly as the sciatic; and it was only by increasing the dose that they discovered that the curari goes step by step from one particular nerve to another, some being more sensitive than others to its influence, and that it always follows that same course; that this thing, which at first was a puzzle, has been now clearly explained by increasing the dose, explained by the comparative effects of curari over the system.

4160. But that is only an explanation of the statement, which I asked your assent to, as to whether it is not a very singular thing, and a thing not to be expected *à priori*, that curari should behave in one way in regard to the action of the pneumo-gastric, and in another way in regard to the action of the sciatic?—It is curious, but it has been proved to be true, that it has different effects.

4161. (*Mr. Forster.*) I understand that that effect had been proved before this experiment?—Yes, before that experiment; but it is a valuable thing that one point, that the pneumo-gastric should still retain its power, showing that the dose of curari was small, and we have had clear proof of what the effect of that dose was when small. It is a valuable point, as illustrating what I formerly said.

4162. (*Mr. Huxley.*) That is one point on which we have had evidence in this experiment. A second point is mentioned by M. Bert himself:—"This is "the first time, I believe, that sugar has been found "in the saliva in the course of induced diabetes." Supposing that to be true, it would be an important fact, would it not?—I am not aware that that has any particular importance about it. It is a little phenomenon and might not happen again, and it is not the one to which he calls special attention.

4163. Thirdly, supposing these experiments to be correct, they throw what to me is a very remarkable light upon the manner in which the action of the bladder is affected by the sensory nerves. I do not ask you whether you think that the experiments establish what they were supposed to do or not, but whether, supposing they do so establish them, that result is important or not?—It is a result that has been known, I think, to the most common observers of little things. That is what he himself refers to. To pinch the skin in many persons causes them to urinate. Upon causing intense pain the bladder contracted and gave forth a certain amount of urine. It is the smallness of the result compared to the immensity of the pain that causes this experiment to be so peculiar.

4164. That is your opinion; but I have indicated three several points which might very fairly be regarded, I do not say justly, but might very fairly be regarded, by M. Bert himself, as results of considerable importance. It may be that you do not think them to importance, but I do not think you will deny that it may be a fair thing for M. Bert, who made the experiment, to consider them of importance?—I did not consider M. Bert's ideas in the matter, but the ideas of the British people on reading the experiment, and whether they would like to see the same thing commonly done in England, as it certainly will be if no stop is put to such practices.

4165. I understand you to say that you have taken your own ideas of this subject, and not paid any attention to those of a person who probably, without any disrespect to you, is as competent to form a judgment on the matter as yourself?—I have translated, as nearly as possible, his own words. I have made comments upon the experiment for those who cannot understand technical language, but there is the translation for every one to compare it for himself with the original. No liberties have been taken with the translation. It is for people to consider whether it be correct.

4166. Then do you consider that the statement, "that these were excited by electricity for ten "consecutive hours"—which, I think, to most persons' minds would mean that the irritation went on for ten consecutive hours—would convey the sense of the original?—Yes. I could not suppose that people would think that there was a special battery for every separate nerve, but that as there were a number of nerves to be gone over, and from the observations which he mentions afterwards, it would take up all the time of the operator, and that all the time the animal was suffering unutterable torment.

4167. (*Mr. Hutton.*) You mean that some one nerve was always being excited?—Some nerve was being excited, or some little operation was going on.

4168. (*Mr. Huxley.*) All that we have here is evidence to show that there are five nerves which are irritated; the irritation of those five nerves, as you must be perfectly aware, from having experimented in these matters, would probably take about two or three seconds apiece?—I never saw an experimenter going in such a hurry about it, I must say.

4169. Do you think it actually needs longer, suppose we say two or three minutes apiece?—The ten hours during which it lay in this state included, of course, many things that would take away the attention from one nerve or another. For instance, laying bare the splanchnic nerve, and laying open the whole of the abdomen, and clearing out the intestines in order to find these nerves, which is very difficult, even in the dead subject. That operation was included in the 10 hours, but the unutterable torment was in no way lessened because the other nerves were dropped in order to get at the splanchnic nerve in the abdomen.

4170. You do not think that writing for the general public, and knowing the sort of feeling existing on these matters, a statement which could not possibly have been misunderstood would probably have been more useful to them?—I consider that when I put forward a statement with the translation along with it, I do all that is required to enable people to form an opinion of the thing for themselves.

4171. And you think that you have fairly stated what would in the general opinion of physiologists be the scientific value of this experiment when you say, "the crowning discovery made, to which the experi"menter calls special attention, being that at times "when thus tortured it urinated"?—I think that it is a fair mode of stating it, and that having consideration, not for Paul Bert and physiologists, but for people at large, and for animals in particular, if, as the friend of the dog, I were to express it ten times stronger with any number of curses on the head of the man who did it, I should be justified.

4172. You quote incidentally a passage from an article by M. Claude Bernard in 1864 to the effect

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that "all this time its intelligence, its sensitiveness, "and its will remained intact." You did not think it necessary to state here in view of the popular feeling on the matter that although this was the view of M. Claude Bernard, and a number of other persons at that time, yet, that of late, whether rightly or wrongly, other competent persons had argued that that was not the case?—I did not think it necessary to enter into any such explanations; I gave Claude Bernard's authority, the book where it was found, the date, and everything. I give the words merely, within inverted commas.

4173. Is it not rather a serious thing when you are dealing with persons who do not know anything about a matter, and who have no access to the channels of information, when you know that there are two views of a case current on good authority, to put only one before them?—No; when the party who is most interested has not got the power to put any view, that is to say, the animal which is suffering.

4174. My question was rather whether, without reference to the animal or anything else, except the interests of common justice, it is not well to state a disputed case in such a manner as shall not lead people to believe that it is not disputed. I suppose you would agree to that general principle?—I am afraid that I would have to make an addition with regard to the motives of those who dispute it, that would not perhaps be in place here.

4175. Then I think that just now you cited Vulpian's opinion, as a recent expression of his unqualified belief that curari does not in any way act as an anæsthetic?—In ordinary doses.

4176. It turns out, from the wording of that opinion, in the first place, that Vulpian said that in his judgment it did not affect the functions of the sensory nerves. I think those were the words?—That it did not abolish their functions.

4177. And as I have just now shown (and as I understand you have assented in a general way in your answers to my questions), a man may quite well believe that the functions of the sensory nerves are not interfered with and yet may believe anything he pleases about the abolition of conciousness. So that, taking that part of M. Vulpian's opinion, it does not count either way?—The real fact is, that it is scarcely within the bounds of possibility that any man in France would suppose that curari is an anæsthetic, or has any idea of the sort. I may give that as as clear an opinion as I can; I say it is hardly within the bounds of possibility that a man could have such an idea.

4178. We seem to be driving at two points, but I am endeavouring to show, or rather to put to you this, that this recent expression of opinion by M. Vulpian, if we take the first part of it, does not properly bear the same construction as the expression of opinion by M. Claude Bernard ten years ago. All M. Vulpian says is, that he believes curari does not abolish the functions of the sensory nerves?—Yes.

4179. And in that he does not mean that it may not abolish the functions of the nerve centres?—He explains further on that it abolishes motion.

4180. The second point is that curari, in moderate doses, does not affect sensation?—In ordinary doses, he meant physiological doses; that it does not annul the function of the nerves.

4181. Will you read again your note of Vulpian's opinion?—"Curari does not act on the sensory nerves, "or at least does not abolish their function." Then, "Curari which abolishes voluntary motion, in no way "annuls sensation, at least in ordinary doses." I may couple with that, if you will allow me, that it is held by many that it vastly increases the power of sensation; only I have no documentary evidence for that. I have been told so by distinguished physiologists, but as I have not documentary evidence to give, it is well to leave it out.

4182. M. Vulpian says, that with ordinary doses it does not abolish sensation?—Yes.

4183. It is perfectly clear from his giving his opinion in those words that he leaves the question open, whether in large doses it does so or not?—Very clearly, and I would just give the same opinion as he does, that if you increase it a little bit, it destroys, not only the sensory nerves, but the whole individual.

4184. Then, did I rightly understand you to say, in reply to an inquiry by Mr. Hutton, that sensibility is not destroyed by narcotics?—Such was the opinion that I read, and I gave it as my own too, that it was not destroyed. The opinion was given that sense was dulled, the consciousness of what had been suffered; the animal did not try to run away, but sensibility still persisted, as shown by the animal crying at the moment that it was pinched.

4185. Surely we are at cross purposes; because if ever you have happened to take an overdose of opium, I presume you found your own sensibility considerably vanishing?—If any one pinched my skin, then, I think I should feel that, but half a minute afterwards I might have no remembrance of it.

4186. Would the phenomena which you would manifest be different from those which would be exhibited if you had injured your spinal cord in the neck; I mean so far as irritation of the surface is concerned, below the point of injury?—I should be quite conscious of the narcotism; I should feel the whole pain of the pinching, which I should not do in the other case.

4187. May I ask what is the evidence of that?—I read out the opinion just now of Bernard.

4188. But the evidence is what I ask for?—The evidence is very easy to find; if any one takes a dose of morphia or opium, he will feel the pain where he pinches himself, but as the remembrance of it goes away he feels almost unconsciousness of the pain he endured.

4189. Then do I rightly understand you to say that even in the deepest narcotism produced by morphia or opium there is no real insensibility to pain?—Physiologically speaking that is the opinion; Persons under morphia or opium feel the pain, but they soon forget it.

4190. Then it is simply because they forget it, you mean?—They forget the pang immediately afterwards.

4191. It is because this is so entirely new to me that I want to question you about it. Then with an excessive dose of morphia, for example, a person is nevertheless competent to feel, and does feel, any injury done to him, but forgets it directly; is that what you say?—I only speak in reference to an ordinary physiological dose of morphia. There is a certain stage in narcotism in which we cease to exist.

4192. I am speaking of a period long before ceasing to exist; I only mean when a person is in a condition of heavy narcotism?—And I am only speaking in reference to narcotism in physiological experiments; I believe that the question is turning on that point.

4193. It comes to this: let us suppose that in some cases a dog has opium administered, and when he exhibits certain signs of narcotism he is operated upon; now do I rightly understand you to say that if a human being were in a parallel condition of narcotism, he would feel all that was done to him?—He would feel it.

4194. But what is the evidence of that. I mean as apart from opinion?—The evidence is given in the paper before you. I read that the dog was narcotised; it was laid up, and the professor said, "You "see that this animal is in a state of immobility, "which allowed me to lay it up here on the experi- "menting trough; it does not seek to run away, "but whenever I pinch it you see how it cries; it "is sensible to pain, although in this semi-un- "conscious state it does not seek to run away."

4195. What you mean to say is, that there are some conditions of narcosis in which an animal is insensible to small impressions, and sensible to large ones?—I have been limiting my remarks to

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the condition which is induced in experiments on animals.

4196. (*Mr. Forster.*) Looking at the account of this experiment the animal seems to have been ten hours under treatment, and then the operators left. Do you imagine that it was necessary to the success of the experiment that the animal should be left alive and not killed ?—I have seen the thing often done.

4197. But in this particular case ?—It simply meant this, that next day they could work upon it; if they kept the dog it might still have life enough in it to see whether its nerves would still act. No doubt they intended to continue their operations the next day, but death came in the interval, and thwarted them.

4198. You stated last time that you had been Demonstrator of Anatomy at Edinburgh; what does that mean, if you please ?—It means that during the day I taught the students practical anatomy, and in the evening I delivered a course of lectures on regional anatomy to the class.

4199. (*Lord Winmarleigh.*) As you do not object to all vivisection, I presume you would not object to its being still further tested on animals what is the real power of curari ?—I certainly would object to opening up a question which has been so thoroughly ascertained before, more especially to tyros beginning questions which dozens of the best physiologists in the world have laid aside as the most complete thing in physiology.

4200. (*Sir John Karslake.*) When did you cease to be connected with the anatomical school at Edinburgh ?—In 1872.

4201. How long had you been residing there, and demonstrating in that school ?—I had been demonstrating in that school for two sessions of the previous year. I had been a student in that school two years previous to that.

4202. Two years previous to 1870 ?—In 1868–9, 1869–70, 1870–71. It was in 1870–71 that I was demonstrating.

4203. Do I rightly understand that your knowledge of what was being done in Edinburgh is confined to the years from about 1867–8 to about 1872 ?—With regard to my personal knowledge of what is being done in Edinburgh at present, of course as I am fixed in London I have no personal knowledge in the way of visiting the place. But I have the knowledge, for instance, of Professor Rutherford's experiments, which have been published in a letter in the newspapers, which are altogether opposite to what occurred five years ago when such experiments were not yet introduced. Those experiments show the progress that experimental physiology is making in this country.

4204. The knowledge which you have obtained personally as to what has been going on at Edinburgh was between the years 1867 and 1872, or about that time ?—It was up to two years ago; although I was not a student, I was in Edinburgh teaching separately afterwards.

4205. Did you yourself attend any of the experiments, or the vivisections that took place in the laboratories for the benefit of classes ?—I did not. The experiments that were performed at the class there were very small things indeed, and I went to the continent for the purpose of getting a knowledge of the practice which I was unable to procure during my student life, and having obtained that, I would no longer practise it.

4206. You yourself never demonstrated upon the living animal before any class at all in Edinburgh ?—Never. I taught anatomy, not physiology

4207. Now let me ask you this question, you said, as I understood you, that medical students were in the habit of practising vivisection in private ?—I did say so.

4208. In what year was that ?—All the time that I was there.

4209. From 1867 to 1872 ?—All that time; and they often, of course, amongst each other spoke of such experiments. One would say, "I tried so and so." I took no special note of the thing at the time as to who said this, only there was the general knowledge which no one for a moment would doubt, that students when they had an opportunity, and thought that it would be advisable to make a vivisection, proceeded to do it.

4210. Then, as I understand you to say, the students who attended the lectures in the physiological laboratories were in the habit, according to your view, of practising these experiments themselves ?—It was not confined to students who attended physiological laboratories. In the University of Edinburgh practical physiology is not a compulsory class, so that all the students do not take it.

4211. I do not see that that in any way affects the question that I am putting, which is, that, according to your statement, a student or students who did attend these classes, by themselves practised such experiments upon living animals ?—We had few experiments, in fact scarcely any experiments, to attend when I was there, in any class; but the students whom I referred to were medical students, who, perhaps, never attended a physiological laboratory at all. I wish to point out that it is not a compulsory class in Edinburgh, and therefore they did not take it.

4212. Now then, the experiments which they practised according to you, were experiments which they thought necessary for illustrating some truth or other ?—No; it was considered, perhaps, enough to get an animal and open it in order to see its heart beat.

4213. Can you give the Commission anything which will enable them to ascertain when or where any particular person did perform these experiments in private ?—I cannot give the Commission any such information, and it is almost impossible for the Commission to obtain information of that sort.

4214. Can you recall to mind the name of any single person who spoke to you and stated that he was in the habit of performing these experiments ?—I can.

4215. Have you any objection to state the name ?—I would rather not state any names; it would have rather a tendency to deter witnesses from coming forward, to bring in names.

4216. Did the gentlemen who spoke to you, and whose name is in your mind now, make the least secret of what he was doing ?—He told me (I may tell his name, but it would not help the Commission in any way) that he and another student had experimented on a number of cats; I think the number he said was 15.

4217. For what purpose ?—I did not get that out of him at all. It was simply in connexion with my own theory that he said so in advising me to do it.

4218. Now can you give us the time, the year, or about the year in which it was made ?—Yes, the statement was made on the day that I read my Dissertation before the Royal Medical Society, but the gentleman who told me is dead. I can give you the precise date, I think. The date on which that young gentleman told me that was the 25th of March 1870.

4219. He told you that he and another gentleman, whose name he mentioned, made experiments on cats ?—Yes.

4220. Did he tell you for what purpose ?—I think he told me, but I cannot remember now what it was for.

4221. Did he tell you where they were made ?—Certainly, in his own rooms.

4222. Did he describe the nature of the experiments ?—I know this, that he opened up the thorax and the abdomen.

4223. Did he tell you, or had you the curiosity to ask, for any explanation of the object of his making these experiments ?—I do not think I inquired the object; he told me that he had made those experiments, and he told me what he had observed during those experiments, which might be made to bear upon my theory of respiration, which was being read and discussed that night. Every one was coming to me

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and saying, "Why, you ought to demonstrate this "on animals," and it was one of the students, whose name I can particularly recall at present, who told me this in connexion with himself.

4224. Have you any objection to state his name?—I have no objection, but he is dead now.

4225. Can you tell us the name of the other gentleman who was assisting him in these experiments, and whether he is living, or not?—I did not know him; it was a French name, and I did not pick it up.

4226. Is there any other instance you can give me to bear out your statement that this was a practice at Edinburgh?—I could give no particular instance.

4227. And you cannot give me the name of anybody who asserts that he has practised it in his own room, besides the gentleman whom you have mentioned?—Not at this present moment. Things of the sort have been said to me dozens of times, but I have retained no special remembrance of what was said in the matter.

4228. Then not having retained any special remembrance of what did occur you cannot assist the Commission in any way, by enabling them to ascertain from the persons themselves, when and where, and with what object they performed these vivisections, if they did perform them?—I am sorry to say you may just as well inquire into Freemasonry. I only mean in saying that to show the difficulty that there would be in getting such information.

4229. (*Mr. Erichsen.*) In reference to this experiment of M. Paul Bert's, I see that it is stated here that the pneumo-gastric nerve was laid bare and tied, and that the sciatic nerve on the same side was laid bare and tied. Now, supposing the animal was sensitive, do you think that the tying of the nerve would be productive of much pain?—I should suppose that it was productive of much pain to do so.

4230. Have you ever seen during an operation a surgeon accidentally include in the ligature with which he was tying an artery, a twig of a nerve not bigger than a thread, and the intense agony occasioned by that?—I have never seen the thing, but I have been (as every student is) carefully cautioned against it on account of the great agony which it causes.

4231. (*Chairman.*) You have been asked a great deal about this experiment; is there any conceivable doubt that unless the animal was anæsthetised it was an excruciating experiment?—There can be no doubt in my opinion.

4232. And that that excruciating pain lasted over ten hours?—That this excruciating pain was continually kept irritated during the 10 hours and afterwards continued until death came.

4233. The period of the night during which the animal lingered being unknown?—Being unknown. It is a common thing, I may tell you, if a dog is not injured so that it cannot live, simply to keep the machine working all night, and in the morning it probably would have recovered from curari and can be set free; curari is eliminated by the kidneys.

4234. In the record given of this experiment the question of whether the pain was taken away or not diminished depends upon what may have been the action of curari in that respect?—It depends upon the action of curari.

4235. This experiment was performed in 1869, was it not?—Yes.

4236. As far as you know the opinion in 1869 was that curari did not act as an anæsthetic?—In the laboratory referred to I have heard it dozens of times pointed out while the animal was lying on the table, "Look at that animal; you would suppose "it does not feel, but it feels doubly." Three years after the date of that experiment such was the opinion of the people in that laboratory.

4237. If then there be any doubt at all stated in later years as to whether curari is or is not either an anæsthetic or a mitigator of pain, it can have no reference whatever, so far as their character for humanity is concerned, to the performance of these operations?—It can have no reference whatever to it. In that laboratory they have still that opinion according to the latest accounts from Claude Bernard.

4238. Is there anything else you wish to say before we terminate your evidence?—There was one thing which I thought I might perhaps explain to Professor Huxley in connexion with the writing of a thesis. On the last occasion, in answer to a question from him, whether it was not the case that students at Edinburgh on taking their degree often take for their thesis a subject requiring original research, I said it was "unfortunately true." What I meant by that is this, that I consider that this demand for a thesis is one of the most fruitful causes of vivisection. We know that when a man who has nothing to say is asked to speak he generally talks nonsense. In the same way when many medical men who have not got an idea are required to form a thesis containing such original ideas, not having them themselves, they go to look for them in the quivering bodies of animals. It is due, I believe, to the fact that in this country the great majority of medical men do not require to go through an university or read a thesis, that we have not so much vivisection going on here as they have on the continent, where every medical man has to do that; in France for instance, every medical man has to go through the University of France and present a thesis containing original remarks. It is that fact alone I believe which gives the continent a pre-eminence over England in vivisection, and it was with that in view that I said "unfortunately true." It has been really an incentive, causing people to make those experiments, and the consequence of that has been that such ideas being forced from individuals, as it were, have led to a very unsatisfactory state of things; that, not being men who had a special aptitude for the thing, but looking upon it as a necessity, they have gone into it; and the result has been what people might have expected. And I believe that this same feeling, that there is a very great deal more vivisection going on than is warranted is partaken of even by the very best of experimenters. I came across, to-day only, a remark by Claude Bernard on this same question, than whom there is not one better able to speak; there is not a physiologist whom I think a greater one, or one for whom I have a greater respect. He considers that there are not strict enough measures put upon vivisection; that people rush to do it and get the roughest results, and the consequence is that the whole of physiological science is so encumbered with a mass of contradictions, that people are apt to say that there is nothing found out in physiology.

4239. (*Lord Winmarleigh.*) Does Claude Bernard recommend any particular measures by which its unnecessary practice may be restricted?—He recommends none, but he deplores the excessive use of curari (I can say that much), in this same work, the *Revue Scientifique*, although he was the original introducer of it.

4240. But he does not go further than that?—No, he simply deplores that it is used too much.

4241. (*Mr. Forster.*) Does his statement that you have just been referring to lead you to suppose that he was arguing against the frequency of vivisection on account of the pain given to animals?—I understood simply that he meant that vivisection was entered upon by people who were not capable of doing it, and he deplores that there was nothing to prevent them.

4242. (*Mr. Huxley.*) I suppose that there is no very large proportion of the theses published in the University of Edinburgh which relates to experimental physiological subjects?—I understand there is a certain number.

4243. But in relation to the whole number of theses, it is a small proportion, is it not?—It is a small proportion.

4244. (*Chairman.*) Have you anything further to say?—I wish to say one or two things, with your permission. I wish to say a word about physiologists in consequence of the exaggerations that have been put out about them. From my experience, physio-

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logists are not the monsters that some people picture them to be. Some of them are bad enough, but my own personal experience has taught me, and on the continent especially, that amongst physiologists you have the kindest, most gentlemanly, and amiable people, but that the glaring fault that was to be noticed was an entire want of feeling, that their feelings were entirely blunted; they seemed to be unconscious that they were inflicting the greatest pain; they did not do it intentionally. I wish to do justice to them by pointing out that it is a question of blunted feelings, not of a gloating in torture as has been represented. Then there is the question of performing experiments on animals before classes. If I may give an opinion upon that point, I absolutely deny that there is any truth in the proposition that it is necessary, or advisable, to fix physiological facts on the minds of students by exhibiting demonstrations of the same on the suffering bodies of animals. Demonstrations are certainly preferable to mere explanations or diagrams, but such may almost always be demonstrated by mechanism or by arrangements which inflict no pain. As an instance, I may say that I learnt as much about the nature of the circulation by the application of the sphygmograph to the wrist of the class servitor as ever I learnt afterwards in a public class on the continent by experiments on dozens of tortured animals, whose living blood vessels were tied on to the tubes connected with barometers for the same purpose. A chemist would be considered a lunatic requiring restraint who blew out the windows of his laboratory in order to impress on the minds of his students the effects of bringing a naked light into a room where a dangerous escape of gas existed; the customary experiment for this end being to mix the gases and explode them in a soda-water bottle. For the same reason, I would consider the mind of a physiologist perverted, and necessitating restraint, who with the sphygmograph at his disposal dared to torture a sentient animal in order to demonstrate the nature of the circulation upon it. I am, therefore, of opinion that demonstrations in class by means of painful experiments on animals, should be absolutely forbidden by law under heavy penalties. In cases where demonstrations are advisable they might be effected by means of mechanisms, into the devising of which the minds of ingenious physiologists would be directed when it was no longer possible to cause torture to animals for that purpose. Such a law would have a double advantage; it would protect animals from being tortured, and it would protect the minds of young men from the blunting and demoralising effects of vivisection. Then further I should like to express an opinion on the question of limiting permission to perform painful experiments in connexion with new discoveries only. I think this would be unsatisfactory, because while theoretically it would seem to be a safe precaution, practically it would be of no use whatever, as any experiments might be made to bear the desired interpretation.

4245. Have you anything further to say?—I think not.

The witness withdrew.

Adjourned to Monday next at 2 o'clock.

Monday, 1st November 1875.

PRESENT:

THE RIGHT HON. VISCOUNT CARDWELL IN THE CHAIR.

The Right Hon. LORD WINMARLEIGH.
Sir J. B. KARSLAKE, M.P.
THOMAS HENRY HUXLEY, Esq.
JOHN ERIC ERICHSEN, Esq.
RICHARD HOLT HUTTON, Esq.
N. BAKER, Esq., Secretary.

Mr. ROBERT SAWYER called in and examined.

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4246. (*Chairman.*) You are a member of the committee, I think, of the Royal Society for the Prevention of Cruelty to Animals?—I am.

4247. You are also a member of the profession to which Sir John Karslake and I, like yourself, have the honour to belong?—I am.

4248. You have been vouched here as a witness present at a lecture that was given by Professor Ferrier in Finsbury Circus?—Yes.

4249. And it is stated that you left the room with some others in consequence of the pain with which you saw the laughter of the young people. No imputation has been made, as I understand it, upon Professor Ferrier of actual cruelty, since it is understood that the experiments took place under complete anæsthesia, but it is stated in a paper put in by the secretary of the Royal Society for the Prevention of Cruelty to Animals, in answer to question 1569, that "popular lectures were being given of a sensational "character by a learned professor who made his "audience laugh over the grim behaviour of his "unfortunate victim"?—I do not think that that is the view that I took of it. I went to that lecture, because I heard that it was to be delivered, and I thought from what I heard that perhaps some experiments or operations would be performed, and I went there for the purpose of watching those operations or experiments, if there were any; I provided myself with a paper and pencil for the purpose of taking notes of anything that I saw there. As it turned out, no experiments or operations were performed, and therefore I did not take notes of anything that I saw; but I sat there through the whole of the lecture, and I took a position which was very close to the lecturer, intentionally, for the purpose of watching any operation. I was close enough to hear anything that was said, and I sat out the lecture. I do not suppose that the witness who said what has been read to me intended to express that I had left the room during the lecture, because, if so, he was certainly mistaken.

4250. He says, in answer to question 1576, "Three "gentlemen were with me who were members of the "committee of the society, and two of them left the "room at once in disgust"?—I did not leave the room at all.

4251. You were present during the whole time?—Yes.

4252. What have you to say to us with regard to what passed?—May I be understood first to say that I have not tendered my evidence, but that I am quite willing to answer any questions your Lordship chooses to ask me. I went there for the purpose which I have already stated. It appeared to me to be a very interesting lecture. Dr. Ferrier expressed himself extremely well, and he pointed out several things which were certainly very interesting. At the time that he was pointing out some particular part of the brain, and explaining that when that part of the brain was touched the animal that was operated upon performed certain movements with its legs and so on, he used an expression which certainly attracted my attention very much. He looked up to the people

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who were sitting above (we were in a sort of theatre, and I was at the bottom of it close to him), and he used this expression, as far as I remember, "I am "afraid to say how many cats I have operated upon "in the investigation of this." That is my recollection of it. It certainly struck me that Dr. Ferrier had brought himself into that state of mind that he considered that he was justified, no doubt he thought properly justified, in performing a number of operations with regard to that particular fact which he was pointing out to us at that time. When he made that observation there was a titter, and certainly an expression of feeling amongst the people, which I was disappointed to see. I was pained at the fact that the people did seem to look upon that assertion of Dr. Ferrier's with considerable levity. I really have no more to say than that, excepting to answer anything that your Lordship likes to ask me.

4253. Did it not appear from the lecture, whether these operations had taken place under complete anæsthesia?—I understood Dr. Ferrier to say, at the beginning of the lecture, when he was first explaining to us how the operations were performed, that after removing a portion of the skull the brain might be cut and touched for the purpose of exciting certain movements, and so on, but that the animal felt no pain.

4254. Then that impression would be derived, I presume, by that portion of the audience who paid proper attention to the lecturer?—I should think so.

4255. So that those who laughed when this remark was made may presumably have been under the impression that all these animals had been deprived of sensibility before the operation, in its nature painful, had been performed upon them?—In the opinion of Dr. Ferrier, certainly.

4256. Do you consider, summing it all up, that there was anything in what Dr. Ferrier said which implied an indifference to pain on his own part, or a desire to promote indifference to pain in the minds of his audience?—No, I should think not. The thing that occurred to me, rather, was that Dr. Ferrier would consider himself justified, in an investigation of that particular phenomenon, in taking away the lives of any number of animals, but that he would say, I suppose, in justification of that, that the animal was not suffering pain.

4257. (*Lord Winmarleigh.*) Did I understand you rightly to say that Dr. Ferrier said that the animal, after the operation you have mentioned, suffered no pain?—I understood so.

4258. Did I rightly understand you to say that Dr. Ferrier had, in the course of his lecture, stated in any way that the operation had been under anæsthetics?—I understood him to say, at the beginning of the lecture rather, when he was describing how part of the skull was cut off so as to expose the brain, that it had been done under anæsthetics.

4259. (*Sir J. B. Karslake.*) Might I ask you who attended with you on this occasion of the Royal Society?—Mr. Colam was there, Mr. Reid Thompson, and Mr. Thomas Allen of the South Wales circuit.

4260. Did you all leave in company?—I think not.

4261. Is it new to you that any one of the gentlemen who attended with you upon that occasion, by appointment probably, left before the lecture was over?—It is new to me.

4262. At all events, you were not one of those who left?—I was not.

4263. Were the others sitting near you?—No.

4264. About how many persons were present, do you think, as far as you could judge?—I have never thought of that at all, but I should think it must have been 50 or 60, or 70; a considerable number, certainly.

4265. And of different ages, varying from quite young people to people of considerable age?—Yes.

4266. And the thing that occurred to you during the lecture, and which you regretted to find existing, was a certain levity of conduct on the part of the audience, when the professor spoke of the number of animals that had been sacrificed for the purpose of obtaining these particular results?—Yes.

4267. Did he refer, do you recollect, particularly to a monkey, and show a diagram of a monkey?—There was no diagram of a monkey; there was a diagram of a monkey's brain, and diagrams upon the wall of a number of brains of different animals.

4268. Was the whole argument which the professor was using that, notwithstanding a complete absence of sensory feeling on the part of the animal, it gave indications by its movements of feeling which he knew that the animal did not feel?—I can hardly say that. He said that it gave indications of feeling, certainly.

4269. Was not the argument this, that although there had been measures taken which prevented the animal from feeling, yet on touching particular parts of the brain the actions of the animal would be such as would exist if the animal had feeling?—Yes, I think so.

4270. Had you attended other lectures at the same place?—None at all.

4271. And you went with the expectation of seeing animals actually operated upon in the room?—I can hardly say with the expectation, I went there for the purpose of taking notes of any operation that might be performed.

4272. With the exception of what you say was the conduct of some part of the audience in tittering when they heard that many animals had been sacrificed in the establishing of a particular theory, was there anything in the lecture that struck you as not worthy of a lecture from an able and scientific man?—Not at all; except this, that Dr. Ferrier, I thought, showed indifference to the fact that he was taking away the lives of a great number of animals.

4273. Did you collect from him that he had performed a single experiment which was unnecessary for the purpose of establishing the fact, if it was important to establish that fact?—No, not at all.

4274. (*Mr. Huxley.*) You were sitting quite close to the lecturer, you say?—Yes, as near as I am to you, I think.

4275. May I ask, as a matter of fact, if he laughed himself?—No; he did not laugh himself, but he smiled; he looked up in this way (*describing it*), and smiled at the people at the top circle of the theatre, so to speak.

4276. Would you say that this, which I find in the answer to question 1588, was a fair description of Dr. Ferrier's course of action, that "the skill of "the lecturer was used to cover the grim character "of the experiment by his humour?"—It did not strike me so. I believe there were two lectures; I was only there the first day; it was the 1st of February that I was there.

4277. (*Sir J. B. Karslake.*) Were you a party to that report which was read to us?—I am no party to any paper which is directed to your Commission; I do not remember ever having seen one. It may be that that commencement, the direction "To the Royal Commission," may have been appended to some paper to which I was a party, but I am not aware that any instructions were given to Mr. Colam to bring a paper to the Commissioners at all. It may be that at some committee meeting at which I was not present, some direction of that sort was given. I think it likely that I have not attended any committee meetings since the Royal Commission was issued.

4278. Referring to the words which have been read just now by Mr. Huxley, let me ask you, were you any party to a report in which that language occurs?—I think not. I must be taken to be a party to a report which was made by the sub-committee, and which was read in my presence, but I have no recollection of such expressions as that being used in the report.

4279. (*Mr. Huxley.*) Were you a member of a committee which was appointed by the Royal Society for the Prevention of Cruelty to Animals upon the

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presentation of a memorial against vivisection?—I was a member of the committee.

4280. Then according to the statement that appears in our printed evidence, one would be led to conclude that that is the very report of which you have just heard a portion read. I will read you the answer of the Secretary in order to show that. At question 1567 he is asked, "Is there anything that you desire to bring before the Commission which has not been included in the questions already put to you?" and his answer is, "I do not know that there is, excepting that I was rather anxious first of all that you should understand that the Royal Society for the Prevention of Cruelty to Animals is not the society established for the total abolition of experiments; and secondly I was instructed when I came here to read to you what the committee which was appointed by the Royal Society for the Prevention of Cruelty to Animals did upon the presentation of a memorial against vivisection, in reference to the examination of the allegations of the memorial, and the deliberations upon its prayer." Then the next question is, "That is a document prepared under the sanction of the society, and you are speaking their language when you read it to us?" And the answer is, "Yes." And then the witness proceeds to read the document, a portion of which you have just heard. Now, whether you were a member of the committee here referred to or not, you have not had the passage which has been read under your consideration?—I was a member of the committee which we call the sub-committee on vivisection.

4281. But whatever committee you were a member of, that particular passage which has been read was not submitted to you?—I do not remember that "grim" part.

4282. (*Mr. Hutton.*) In answer to question 1576, Mr. Colam said of Professor Ferrier, "I have heard him say that the animals appeared to be in intense suffering, and then joke about the stupidity of the animal, especially if the animal happened to be a monkey, giving humorous descriptions of its behaviour, so much so that the whole lecture place was really like a comic scene;" and on that I put a question to him afterwards, at Question 1606, asking him to explain his statement, that the lecturer joked about the stupidity of the monkey, and asking him this: "Those movements were involuntary, were they not?" And he said, "Of course they would be, because he showed that the animal had no volition at all." "And therefore," I said, "it was meant as sort of joke upon the apparent expressions of the monkey?" To which he replied "Yes, the loss of intelligence of the animal." Now do you remember anything of that, and did it strike you that there was levity in that description of the loss of intelligence on the part of the monkey?—I do not think that that occurred at the lecture that I was present at at all.

4283. You think it must have been at the other lecture?—I think so.

The witness withdrew.

Mr. Joseph Lister, M.B., called in and examined.

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4284. (*Chairman.*) You are Professor of Clinical Surgery in the University of Edinburgh?—Yes.

4285. And Surgeon in Ordinary to the Queen in Scotland, and a Fellow of the Royal Society?—Yes.

4286. I think I may also ask you if you are a son-in-law to the late Mr. Syme?—Yes.

4287. And you have not only had a good deal of experience yourself in the matter which has been referred to us, but much of that experience was in the society of that very eminent surgeon?—He was well aware of what I did in these matters.

4288. He performed, I think, some experiments himself?—Yes, very important ones.

4289. Was that before the time of anæsthetics?—Yes.

4290. Can you tell us, in the first place, whether I am right in saying of him that he was very desirous to promote humanity in the treatment of animals in this particular as far as possible?—Undoubtedly he was.

4291. You have had then very considerable experience upon this subject?—Yes, I have.

4292. Now at the present time, what is your opinion about the necessity or otherwise of experiments on living animals for the purpose of ensuring the progress of physiological and surgical and medical knowledge?—I consider them one of the most important means that we possess for those objects.

4293. Can most of the experiments that are necessary in your judgment be performed under chloroform?—In the case of the higher animals the majority can undoubtedly.

4294. Of those that cannot, is it a frequent case that the more painful parts of the experiments can be performed under chloroform?—It is certainly. The operation may be performed anæsthetically, and the results witnessed afterwards.

4295. Where you perform the operation itself under chloroform and then retain the animal alive for witnessing the consequent results, is it usually a very painful period to the animal while you are witnessing those consequent results?—In my own experience it has as a rule not been so. The wound itself has as a rule been free from inflammation and from pain.

4296. Then is there a residuum of experiments which are in your opinion necessary and which must be painful?—I cannot doubt that there must be such.

4297. Are those experiments, do you think, of protracted agony on the part of the animal?—Not as a rule; but it is impossible for me to foresee what the requirements of physiological science may demand.

4297*a*. If such experiments were to be held to be necessary, and were to be performed at all, I need scarcely ask you if they ought to be performed by the most highly educated and most competent persons?—Undoubtedly.

4298. And whether for science itself that is not as essential as it is for humanity?—Certainly.

4299. Now I gather from what you have said, that if the Legislature should interfere for the purpose of proposing any check upon such experiments, it would be a first consideration with you whether that legislative measure interfered with any experiments which you thought necessary for the progress of physiological knowledge?—Yes. I should feel it a very doubtful matter whether any legislative interference could take place that would not hamper in an undesirable manner such investigations.

4300. But your objection would probably depend upon whether or not such impediments were thrown in the way?—Undoubtedly if there were no impediment thrown in the way of investigation, I could have no objection to legislation.

4301. If there were no impediments thrown in the way, that is to say, of the prosecution by the most scientific and educated persons of such experiments as might be absolutely necessary for the progress of physiological science?—It is sometimes difficult to define who are the most scientific and competent persons. Some private practitioner, for instance, who might be an obscure person to the profession generally might yet perform experiments that might be very valuable.

4302. That is a conceivable case; but within your knowledge has it ever been an actual case?—It is not in my knowledge that any such experiments have been performed except by persons of scientific competence.

4303. Then probably all the persons of scientific competence are at this moment known by name to yourself, for example?—Well, any man who has

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had a thoroughly good education as a medical student I should conceive to be competent. I should think it would be a mistake to throw any barrier in the way of his performing investigations that he desired. If I may be so egotistical as to refer to myself, my first experiments were performed when I was altogether unknown as a person of any sort of reputation whatever. When I first took in hand to teach others, I felt that there were some points on which I desired more information than I could get satisfactorily from any book, and my first experiments were performed with the object of preparing myself for teaching, while I was not a person of recognised scientific attainments.

4304. You performed them with the knowlege and under the sanction of Professor Syme, did you not?—The first that I did were not with his knowledge at all.

4305. (*Mr. Erichsen.*) But you had already been appointed a teacher?—I had been appointed a teacher.

4306. (*Chairman.*) And, speaking generally, you have said that your experiments were performed with the knowledge and under the sanction of Professor Syme?—Not in the least by his direction. I only mean that I used to converse with him on what I was doing, and that what I did had his warm approval. I said that with reference to his character, which I do not wish to be misunderstood. I meant that while he very much disapproved of anything like cruelty or the infliction of needless suffering, he, on the other hand, highly valued such inquiries when legitimately conducted.

4307. (*Lord Winmarleigh.*) Have your practical experiments on living animals been attended by a numerous audience?—No; they have been entirely for my own private investigation.

4308. You have not lectured on the subject?—I have shown a demonstration before now to a class, many years ago.

4309. But you have not been in the habit of doing it?—No.

4310. Have you been in the habit of attending much at lectures or experiments performed by others?—Not since I was a student.

4311. In your experience, have you ever witnessed the effect that the experiments have had upon the young men who attended the lectures?—Not except in my own courses of lectures, which were then delivered to small classes. The influence was, so far as it went, highly beneficial.

4312. In what way beneficial?—Because a demonstration of an actual object gives information which description cannot give.

4313. But you have never seen any experiment or demonstration which has caused any laughter on the part of those who attended it?—No. I remember the flood of light thrown upon Dr. Sharpey's physiological class by his demonstration to us of the effects upon the circulation of the division of the sympathetic nerve in the neck; how intensely interested we all were, without, certainly, the shadow of levity on the part of any one of us.

4314. Should you believe that amongst the attendants on the lectures that are demonstrated by experiments it is the habit to show any levity?—I have never heard of any such thing.

4315. (*Sir J. B. Karslake.*) I think you said that in your opinion experiments on living animals will necessarily be carried on extensively hereafter, and many of them of a character which you cannot suggest at the present time?—Yes.

4316. That must remain to be determined by the necessity pointed out by experience?—Yes.

4317. How long ago is it since you yourself lectured, and had to perform experiments or to show living animals to your class?—My last experiment was performed about six months ago.

4318. I refer, not to private research, but how long is it since you have demonstrated to a class by means of animals under chloroform or other anæsthetics?—Well, it dates back to the beginning of my lecturing, before 1860.

4319. All your lectures have been given since chloroform or other anæsthetics have been in use?—They have.

4320. I think I understood you to say that you have used very few experiments upon living animals for the purpose of demonstration?—Very few; it has not been much required in my department of teaching, which is that of surgery.

4321. Is there any particular branch of surgery in which you teach?—I now teach practical surgery. I used to teach the whole subject, and then, in illustration of the more fundamental parts, the pathological parts, I used to find illustrations of that kind useful.

4322. But in the teaching of surgery it is less common, is it not, than in other things?—Very much so indeed.

4323. You teach chiefly by the dead subject?—Yes, or in my case now upon the living subject.

4324. With reference to the question put to you by Lord Cardwell as to these experiments being only those which are absolutely necessary, and which are performed by competent persons; in your opinion, would persons who were competent sometimes perform these experiments, although they were unknown to fame at the time they were doing it?—I certainly think so.

4325. And there would be a difficulty in excluding those persons?—That is my feeling. I do not think that anybody who was not connected with a school would be likely to perform such experiments, unless he had in him such an ardent love of the pursuit of physiological truth as it would be very undesirable to check by any legislative enactments.

4326. (*Mr. Huxley.*) Take the case of another science. Suppose that there was any legislative interference with the freedom of persons unknown to fame to perform some experiments in chemistry, we should then in all human probability have entirely lost Davy, I presume, and Faraday?—Precisely so.

4327. And there probably will be in your judgment no great reason for believing that there is such a great difference between the physiological sciences and others that the same consequences might not follow there?—That is very much my feeling.

4328. I should like to ask you what is your own feeling about causing pain to animals without some clear and definite purpose in view?—I think there ought always to be some clear and definite purpose in view. At the same time I must confess that if we consider the sort of objects for which pain is commonly inflicted upon the brute creation, as for example in the castration of male animals merely for the purpose of making them more easily fattened and their flesh better fitted for food, or, in the case of the horse, more patient and docile drudges, (these operations are exceedingly painful and often involve considerable suffering afterwards in the way of inflammation, and the objects in view are not essential to man's existence, they merely promote man's luxury and man's comfort;) if for such purposes painful operations are performed, without any objection,—in fact it is universally admitted that such procedures are legitimate,—as compared with objects like those, I confess that to myself even a demonstration of a physiological truth, as distinguished from an investigation, seems to me to rank higher. I believe that this matter ought to be considered, not so much with regard to the actual pain caused to the animal, as with reference to the effect produced upon those who inflict it, or those who witness the infliction. I think that for students to have unnecessary infliction of pain practised before them would be highly demoralising; and therefore from that point of view I think that such demonstrations should be performed, where they possibly can be, under anæsthetics; but that not so much for the purpose of avoiding pain to the animals as for the sake of avoiding a demoralising influence on the students. For after all I suppose that the actual pain caused by all the vivisectional experiments that take place throughout Great

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Britain in a year would not amount to the pain that results from by the winging of pheasants in a single day's shooting on the battue system. There it is considered legitimate; whether it is so or not is another matter; but as compared with what is ordinarily regarded as legitimate, the actual pain caused by vivisection is in truth insignificant. It comes to be of very great importance in its influence on the mind of the student, but I think that is the really essential point of view.

4329. I presume from what you have said that you yourself would think it morally wrong to inflict needless pain upon any animal; that is to say, if you had the alternative of being able to administer chloroform in a given experiment or not, you would think it your duty to administer it?—If I could attain the object of the experiment as advantageously; but not otherwise. This is a subject on which I have thought and felt a great deal; and I may say that I have often performed these experiments at very great sacrifice to my own feelings; but I have believed that the consideration of the probability of good being gained to the human race was a higher consideration than my own personal feeling in such a matter.

4330. Your acquaintance with men following experimental physiology will doubtless be extensive; what is your judgment about the feeling on those matters of the persons whose opinions you have reason to know?—My own impression is that medical men are the most humane class of the community. Their whole lives consist in acts of beneficence which bring out their best feelings; and I think that there is no class of mankind who would be so likely to feel tenderly for a lower animal as medical men. Such a man knows what the suffering is and appreciates it no doubt more truly in some respects, and would sometimes think less of the suffering than some other persons might; for example, he would know that a frog would not suffer like a man; but still with reference to the suffering, such as it is, I believe nobody would feel so tenderly as a medical man.

4331. I presume that if anyone told you that he would not administer an anæsthetic except for his own convenience, you would not approve of that sentiment?—Certainly I should not; at the same time I would not push the thing too far. For instance, suppose a small operation had to be performed on a horse's neck, involving a small incision and not much pain consequently, it is a very laborious thing to put a horse under chloroform, and I would not judge a man too hardly who under such circumstances said "On the whole I think we need not give chloroform here." I think we must not draw too strict a line of that kind.

4332. That was not the case which I had in my mind; I had in my mind the case of a severe experiment where chloroform could be administered easily?—Undoubtedly, I should think it a duty under such circumstances.

4333. (*Lord Winmarleigh.*) Would the administration of chloroform occupy much time?—It occupies a considerable time in the horse; a large animal like that takes a long time.

4334. And in the dog or cat, or rabbit, does it take long?—No, not very long.

4335. How many minutes should you say it would take to put a rabbit under chloroform?—Four minutes, perhaps.

4336. And a dog?—I could not say exactly; not any very long time.

4337. It would not be a material interruption, then, to any experiment on those animals?—No.

4338. (*Mr. Huxley.*) I understand that you consider that experimental physiological research has borne, and is likely to bear, very important practical results. Could you enforce that by anything within your own knowledge?—Well, perhaps the best way would be to take my own actual experience. The experiments to which I have referred before with reference, in the first instance, to teaching a class, had the effect of giving me a kind of pathological information, without which I believe I could not by possibility have made my way in the subject of antiseptics; and that subject, I believe, is becoming recognised as one of considerable practical importance. I have often felt that without the basis which I derived from experiments on the lower animals, I should never have been able to thread my way through the very perplexing and apparently contradictory facts which I met with in the first instance.

4339. I understood you to attach a very great value to actual demonstration of facts in the teaching of science, as far as can possibly be carried?—Yes.

4340. I suppose that in the teaching of surgery, which is your own special subject, although it is quite possible by the aid of clear descriptions and figures to give a student as good an idea of an injury as you can give of most things by mere talking about them, you would look upon the state of mind of the man who had heard an injury described, and that of one who had actually seen it, as very different?—Very different indeed.

4341. It is hard to describe the difference, and yet do not we know that it is, I might say, almost infinitely different?—It is inconceivable what a great difference there is.

4342. And as a matter of fact, in your branch of medical science, as in all others, the examining bodies very justly insist upon every student having had at least the opportunity of becoming pratically acquainted with the facts of disease?—Yes.

4343. And I presume that if any suggestion were made that such opportunity of occular inspection should be omitted, that would be regarded as a very retrograde step in medical education?—Undoubtedly; the tendency now is to insist more and more upon everything, as far as possible, being made practical and demonstrative in the way of instruction.

4344. Have you thought of a proposal that has been made to prevent all painful experimentation upon animals, that is to say, experimentation except under chloroform or anæsthetics, except in the case of those persons who may be provided with a license?—I have thought of that, and I confess that it has not seemed to me that any such legislation is called for. I am not aware of any temptation existing in this country to experimentation of that kind by persons who are not competent for it.

4345. In fact you think that it would be superfluous legislation?—I think it would be superfluous; and not only so, but personally I should feel it a sort of blot upon our profession, as implying that such legislation was necessary, which I believe it in this country not to be.

4346. You would feel, as you have just now expressed yourself, that a large amount of suffering is being inflicted for various purposes, and which is not meddled with, and that the only persons who inflict suffering for beneficial purposes would be meddled with if such a proposal were carried out?—They would be if that were so.

4347. Can you state to the Commission if that is the general feeling, so far as your experience goes, among members of the profession?—The few that I have talked with on the subject have, I think, that feeling.

4348. A suggestion has been made, on which I should very much like to have your opinion as a practical investigator and teacher of large experience, namely, that no experimentation of this kind should take place except in laboratories, and that the laboratories should be provided with a gallery, and that to that gallery the public should be admitted. Do you think that that suggestion is at all one which is likely to commend itself to those who are occupied in teaching or in investigation?—I should feel a very strong objection to it myself. In the first place (to allude to myself again), under such a rule, my experiments could not have taken place; they were not done in physiological laboratories at all; I should not have had the opportunity of doing them in any physiological laboratory. And in the second place, I think of all

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undesirable things the most undesirable is that the public should witness these things; because they would simply see what they would not understand, and would suppose that very dreadful things were taking place, when they were in reality perfectly proper; and no advantage could possibly be derived, that I can see, from the public witnessing these things.

4349. That is to say, in point of fact, very innocent things might be done which would give them the impression that very horrible things were being done; and, on the other hand, very horrible things might be done which would give them the impression that very innocent things were being done?—Undoubtedly.

4350. (*Mr. Erichsen.*) Am I correct in understanding that in all your various capacities, as practitioner of surgery and as a teacher of surgery, and also as a man who has been foremost in the advance of surgical practice and science, you have found experimentation on living animals necessary?—That has been so.

4351. And that through the medium of that experimentation, originally commenced possibly, I think I understood you to say, with other views, you were gradually led to the development of that method of treatment which is now known as the antiseptic system?—Yes.

4352. That in the pursuit of that inquiry you were led to ligature the arteries of some of the larger animals?—Yes.

4353. Am I right in thinking that it was necessary that you should discover some substance to use as a ligature which did not produce the irritation which is occasioned by an ordinary ligature?—Yes.

4354. That was an essential part of the business?—It was.

4355. And that could only be ascertained by experiments on living animals?—Yes, certainly.

4356. Had you not made experiments on brutes, you would have had to experiment on man; there was no alternative, was there?—There was no alternative.

4357. You could not discover such a thing as an antiseptic ligature, for instance the antiseptic catgut ligature, by any *à priori* reasoning?—It must be tested by experiment.

4358. In ligaturing these larger arteries in large animals you used, I believe, calves, horses, and animals of that kind?—Yes.

4359. Will you tell us whether that operation of the application of a ligature to the large arteries of a large animal is a painful process?—As it was performed with that particular object it did not involve pain after the performance of the operation, as there was really no inflammation.

4360. The animal could be anæsthetised during the operation?—During the operation it could be anæsthetised.

4361. We have been given to understand that the after effects of the application of the ligature to an artery are extremely painful, is that so in your experience or not?—It is certainly not only contrary to my experience in the lower animals, but contrary to my experience in the human subject.

4362. It is your opinion, that if the ligature is carefully applied, neither in the man nor in the animal are the after effects painful?—Yes; especially if, by antiseptic management of the wound, the occurrence of inflammation is prevented.

4363. Do you think that experiments on such animals as dogs, calves, and horses, and animals of that description, form a safe analogy to the corresponding results in man?—I must say I think they do. I have seen precisely the same effects follow in man that I have seen in the lower animals.

4364. If you apply a ligature to the artery of a dog or of a horse, and that animal is killed some days afterwards, is it the case that you would find the same appearances, as nearly as possible, as if a ligature had been applied to the artery of a man who had died some days afterwards, not in consequence of the operation, but perhaps from the effect of an injury or whatever the cause might be?—Certainly.

4365. So that there would be a close analogy between experiments upon the larger animals and upon man?—Yes.

4366. And it would be safe to deduce an inference from the close analogy that exists between the two?—I think so.

4367. You stated, I think, that you do not think that experiments on the lower animals produce hardening or demoralising effects upon students, those who witness them; that is the result, I think, you stated, of your experience in a very large medical school?—Yes, if done as any humane teacher would do them.

4368. That the mere infliction of pain, even if it were necessary that pain should be produced, would not necessarily demoralise or harden the student?—Certainly it would not if it were done under a proper feeling.

4369. I am supposing that it is done in a solemn manner, as it were, by a man perfectly competent to do it, and with every possible precaution, but yet that pain was the inevitable result?—Yes, I understood the question in that sense.

4370. I think that you are not old enough to recollect the operations on the human subject before the introduction of anæsthetics?—I saw Liston do the first operation ever performed under chloroform in London. I was then a junior student in University College.

4371. From your knowledge, you must be aware that there were most dreadful and painful scenes witnessed every week and almost every day in the operating theatres of all the large hospitals?—Yes.

4372. Do you think that the surgeons who witnessed and were participators in these scenes, the surgeons, in fact, of a past generation, and those who were educated at that period, were less tender-hearted or less humane than the surgeons of the present day, who perform all their operations under the influence of anæsthetics?—I do not. I may take, for instance, Mr. Syme's case, his name having been already mentioned. Mr. Syme was a most tender-hearted man: as a man of a feeling heart, I would put few before him; and yet more than half of his surgical career was before the time of anæsthetics.

4373. He had a most active surgical career, and had been a great operator before the year 1846 or 1847, when anæsthetics were introduced?—Yes.

4374. In performing these experiments on the lower animals, you have hitherto spoken of the larger animals. Have you ever had occasion to experiment on the frog?—Yes, very much.

4375. Do you think that the sensibility of the frog is as exalted as the sensibility of those larger animals which we have just been speaking of?—Undoubtedly not. Perhaps I might illustrate that subject by examples. The lower we go in the scale of animal organisation, the lower is the sensibility. I remember being very much struck, when a medical student, with the following circumstance. I had some marine animals for observation in a vessel of sea water, some of them attached to a piece of rock; and out of a crevice in the stone there happened to come a very long sort of sea worm, and the creature's tail was still in the rock while the head was moving about, as if in search of food. I wished to draw the animal out to see what the tail resembled, and I took hold of it near the tail with a pair of forceps, necessarily pinching it extremely severely; yet the front of the creature continued its exploratory movements, absolutely unaffected by the very severe treatment of the part near the tail. I was very much struck, I recollect, by seeing that. Then if we proceed higher, take, for instance a fish, to which a frog is closely allied in organisation, it seems to me that the way in which a salmon pulls when it has taken the fly is as good evidence as any experiment made for the purpose can afford, that the sensibility of the tongue and the interior of the throat is altogether different from that of the human subject. Supposing a man were caught by a sharp hook in the tongue or interior of the throat,

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he could not by possibility pull, as the salmon does, against the hook; the torture would be so exquisitely great that it would be an impossibility for him to pull in that way. That is one instance; but it seems to me that it is only one among many that might be given which are perfectly conclusive on the matter.

4376. There is no evidence of the animal in that case suffering any particular pain, but he tries to escape, he is restrained, and he is in an uncomfortable position, just as he would be if put into a basket?—Supposing we really believed that a fish when hooked suffered as a man would suffer, it would surely not be legitimate to set a long sea line and let cod and other fish remain attached to its hooks for hours together. Nor should we be justified in putting a lot of herrings down in the bottom of a boat, if we supposed that they would suffer in their death as the persons put into the Black Hole of Calcutta suffered. I am astonished that there should be any doubt on the question.

4377. (*Lord Winmarleigh.*) You said that hundreds of other instances could be given. Could you give us some one or two more?—I have been told on good authority, that a crab in a vivarium was seen eating food, while another crab was eating the posterior part of the eater. There is a practice familiar to boys, which is this: Take a dragon fly and take off the abdomen, and then give the creature food, and see it swallow. The food passes out through the gullet and the creature goes on voraciously eating, its appetite never satisfied.

4378. Have you seen that experiment done?—I have not, but I have known it done.

4379. (*Mr. Erichsen.*) In your experiments on frogs, you have not found it necessary as a rule to anæsthetise them by chloroform or ether?—There is great inconvenience in the use of chloroform, because it stimulates the frog's web and produces inflammatory action.

4380. In an animal like the frog (we will keep to the frog at present), would you take the movements of the animal kicking, struggling, and so on, as necessary indications of pain?—Certainly not, because they go on after the frog's head has been removed.

4381. Or the spinal cord divided?—Yes, the spinal cord divided next the brain.

4382. (*Lord Winmarleigh.*) At the same time, when a dog is flogged with a whip, or struck with the foot, or hit with a stick, he shows, does he not, by his howling, and by the mode in which he crouches and puts his tail between his legs, that he does suffer very great pain?—The dog is of course in a very different position from the frog; very much more highly organised, and correspondingly much more sensitive.

4383. I collected from what you said before, that the lower you go in the animal creation the less sensible the animals are to pain?—Yes; but the dog being comparatively high in organization, is correspondingly high in sensibility.

4384. Take a worm?—I have taken one instance of a worm.

4385. Take a worm put on a hook; when it writhes about, are you of opinion that that worm does not suffer pain?—I am of opinion that the suffering there is probably almost nil.

4386. How do you prove that?—The analogy of all physiology proves it. The mere movement to which Mr. Erichsen was referring just now is in itself no necessary indication of suffering; just in the same way as in a paralysed man, where there is absolutely no feeling, pinching the leg or tickling the foot will often cause the leg to kick violently, without there being the slightest feeling on the part of the patient.

4387. (*Mr. Erichsen.*) Or without his being even conscious of the limb moving?—Yes.

4388. (*Mr. Hutton.*) But you would not argue, would you, that because a paralysed man kicked violently when he was not conscious of the suffering, it was no proof, if he were not paralysed, that he felt pain? You would not argue from the reflex action in the case where the man was not suffering, that it did not prove that he was suffering in case he were not paralysed?—Of course not. I only take that as absolute evidence that movement does not necessarily imply suffering.

4389. You mean to say, that it does not necessarily prove it; but still, if the frog were whole and perfectly unparalysed, as far as you know, it would rather be evidence that it does suffer, would it not?—Not evidence that it did suffer. For example, take the sensitive plant; I do not suppose that anybody imagines that the sensitive plant suffers when the leaves are so stimulated as to induce them to contract.

4390. Still, the frog is comparatively very high in the order of organised beings. It has a very large proportion of nerve system to its whole being, has it not?—The brain is not very large in proportion to its whole being.

4391. But the nervous system on the whole is, is it not? We have had it called the physiologist's animal on that account. Is it not selected as the physiologist's animal because it has so considerable a proportion of nervous system to the whole being?—I do not think that is the reason that it is selected. It is selected for its convenience, because it is so tolerant of severe treatment, and also undoubtedly largely from the fact of its being not supposed to suffer materially.

4392. Then we have been told by a veterinary surgeon that frogs do show every sign of very considerable suffering under the influence of parasites; and from that he argued that the frog probably was not so low in the scale of sensation as you have just been representing it?—It would be very difficult to prove that frogs *suffer* from parasites.

4393. You were saying that you personally, and that a great many of your colleagues, would think it a blot on the profession if any restriction of any kind were to be imposed. Now do you think that the factory owners would be justified in thinking it a blot on their profession that certain restrictions were imposed upon their employment of children?—Well, perhaps if I were a factory owner I might feel it rather a blot upon myself.

4394. Do not you think that these are the conditions under which legislative restraint is sometimes allowable, that the interests of the creatures whose welfare is at stake cannot be defended by themselves, and that these interests run counter to the interests of the persons who cause them the suffering, or who are liable to cause them the suffering. That is the case with the factory children, for instance; they are not able to defend their own interests, and their interests and those of the trade are at issue and in conflict. Now is not that equally true of this particular case, that the interests of physiologists in the advancement of knowledge, and the interests of the animals which are experimented on in order to advance knowledge, are in conflict with each other?—I do not think it equally the case. I believe that there was a case for the Factory Acts, as far as I have understood, but I do not think that there is a case for any action here.

4395. Now you have said very justly that the medical profession are probably the most humane in the world; but is not that, in a very great measure, because the whole object of their effort is to diminish suffering. That, I suppose, is one of the chief reasons, is it not, why they are so humane; that they are engaged in diminishing suffering?—Yes. They are brought into circumstances in which their best feelings are brought out for the relief of suffering, mental as well as bodily; their sympathies are evoked, as it were, whether they will or not.

4396. Now that does not apply in any way, does it, to the class of physiologists who are engaged in extending knowledge by these inquiries, and not engaged in healing, &c.?—Well, it applies less, of course, in proportion as a man may be more purely devoted to abstract physiological inquiry. If he is not engaged at all in medical practice, of course, in

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so far as it does not apply to him, further than his medical instruction had a humanising influence upon him as a student.

4397. Therefore in his case you have not the guarantee that his whole life is bent on diminishing suffering?—No, certainly not.

4398. And a very large class, an increasing class, of physiologists are springing up, are they not, in this country, and are likely to be needed?—I think it very likely that there are.

4399. I understood you to say that you were very much afraid of diminishing the number of private inquiries of this kind. Now it has been given in evidence by a very eminent man here that if nine tenths at least of this sort of original research were strangled the remaining tenth would be all that anybody would find any advantage from. Does that in any way agree with your knowledge of the original research that has been going on?—No, I should not give such a proportion as that. But besides that, supposing it were true, it would be impossible for anybody to say which was the one tenth that was to be encouraged, and which were the nine tenths that were to be discouraged.

4400. It would be very difficult no doubt?—Impossible.

4401. Still if you are to prevent an immense amount of suffering to animals that is useless, you must set against that, some disadvantage in checking investigations that might be useful?—I do not think you can properly describe it as "an immense amount" of suffering to animals. If you compare it with what animals suffer from being caught in traps and in various sorts of ways injured in the ordinary circumstances of human life, not to mention what they suffer in death from natural causes, cold and hunger and injury and disease, that which vivisection occasions is an absolutely insignificant amount in the abstract.

4402. But though an insignificant amount in this country, it is not at all an insignificant amount in other countries, and it may become a very significant amount in this country?—If it ever should be so I should think it would then be time enough to think of legislation.

4403. And not at the time when the danger is beginning to occur, when we are actually getting a new school of men who are not employed in diminishing suffering, but who are employed in merely investigating physiology as a pure science?—I do not believe that there would be any danger of persons engaged in that way in this country prosecuting it in any inhuman spirit.

4404. But now let me ask your opinion of an investigation which we have had a good deal before us, in which a dog has been subjected to eight or nine hours suffering, of the amount of which it is exceedingly difficult to form an opinion (some witnesses have told us it was very great, and some have told us it was very small), for the purpose of discovering the power of rhubarb in increasing the secretion of the liver. Now assuming the suffering were very great, do you think that is a sufficient end for inflicting eight or nine hours of pure suffering on the dog?—I must say I do. The other day I saw in the country a rat that had been caught by two legs in a rat trap, the creature must have been suffering for hours; that is a kind of thing which is going on all over the country, and which you can never stop. Now the suffering there is very much greater than the suffering in the case that you mentioned.

4405. But still it is not suffering consciously indicted?—The one is suffering consciously inflicted for a noble object, the other was thoughtlessly inflicted for the comparatively ignoble and selfish object of keeping the rat from stealing fruit.

4406. But I thought you said just now that you considered that the fact of its being consciously inflicted was a great additional mischief?—Not the fact of its being consciously inflicted, but *unnecessary* suffering inflicted consciously.

4407. You would not think it in itself an evil, the mere fact that it had been consciously inflicted?—No, if a man has a due sense of his responsibility and the importance of the object before him.

4408. Not even though the creature itself is not the object of his sympathy, but is sacrificed to some remoter object of symyathy?—I still think so. Take for instance such a case as this: suppose a race horse had some tumour growing upon it, upon some place where it did not interfere in the least with the creature's health, but merely with his running, or where it produced a blemish to his appearance; I do not believe anybody would object to a veterinary surgeon being called in to take out the tumour, and even if it were done without chloroform there would be no outcry, although it would be entirely for the pocket or the pride of the possessor that the operation would be performed.

4409. Probably it would be for the creature's advantage too?—It might not be so in the very least.

4410. Then you do not think that the relation between the man and the creature on which he is inflicting suffering for a remote purpose is in itself an evil; you think that it does not tend to harden the sympathies towards the lower animals?—I do not think it does at all if it is done in a right spirit.

4411. Can you give us any impression at all as to the first experiments you made; were they made under chloroform. I refer to your earliest investigations and experiments?—My earliest experiments were made on the frog, which I believe suffers so little that its sufferings scarcely need be considered. I daresay many here may have seen frogs shown at soirées for the purpose of exhibiting the circulation of the blood, the frog's body being in a bag and the toes tied out. If people really supposed that frogs were suffering as much as we should if we had our fingers tied with strings, we should not tolerate such an exhibition.

4412. Surely the question is not whether they suffer as much as we do; it might be conceded that they do not; but whether they suffer very much. Now we had evidence given us that frogs were kept in a jar till ulcers formed all round the eye from the unhealthy manner in which they had been kept, and that that was regarded as a matter of no moment whatever. Should you think that frogs with ulcers all round their eyes suffered nothing?—I really do not suppose that they suffer much as a matter of my own belief.

4413. That is very much matter of conjecture, I suppose?—No, I do not think it is at all. Take the case that I mentioned, for instance, of the salmon; I think that is absolute proof that the sensibility of the salmon's tongue and throat is nothing to compare with our own.

4414. Proof for the salmon but not for the frog?—But the salmon and the frog are very closely allied.

4415. Then you do not disapprove of the practice, which we find prevails in some schools, of not regarding the frog as a sensitive animal at all, and not administering anæsthetics during operations on it which would without them be painful?—I do not think it necessary to give anæsthetics to frogs, I must confess.

4416. Is that view at all caused by the difficulty of doing so, or is it simply that you think it does not suffer? Is there any considerable difficulty in giving ether?—There is another reason, that for one purpose for which the frog is very often used, demonstrating the circulation in the transparent web of the foot, if you give chloroform it is almost impossible to prevent the vapour of the chloroform from irritating the web and producing unnatural appearances.

4417. Does that apply to ether or injecting opium?—I have not tried ether myself, but I think it would probably have a similar effect.

4418. Or injecting opium?—Injecting opium might be as painful to the frog as the experiment that you were going to perform.

4419. There is no great pain attending the injection of opium into the human subject, is there?—No; but

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the mere holding the frog in your warm hand is about as painful, probably, as any experiment that you would perform; to the cold-blooded animals it must be very painful, so far as they are capable of suffering.

4420. Then you would think the experiment which is given in the handbook before us, of raising the temperature of the frog to 35° or 40° Centigrade, a painful operation?—I should, relatively to the animal; but I do not believe the sufferings of the frog are deserving of serious consideration.

4421. Do you think that these kinds of experiments would be materially hampered by excluding the domestic animals, dogs, cats, and horses, from them? —I cannot imagine what object there would be in so doing.

4422. Only that evidence has been given to us that they are liable to a kind of hyper-æsthesia, a kind of suffering which is not the case at all with the rabbit or the non-domestic animals?—I confess I was not at all aware of the fact. I should have thought that in proportion the wild rabbit would suffer as much as the domesticated rabbit.

4423. But the rabbit itself is a creature which apparently gives very little sign of suffering of any kind, is it not?—The scream of a rabbit is not a pleasant thing to hear.

4424. Do you think that no distinction could be drawn between the different classes of the lower animals?—I think it would be a very undesirable thing to make any such limitation.

4425. (*Chairman.*) Your objection to legislation is founded mainly upon the opinion that no occasion has arisen for it yet in this country?—Certainly.

4426. That the operations are performed by persons who are perfectly competent, and who have a just sense, as you have expressed it, of their responsibility in the matter?—Yes, that is my distinct belief.

4427. If they were performed by other persons you think that the effect would be demoralising?—I think that if there were infliction of wanton cruelty that would be very demoralising.

4428. And if demoralising, then reasonably to be prevented by the interference of the legislature?—Undoubtedly.

4429. So that, to use your own phrase, your objection is that legislation is superfluous?—Yes.

4430. And when you express a sentiment that it might be in some way dishonouring to your profession, or a blot upon your profession, it would merely be the application to them of legislative precautions, for which you think they have given no provocation and no occasion?—Yes, as implying that there had been a necessity, which I did not believe existed.

4431. But if it should be the object of legislation to limit the power of performing any of these experiments to persons who are competent, and who have a proper sense of their responsibility, and only to exclude persons who are neither competent nor have such a sense of responsibility, perhaps that sentiment would be diminished?—If it could be done so as not to interfere with private investigation it might be unobjectionable.

4432. (*Sir J. B. Karslake.*) Do you yourself think that there is much difference in the pain that a dog suffers, as compared with that which a rabbit suffers under vivisection?—It is a point on which I cannot say that I have had much experience. I have several times operated on rabbits, but I have not operated much on dogs.

4433. Now, take a smaller animal than the rabbit; do you think there is any appreciable difference between the pain which a guinea pig would suffer and that which a rabbit would suffer under vivisection? —That I am not prepared to say.

4434. Do you think that there would be any appreciable difference in the pain that a wild fox suffers as compared with that which a dog suffers?—That I could not say; it is a subject to which I have not had my attention directed.

The witness withdrew.

Mr. George Richard Jesse called in and examined.

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4435. (*Chairman.*) Are you in any profession?—I am retired from one.

4436. Will you have the kindness to tell us what it was?—Railway engineer.

4437. I think you are the Honorary Secretary of the Society for the Abolition of Vivisection?—Yes.

4438. Are you prepared to make a statement to this Commission on behalf of that society?—I am.

4439. Will you have the goodness to do so?—As already stated in a letter to this Commission, I appear at its request, not as a medical or scientific witness. My opinion as to vivisection has been arrived at by intimacy and close friendship with animals, the perusal of books written by vivisectors, and conversation with medical and other men, some of whom have witnessed the torture of animals for so termed scientific purposes. I am, on the part of the Society for the Abolition of Vivisection, prepared to amply and fully establish that the cruelties of vivisectors are not surpassed by any recorded in the history of mankind. I am prepared to establish that by furnishing extracts from the books written by those men themselves. I wish also to produce correspondence between some of those men and our association; moreover, extracts from the life of the late Dr. John Reid, of St. Andrews, and the life of Sir Astley Cooper. That these practices are becoming a moral ulcer, that they tend to demoralise mankind, and are by education vitiating the minds of the young, I wish to prove by the above and other works, and by reading extracts from Youatt's "Humanity to Brutes" (the famous veterinary surgeon), Dr. Elliotson's "Human Physiology," Lockhart's "Life of Sir Walter Scott," Drummond's "Rights of Animals," Dr. Ferrier's experiments, Dr. Bennett's experiments, and other books, including "Elementary Lessons in Physiology," "The Quarterly Review" for 1849, and also by publications of the Society for the Abolition of Vivisection. The Society for the Abolition of Vivisection, which contains many professional men amongst its members, peers, professors, officers in Her Majesty's service, and others, wishes to make known through this Commission to the people of England the deeds that have been done, and the opposition to the exposure of them it has met with from the unfair conduct of the "Times," "Standard," and the "Pall Mall Gazette."

4440. Can you put in a list of the members of the society?—Not at this moment.

4441. But you can send one to the secretary, I presume?—I could not quite pledge myself to that, because there are some who are members, and who would not like their names to be known.

4442. We only wish it to be understood by us correctly whether you are prepared to send in a list or not. I understand that you are not?—No, excuse me, that would be an erroneous impression. I am prepared to send in a list of the people who have given their names to the association, and which comprises by very far the greater number of its members. I am prepared to give that, but, of course, I cannot in honour give the names of men who wish their names to be private. If you like, I will apply to those men also.

4443. Will you have the goodness to proceed with your statement?—This is one of the first that comes to hand: The British Medical Journal, December 5th, 1874.—"Report of the Committee of the British "Medical Association to investigate the antagonism "of medicines, by John Hughes Bennett, M.D., "F.R.S.E., Chairman and Reporter." I should only take up time, I think, unnecessarily, by reading a

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great deal of this. I will come to the part, if you will allow me, which is most cogent to the point. "Experiment 473.—A male cat, weighing 4 lbs. 9 oz., "had six grains of theine injected under the skin of "the back. For five minutes nothing was observed. "At the end of this period it began to move back-"wards and forwards, and the excitement gradually "increased until the close of 15 minutes after the "administration of the drug, when it seemed to be "frantic. These fits of intense cerebral excitement "afterwards occurred at intervals of two or three "minutes; between them the animal lay quiet. It "appeared to be extremely susceptible to irritation. "A stick brought near it was immediately bitten. "Salivation became very profuse. Twelve minutes "after the dose had been given the cat had difficulty "in moving its posterior extremities. This difficulty "gradually passed into complete paraplegia. Thirty "minutes after it could not move the hinder part of "its body, but its senses were very acute. The ani-"mal noticed every movement made near it, and it "tried to bite. The paralysis gradually invaded the "fore extremities also, and the cat was unable to sit "up. It lay with its head slightly raised, but still "there was the same acuteness of the senses of hear-"ing and vision, and it was easily irritated. It "remained in this condition for an hour and a half, "when, after two very severe clonic spasms, it died. "The narrative of the above experiment indicates "intense cerebral excitement, associated with gradual "loss of the functions of the spinal cord." In regard to some experiments I have here, the Society has made extracts from them and printed them, and we have taken out the pith of them, as we consider; and it may save your time if I read what we printed. I can leave the document with you, so that you can verify it.

4444. Will you read it, if you please ?—It is No. 13 of our publications.

"Facts: 'Thine own mouth condemneth thee, and not I: Yea, thine own lips testify against thee.'—(Job, 15th ch., 5th and 6th v.) As it is often asserted that the practices of vivisectors are exaggerated, this Society brings forward more examples of them. 'Experimental Researches in Cerebral Physiology and Pathology,' by David Ferrier, M.A., M.D. (Edin.), M.R.C.P., Professor of Forensic Medicine, King's College, London, Assistant Physician to the West London Hospital. 'I have to thank Dr. Crichton Browne for kindly placing at my disposal the resources of the Pathological Laboratory of the West Riding Asylum, with a liberal supply of pigeons, fowls, guinea-pigs, rabbits, cats, and dogs, for the purposes of my research. The animals were extended on boards, the head and limbs first tied with cords, which were afterwards relaxed. The brain was exposed by sawing away part of the skull; the surface then extended by pincers, and electrical shocks or irritation given through copper wires applied to it. Concentrated chromic acid was also squirted into the brain through a small hole in the skull. Experiment I.—Medium-sized guinea-pig. Narcotised with chloroform, and the greater part of left hemisphere exposed. The animal was then placed on the floor, and watched. On recovery from stupor the body becomes curved from left to right, the head touching the tail. Active movements of the fore-legs are made, causing the animal to rotate round from left to right. Sometimes the animal makes violent struggles, and falls on its back. When the muscles of the right side are forcibly overcome, the state of pleurosthotonus returns on removal of the resistance. When placed on its right side the animal makes rapid movements with both fore and hind legs, as in running, but is unable to alter its position. When placed on its left side, it becomes curled from left to right, as before, and regains its feet. The pleurosthotonus and movements of the legs occasionally remit. Half an hour after the operation the animal is able to rest quietly on its feet, but with the head directed towards the tail, from left to right. The animal was then, 45 minutes after the operation, again narcotised, and submitted to electric stimulation, but death took place, probably from an overdose of chloroform, during the first application of the electrodes. Apparently the brain retained its excitability, but I discovered that the movements induced by stimulating different parts, and which were on the left side, were in reality due to conducted currents from too strong stimulation. The inferences I subsequently drew from the experiment were that the vital irritation, consequent on exposure of the hemisphere, acted on the muscles of the opposite side of the body through the corpus striatum, causing tetanic spasm and pleurosthotonus. The next experiments were undertaken with a view to ascertain,' &c. 'Several rabbits and cats were employed for this purpose,' &c. After detailing the above, Dr. David Ferrier proceeds to assert, 'It may be mentioned here, once for all, that before and throughout all the following experiments, ether or chloroform was administered.' The natural inference from this qualified statement must necessarily be that narcotics were not administered in all the previous experiments, whether in later ones they were always given or not, or given to render the creatures helpless. Whether the details now published invalidate the assertion of Dr. David Ferrier, whether intense and protracted agony was inflicted, may be estimated by these extracts from a 'mere preliminary instalment of a more extended and complete investigation,' as these experiments are termed by Dr. David Ferrier. The public may perhaps realise from them what is the significance of the phrase, experimenting on animals under anæsthetics. 'Experiment III.—The next experiment was made on a large strong cat. The skull was removed,' &c. 'The animal was only partially narcotised.' P. 68. 'My experiments, therefore, had to be extended over a great many different animals,' &c. 'With and without chloroform, the application of the electrodes apparently produced no effect, the animal remaining perfectly quiet. Occasionally, during application of stronger currents, the animal exhibited restlessness and uttered cries; but they did not indicate anything further than the usual restlessness and cries of guinea pigs when under experimentation.' P. 69. 'In addition also, the position of the animal's head, and its condition as to anæsthesia, seems to modify the results.' P. 77. 'In confirmation of these same results, I observe in my notes of experiments made on a rabbit, already alluded to (p. 36), that after about two thirds of the superior surface of the left hemisphere had been exposed, the animal ultimately recovered, but the whole of the exposed part become the seat of suppuration and fungus cerebri; so that the greater part of the cortical substance was rendered entirely functionless. During the five days intervening between the first operation and the second, which necessitated its death, the animal had apparently recovered perfectly without any distinct paralysis of the opposite side; but it had lost its natural sense of timidity, and regarded things which at other times would have frightened it, with a degree of stupid unconcern. It was also observed that though there was no distinct paralysis of the right side, the animal always had a tendency to run in a circle towards the right,' &c. The following are some of the scenes which ensued in regard to a cat. 'Experiment IV.—The greater part of the right hemisphere of a full-grown strong cat is exposed. The animal lies breathing quietly in the semi-narcotised condition,' &c. Observation 6. 'The animal exhibits signs of pain, screams, and kicks with both hind legs, especially the left.' Observation 12. 'The animal exhibited signs of pain, screamed, and kicked out with its left hind leg, at the same time turning its head round and looking behind in an astonished manner.' Observation 18. 'Repeated opening and closing the jaws. The tongue was seen often to be protruded and retracted.' Observation 19. 'While the temporo-sphenodial gyri were being further exposed, the animal bit angrily and gnawed its own legs. It

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did the same generally after irritation of the same parts.' Observation 20. 'In every case restlessness, opening of the mouth, and long continued cries as if of rage or pain.' Observation 21. 'The animal suddenly starts up, throws back its head, opens its eyes widely, lashes its tail, pants, screams, and spits as if in furious rage. This observation was several times repeated.' Observation 25. 'The excitability of the brain was now well nigh exhausted, and it entirely disappeared four hours after the commencement of the experiment, during which period the exploration was kept up uninterruptedly.' 'Experiment VI.—The left hemisphere of a lively mongrel cur' (sic) 'was to a great extent exposed before beginning the faradisation, and the other parts exposed in detail after the function of the previously exposed portions had been determined.' Observation 10. 'Electrodes to points, &c. Several applications elicited only cries as if in pain. As the animal was emitting cries also during the intervals of stimulation, it was thought that the cries of pain might not be the result of the stimulation, but their immediate intensification on the application of the electrodes seemed to be the direct result of irritation, &c. But the facts of the following experiments on dogs at first seem hardly in accordance with the idea of crossed action in the cerebral hemispheres. In two dogs, one of which has already been alluded to (p. 54), after failing to get further results from stimulation of the convolutions, I completely extirpated the right hemisphere. I give the particulars only of one case, though the two were somewhat similar. The first animal (p. 54) lived three days after the operation. In the second I removed the whole of the right hemisphere just anterior to the corpora quadrigemina. A few minutes after the operation the animal began to howl and bark, the movements of the jaws not having suffered the slightest degree of paralysis. The left sides and the limbs were distinctly weakened, while the right side and limbs seemed to retain their full power. The animal frequently turned its head to the right and struggled with its legs as if to rise and walk. The right fore and hind legs moved vigorously, but the left hind and fore legs were also frequently moved in a similar manner, though less powerfully. The movements of the left legs alternated with those of the right, but occasionally, when the right legs were held so as to get rid of their driving or leading action, the left fore and hind legs would be moved by themselves in the struggle to get free. Thinking that possibly the corpora quadrigemina might be concerned in these bilateral movements, I broke up the ganglia on the right side. But the phenomena remained much the same, though the animal seemed blinded, as it ran against furniture, walls, and jammed its head into impossible corners. It retained the power of opening both eyes, and of howling and barking in a very vigorous manner. Apparently it remained quite conscious, for, when called to, it would struggle to get up, and would sometimes regain its feet, and even succeed in walking a few steps, when it would fall over in a helpless manner. In these attempts the weakness of the left fore leg was very evident. An hour and a half after the operation, when in any way disturbed, it made struggles to get up. In these the hind legs moved alternately, the right certainly more actively, but the left fore leg was scarcely moved, though the right retained full power. The animal growled and barked very frequently. In order to determine whether the combined movements were conditioned by the voluntary impulse of the left hemisphere, I next proceeded, two hours after the removal of the right hemisphere, to expose the sigmoid gyrus of the superior external convolution of the left hemisphere. Having ascertained by electrisation that I could induce the usual movements of the right fore leg by stimulation of its centres here situated, I cut away the greater part of this gyrus, checking the hæmorrhage with cotton wool steeped in perchloride of iron. After this the animal ceased to struggle, and lay in whatever position it was placed. Pinching the toes caused reflex movements in all the four limbs, and at the same time the animal barked energetically and howled when pinched. Pinching of the tail especially caused the animal to bark. This condition continued for several hours, barking being always elicited and some reflex movements of the legs, but not to any great extent. The barking may also have been a reflex phenomenon; but from the fact that barking alone was sometimes induced without any marked reflex movements of the limbs I was rather inclined to attribute the phenomena to retention of consciousness and distinct sense of pain. Ultimately (five hours after the first operation) no barking was caused, but only reflex of the limbs and trunk when the legs or tail were pinched. The dog survived for eight hours after the removal of the hemisphere.' See the 'West Riding Lunatic Asylum Medical Reports,' edited by J. Crichton Browne, M.D., F.R.S.E. Vol. 3. London. Smith Elder & Co., 15, Waterloo Place, 1873. Price 8*s.* 6*d.* This and other papers on the subject may be purchased on application to the honorary secretary, at the rate of one page pamphlet, 2*s.* per 100; two page, 3*s.* per 100; four page, 4*s.* per 100; postage extra. The proceeds are given to the Society for the Abolition of Vivisection."

If you will allow me I will proceed to read now a correspondence between Professor Ferrier and the association.

"Vivisection. The Morning Post, London, Monday, 11th October 1875. We have been requested to publish the following correspondence, '16, Upper Berkeley Street, Portman Square, W., August 2, 1875. Sir, My attention has been drawn to an advertisment inserted by you in the Times of to-day, in which you profess to give extracts from a paper written by me in vol. 3 of the West Riding Lunatic Asylum Medical Reports, edited by Dr. Crichton Browne. With your views as to the justifiableness of the experiments there recorded I do not concern myself, being content to leave this question to the decision of a more competent and impartial tribunal than the society which you represent; but I have to request that when you quote me you will do so fairly. The first sentence of the paragraph from which you draw your account of the operative procedure, commences thus:—"In order to carry out the operations necessary for exposing the brain, and observing the effects of stimulation, I have simply *narcotised* the animal and extended it on a board," &c. (page 34). (The italics are not in the original.) In the opposite page (page 35) occurs this sentence: "It may be mentioned here once for all, that before and throughout all the following experiments, ether or chloroform was administered." You may draw what inference you please as to the purpose this narcosis was intended to serve; but when you suppress these important statements, and at the tail of your quotations convert them (italics excepted) into the following; "*it is stated* that chloroform and ether were given in *some* of the experiments," you do not succeed, whatever your intention may be, in conveying a truthful representation of the facts. I am, Sir, Your obedient Servant, David Ferrier. George R. Jesse, Esq., Henbury, Macclesfield."

"Henbury, Macclesfield, August 4th, 1875. Sir, Your letter dated the 2nd, and bearing London postmark of 3rd instant, arrived here this day. The expression of your unconcern for the views of this Society as to the justifiableness of the practices you term 'experiments' we estimate duly. And your professed opinion of the Society's competence and impartiality is what might be anticipated, as our endeavours are directed to bring those practices before the tribunal of public opinion. We believe you have been fairly quoted. If you think otherwise you can make your own statement in the newspapers. Your assertion that ether or chloroform was administered before and throughout all the experiments, is, we consider, invalidated by the details which accompany it. Will you explain why (if ether or chloroform was administered throughout 'Experiment 4 on a full-grown,

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strong cat') the creature exhibited 'signs of pain,' 'screamed,' 'turned its head round, and looked behind in an astonished manner,' 'bit angrily and gnawed its own legs,' uttered 'long continued cries as if of rage or pain,' 'lashed its tail,' 'panted,' 'screamed,' &c.? Was this an instance and proof of your assertion, 'It may be mentioned here, once for all, 'that before and throughout all the following experiments ether or chloroform was administered'? We pause for a reply. You also say in the first sentence detailing this experiment, 'the animal lies breathing quietly in the semi-narcotised condition.' How long did this 'semi' condition continue? Was it over ere you commenced giving electric shocks to the brain? Again at pp. 36 and 37, experiment 3, 'on another strong cat,' these words appear, 'the animal was only partially narcotised,' and a page 68 you have written, 'with and without chloroform.' Can you explain away the apparent inconsistencies of these statements of your own? The impression produced on the reader's mind, and stated in the 'Times' of the 2nd instant, is that to some of the unhappy creatures who fell into your power you gave ether or chloroform merely that they should be passive, helpless, and unable to struggle or defend themselves against you. Then they were fast bound, skull cut away, dura mater removed, eyeball extirpated, &c., after which, when the effect of the narcotic has passed away, you did that which drew forth screams and other manifestations of agony. Can you deny it? Our belief is that the statement as to giving ether or chloroform, whatever your intention may have been, does not convey the truth, but appears to palter in a double sense. The public may now, perhaps, realise what is the actual meaning of the phrase 'experimenting on animals under anæsthetics.' I am, Sir, your obedient servant, George R. Jesse, Honorary Secretary for the Abolition of Vivisection. Dr. David Ferrier, 16 Upper Berkeley Street, Portman Square, London, W."

"16, Upper Berkeley Street, Portman Square, W., August 9th, 1875. Sir, I am in receipt of your reply, dated 4th August, to mine of the 2nd, in which you ask me to explain certain 'apparent inconsistencies' between the phenomena I have described and the statement that the animals were narcotised. What to you may be inconsistencies do not appear so to those capable of interpreting the true significance of the facts. I would refer you to a letter on this subject by Dr. Crichton Browne to the editor of the 'Times' of August 4th, but personally decline to enter into discussions with you either publicly or in further correspondence. I have waited for your usual advertisement in Monday's 'Times,' and am glad to find it conspicuously absent. As it stood, it was an unfair distortion of the distinct statement which I again make, that anæsthetics were humanely administered before and throughout all the operations and experiments to which you allude. I am, Sir, your obedient servant, David Ferrier. George R. Jesse, Esq., Henbury, Macclesfield."

"Henbury, Macclesfield, September 2, 1875. Sir, We propose publishing the correspondence which has taken place between you and ourselves. As the question is a public one, we presume you can have no objection to this course. I am, Sir, your obedient servant, George R. Jesse, Hon. Secretary. Dr. David Ferrier, 16, Upper Berkeley Street, London, W."

To that letter we never had any reply. We sent it by two different channels.

Then the pamphlet containing that correspondence proceeds: "The following are the extracts alluded to." I have already read them to you from the other paper, and need not read them over again. Then there is this note, "Dr. David Ferrier commenced this correspondence (possibly in the hope of being able to use it to his advantage), but was prompt to end it. He evades explanation of the apparent inconsistencies of his own statements. His mere assertions will hardly satisfy public opinion for the absence of evidence. It is remarkable that while Dr. David Ferrier insinuates we are incapable of interpreting the true signifiacance of the facts, and refers us to his colleague, Dr. J. Crichton Browne, the latter, in the Times of August 4th, gives us credit for being well informed as to the 'phenomena' these experiments drew forth." The letter I now wish to read is the one to which he alludes from Dr. Crichton Browne. The answer to that I also now have in my hands, but the Times would not allow it to appear. There is no doubt that they received it; we sent it by an agent, who saw Mr. Knight, of the Times, and we also wrote again; and we contemplated at one time proceedings against the Times for allowing Dr. Crichton Browne's letter to appear, and not our reply to it. We wrote again to request that they would insert the reply, but got no answer. We did not take proceedings, because we were advised that we were not likely to make much of it. This is the letter which appeared in the Times from Dr. Crichton Browne:—

"To the Editor of the Times.—Sir, An advertisement which appeared in your columns yesterday, under the authority of the Society for the Abolition of Vivisection, contains statements which are skilfully misleading, and which call for immediate correction. After giving an inaccurate description of the mode of experiment adopted by Professor Ferrier in his important researches into the functions of the brain, and after quoting a few of his observations, adroitly selected, because to the non-professional reader they may seem to justify the unwarrantable conclusion that 'intense and protracted agony' was inflicted during their progress; the advertisement proceeds to say 'it is stated that chloroform and ether were given in some of these experiments, but it appears to have been done with the view of rendering the animals helpless.' It will, I think, scarcely be credited that this is the version which the Society for the Abolition of Vivisection thinks proper to give of Professor Ferrier's distinct intimation, made at the outset of the paper in the 'West Riding Asylum Medical Reports,' from which the observations were quoted. 'It may be here mentioned, once for all, that before and throughout all the following experiments ether or chloroform was administered.' Everyone able to understand the meaning and purpose of Professor Ferrier's experiments will at once perceive that it was essential to their success that the animals employed in them should be unconscious and incapable of feeling pain, or of making any voluntary effort. The movements and cries produced by the faradization of the brain were not expressive of suffering, but simply of the stimulation of a motor-centre, and the 'intense and protracted agony' of the animals exhibiting these signs of pain was not greater than a pianoforte when its keys are struck. Similar signs of pain may be witnessed in an animal without a brain, or in the deepest state of anæsthesia. It is an elementary truth in physiology that the brain tissue itself is absolutely insensitive, and may be irritated or sliced away without even discomfort being occasioned. All this must have been well known to those who concocted the advertisement referred to, and yet they have not hesitated to deceive the public by representing mere automatic movements as indications of 'intense and protracted agony.' To such base practices may not ill-judged enthusiasm or a thirst for subscriptions reduce even a benevolent society! I am, Sir, your obedient servant, J. Crichton Browne, West Riding Asylum, Wakefield, August 3."

This is the answer which we sent to the Times and which it would not insert:—

"Vivisection.—To the Editor of the Times.—Sir, 'To such base practices may not ill-judged enthusiasm or a thirst for subscriptions reduce even a benevolent society!' You have thought fit to give place to the above language in your issue of the 4th instant; your sense of justice will allow the reply. Professor Ferrier's 'experiments' were brought to our notice by a Fellow of the Royal Society, who expressed his disapprobation both as to the absence of scientific

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perspicacity and common humanity, which, in his opinion, characterised them. Mr. J. Crichton Browne asserts the statements of the Society for the Abolition of Vivisection are 'skilfully misleading,' and the quoted observations 'adroitly selected.' He compliments our ability at the cost of our veracity. But our quotations from Dr. Ferrier's 'experiments' were literal extracts taken in regular succession from one on a cat. Dr. Ferrier's assertion that ether or chloroform was administered before and 'throughout all' his 'experiments' is, we believe, invalidated by the details which accompany it. If ether or chloroform was administered throughout 'experiment IV.' on a full grown strong cat, and (as Mr. J. Crichton Browne's credulity permits him to affirm, though it is what he cannot possibly know) the creatures felt no more than a pianoforte, how was it the animal gave evidence of astonishment, anger, rage, and pain? How was it the creature uttered long continued cries, screamed, and gnawed his own legs, &c.? The last a strong manifestation of agony. As the animal expressed sentiments of the mind, was it not sensible? Can stimulations of 'motor centres' give rise to emotions of the mind corresponding therewith? Even, if so, has agony a 'motor centre'? We are told these manifestations of agony arose from 'simply the stimulation of a motor centre'? Why then when other 'motor centres' were 'stimulated' did not the cat evince signs of love and other feelings? Why did it not purr, show pleasure, and attempt to caress the professor instead of displaying the most marked indications of astonishment, anger, rage? At experiment III., on another 'strong cat,' these words appear, 'The animal was only partially narcotised,' and on another page we find the words 'with and without chloroform.' In one 'experiment' the right hemisphere was removed from a dog and he lived three days after the mutilation. In another 'the whole of the right hemisphere was removed just anterior to the corpora quadrigemina.' The animal, a few minutes after, howled and barked, &c. 'Apparently it remained quite conscious, for when called to it would struggle to get up.' From the fact that barking alone was sometimes induced without any marked reflex movements of the limbs, I was rather inclined to attribute the 'phenomena to retention of consciousness and distinct sense of pain.' 'The dog survived for eight hours after the removal of the hemisphere.' Mr. J. Crichton Browne asserts the insensibility of the brain. Can he be sure that sensation may not be conveyed by it? He will hardly say that the dura mater is otherwise than excessively sensitive. In conclusion, I ask, why does not Professor Ferrier stand forth himself publicly and defend his own deeds. As to Mr. J. Crichton Browne, who comes in his place, the intemperance of his language is not indicative of the justice of his cause. We trust he will live to feel ashamed of the aspersions he has cast. I am, Sir, Your obedient servant, George R. Jesse, Honorary Secretary of the Society for the Abolition of Vivisection. Henbury, Macclesfield, Cheshire. 7th August 1875. P.S.—There are different reasons for giving chloroform. It is not always administered so as to produce insensibility. Sometimes merely enough may be exhibited to narcotise the public mind, and enable the 'experimenter' to make merciful statements. It is often given only to secure entire helplessness on the part of the patient. G. R. J."

The next paper is the following:—

"Facts. As it is often asserted the cruelties of vivisection are exaggerated, this society brings forward examples of them. Since Majendie's time (who did much to inoculate this nation with the contagious virus of the moral ulcer) there is evidence to prove they have largely increased in number. House of Commons, February 24, 1825. 'Hansard, N. S., Vol. 12, p. 658. Mr. Martin of Galway. There was a Frenchman . . . . of the heart and viscera.' (Great disgust at the statement of this cruel experiment was manifested by the House.) House of Commons, March 11th, 1825. Mr. Martin replied.—'One word as to Professor Majendie. . . . . made it in that House.' 'During the first winter in which I attended . . . . can conscientiously vouch.'—'The Rights of Animals, &c., by William H. Drummund, D.D., M.R.,I.A., London, &c. 1838.'"

"The late Dr. John Reid, Fellow of the Royal College of Physicians of Edinburgh, and Professor in the University of St Andrew's, repented of the tortures he had perpetrated upon the defenceless. Dr. Reid was cut off in the prime of life by cancer at the roof of the tongue, and he repeatedly said to his medical attendants and the members of his family, in allusion to the seat of his long continued agonies, being the same nerves on which he had made so many experiments, 'This is a judgment on me for the sufferings which I inflicted on animals.' How terrible those torments must have been may be estimated by the awful retribution recorded in his life. The grave suddenly yawned at his feet, his countenance cheerless and wan, was gloomy and desponding almost to despair, opium and chloroform daily taken to alleviate his anguish, sleepless nights of unquenchable agony, slow starvation, bodily exhaustion, demon voices reiterating blasphemies, and hoarsely whispering 'curse God and die.' So lowered were his powerful frame and natural energy, that on meeting accidentally his old companion (Mr. Fergusson) in the street, Dr. Reid burst into tears. This physiologist referred to his past life as nothing but a long and dark array of sins and follies. The sufferings he inflicted were not merely incidental to dissection, equivalent to severe surgical operations, but in many of the experiments recorded were deliberately inflicted. It was considered essential that the animals should be left free to exhibit all the pain they felt, and should be expressly subjected to torture. He confessed to having thought much of scientific fame in his labours, and it would be untrue to say the alleviation of human suffering was the motive always before him when he inflicted pain on the lower animals. Dr. John Reed departed this life the 30th July 1849. *See* Life of Dr. John Reid, by George Wilson, M.D. Edinburgh, Sutherland and Knox. London, Simpkin, Marshall, & Co., 1852. Also, Physiological, Pathological, and Anatomical Researches, by John Reid, M.D., &c. Edinburgh, Sutherland and Knox. London, Simpkin, Marshall, & Co., and Samuel Highley, 1848.'"

As this has been called in question several times, there has been a great deal of correspondence in the Scotsman, where we have had to meet a good many antagonists; but I think, in the opinion of any candid and clear sighted man, we have most completely overthrown them, and substantiated every word of it. In fact, there is Wilson's book for anybody to go to. We go on to say, in our pamphlet,—

"In proof that such nameless and demoralising deeds are not only perpetrated, but even becoming educational and systematic in the kingdom, thereby tainting the minds of the rising generation of young men devoted to the profession of medicine, and familiarizing them to scenes of cruelty and cries of agony, it is of great moment to state that in a pamphlet on Edinburgh University extension, and which contains notes on plans, submitted after confidential intercourse and discussion by an architect in 1874, occurs the following:—Page 13. 'I have placed physiology at the south-west corner, because it is desirable to place this department in such a position that it cannot be overlooked, and also where good south and north light can be had. . . . On the ground floor is a room (30 ft. 6 in. by 9 ft.) for physiological experiments on animals. . . . . Another reason I have for placing the physiological department here is, that there is good space for keeping animals, and plenty of south light to preserve them in health. . . . Good accommodation can also be had for keeping the animals belonging to the pathological department. Edinburgh, 30th December, 1874.' Anderson is the name of the architect. We did not put his name in the pamphlet, because he only acted in his professional capacity. "A professor,

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of Edinburgh University, has recently stated, 'For the discovery of new truth, or for the purpose of testing the truth of statements not yet coroborated, experiments without the use of narcotics are, if necessary, had recourse to on rabbits, guinea pigs, or dogs, but those are never given as demonstrations to students. Such experiments are performed in private by my assistant or by myself, and, it may be, some fully qualified person working under our immediate superintendence.' William Rutherford, Professor of the Institutes of Medicine (Physiology), University of Edinburgh, July 2, 1875. [See 'The Scotsman,' 3rd and 10th July 1875.]"

I propose now to refer to the life of Dr. John Reid, by Dr. George Wilson, page 124. "I am not anxious . . . . at present they are."

4445. What is the date of that?—This is the second edition, dated 1852.

"A premium as . . . . ethics it is one."

4446. Are there other passages in that book to which you desire to draw our attention?—This is the only remaining one, and it will be found at page 130:—

"We are thus taught . . . . committed to our care."

The next book to which I have to refer is "Lessons "in Elementary Physiology, by Thomas H. Huxley, "LL.D., F.R.S." 1874. "Preface to the first "edition. The following 'Lessons in Elementary "Physiology' are primarily intended to serve the "purpose of a text-book for teachers and learners in "boys' and girls' schools." In the preface to the second edition I find, "As the majority of the "readers of these lessons will assuredly have no "opportunity of studying anatomy or physiology upon "the human subject, these remarks may seem dis"couraging. But they are not so in reality. For "the purpose of acquiring a practical, though ele"mentary, acquaintance with physiological anatomy "and histology, the organs and tissues of the com"monest domestic animals afford ample materials." Then there are these directions at page 255: "If "when the cord is cut across in an animal the cut "end of the portion below the division, or away from "the brain, be irritated, violent movements of all the "muscles supplied by nerves given off from the "lower part of the cord take place, but there is no "sensation. On the other hand, if that part of the "cord which is still connected with the brain, or "better, if any afferent nerve connected with that "part of the cord be irritated, great pain ensues, as is "shown by the movements of the animal, but in these "movements the muscles supplied by nerves coming "from the spinal cord below the cut take no part; "they remain perfectly quiet." At page 252,—"If "the trunk of a spinal nerve be irritated in any way, "as by pinching, cutting, galvanising, or applying a "hot body, two things happen. In the first place, "all the muscles to which filaments of this nerve "are distributed, contract; in the second, acute pain "is felt, and the pain is referred to that part of the "skin to which fibres of the nerve are distributed. "In other words, the effect of irritating the trunk "of a nerve is the same as that of irritating its "component fibres at their terminations." At page 52,—"That this is the real state of the case may be "proved experimentally upon rabbits. These ani"mals may be made to blush artificially. If in a "rabbit the sympathetic nerve, which sends branches "to the vessels of the head, is cut, the ear of the "rabbit, which is covered by so delicate an integu"ment that the changes in its vessels can be readily "perceived at once, blushes. That is to say, the "vessels dilate, fill with blood, and the ear becomes "red and hot. The reason of this is that when the "sympathetic is cut, the nervous stimulus, which is "ordinarily sent along its branches, is interrupted, "and the muscles of the small vessels, which were "slightly contracted, become altogether relaxed. "And now it is quite possible to produce pallor and "cold in the rabbit's ear. To do this, it is only "necessary to irritate the cut end of the sympathetic, "which remains connected with the vessels. The "nerve then becomes excited, so that the muscular "fibres of the vessels are thrown into a violent state "of contraction, which diminishes their calibre so "much that the blood can hardly make its way "through them. Consequently, the ear becomes "pale and cold." I might read other passages of the kind in this work, but as they are very much indeed to the same purpose, I shall not take up the time of the Royal Commission unnecessarily; but I wish to make that remark to go in evidence, that there are other passages of the kind. The next book to which I wish to refer is "The Rights of Animals, "and Man's Obligation to treat them with Humanity, "by William H. Drummond, D.D., M.R.I.A., and "Honorary Member of the Belfast Natural History "Society."

4447. Is he living?—That I do not know. The date of this book is 1838. At page 163 it says:—

"Innumerable experiments have . . . . suffering entirely obliterated."

Then the next book to which I would refer is a book entitled "Vivisection. Is it necessary or justifiable? Being two prize essays published by the Royal Society for the Prevention of Cruelty to Animals," and published in London by Robert Hardwicke, 192, Piccadilly, in the year 1866. What I wish to refer to is an extract which it contains from a work written by Mr. J. Burn Murdoch, of Gartincaber, Edinburgh, in 1846, entitled, "An Account of a Visit to the Veterinary School of Alfort, in 1844."

4448. Do you wish to refer us to particular passages in that book, as showing the evils of the system of vivisection?—Yes.

4449. Will you be so good as to give us the references to them?—I will furnish them afterwards. My object in referring you especially to that passage headed "A visit to Alfort" is this; it is simply harrowing in its details, and he being a veterinary surgeon, of course he is competent to speak, and I expect he is a very good authority; and the thing almost culminates in those horrors. Our object in bringing that forward is to show what length these things not only can go to, but have gone to, and that what has occurred in France may occur here. I am now going to read a very few lines out of another work, and my object is to show how the thing is spreading; that it is not confined merely to laboratories and hospitals, but is done in lodgings and so on; in fact, that it is permeating general society. It is called "Me"dical Students of the period. A few words in "defence of those much maligned people; with "digressions on various topics of public interest "connected with medical science, by R. Temple "Wright, M.D., late scholar of King's College, "London," and it is published by William Blackwood and Sons, Edinburgh and London, 1867.

4450. Is Mr. Wright living?—I do not know; I have not the honour of his acquaintance.

4451. You refer us to that book as showing that in your opinion evidence is obtainable of the spread of the system?—Yes.

4452. What page do you particularly direct our attention to?—Page 137 and page 138.

4453. (*Lord Winmarleigh.*) Does that book contain opinions expressed by people to the society, or are they facts?—I do not know whether they are facts or not. The writer gives them here in this book as facts. I want to show what some of these young fellows do in their lodgings when they meet in an evening.

4454. (*Chairman.*) Have you read the book yourself?—No; I have read the passages I particularly refer to.

4455. Are you acquainted with the writer?—Not at all.

The witness withdrew.

Adjourned to to-morrow at two o'clock.

Tuesday, 2nd November 1875.

PRESENT :

THE RIGHT HON. VISCOUNT CARDWELL IN THE CHAIR.

The Right Hon. LORD WINMARLEIGH.
Sir J. B. KARSLAKE, M.P.
THOMAS HENRY HUXLEY, Esq.
JOHN ERIC ERICHSEN, Esq.
RICHARD HOLT HUTTON, Esq.
N. BAKER, Esq., Secretary.

ROBERT MCDONNELL, M.D., called in and examined.

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4456. (*Chairman.*) You are surgeon to Steevens' Hospital in Dublin ?—Yes.

4457. And a Fellow of the Royal Society ?—Yes.

4458. And a surgeon in practice in Dublin ?—Yes.

4459. Now have you paid particular attention to the subject of our inquiry ?—I have been for many years a teacher of physiology,—Lecturer on Anatomy and Physiology.

4460. What is the general feeling in Ireland about experiments upon living animals ?—I do not think that there exists in our schools any abuse that has called forth any very strong feeling on the subject.

4461. But we have been told by one of our former witnesses that there is a very strong feeling upon the subject in educated society in Dublin; do you believe that to be the case ?—Yes, no doubt.

4462. And is that same feeling that is a sentiment of humanity, strongly shared by the medical profession in Dublin ?—I think it is.

4463. And by the students ?—I think it is.

4464. Do you believe that there has ever been in Ireland any reckless experimenting by students ?—No, certainly not; I never knew of it.

4465. Suppose that you were to introduce living animals for experiments for demonstration to students, and that you did not propose to use anæsthetics, what do you think would be the feeling of the students ?—Unless you were able to give some good reason for doing away with the anæsthetics I am sure that our students would not tolerate it; the public opinion of our students, who are intelligent and kindly young men, would be strongly against it.

4466. Now of such experiments as are useful for demonstration, may all be done under anæsthesia ?—The vast majority, I should not say all.

4467. But very nearly all ?—Yes, very nearly all.

4468. Now speaking both of demonstration and of original research, may most of the experiments be performed while the animal is unconscious ?—I should say the vast majority.

4469. And of those that remain may a considerable part be so performed that the most painful portion of the experiment shall be under anæsthetics ?—I should say clearly yes.

4470. Then with regard to those which still remain, are they experiments upon sensation ?—Yes, they would be experiments that must be excluded from anæsthetics.

4471. Are they numerous or rare ?—They are rare.

4472. When a fact has been already established, such, for instance, as the facts which were established by Sir Charles Bell, would you propose that there should be a repetition of such experiments ?—No, certainly not, when once thoroughly established; but while *sub judice* I think the case is different.

4473. But where a fact has been sufficiently established, you would hold that it is very wrong to repeat it, if of that kind where the animal must necessarily feel pain ?—Yes.

4474. Then of this small portion of the experiments that remain, namely, where sensation is the object of the experiment, and where the fact to be established is a new one, do you contemplate that protracted agony would be one of the consequences ?—No, certainly not; the animal would be destroyed immediately after the experiment.

4475. Now have you turned your attention to the subject of legislation ?—I have thought about it, but I confess I see immense difficulties in practical legislation. I cannot say that my thoughts have taken any definite form. I see very great difficulties in the way.

4476. Do you think that the position of the physiologist would be in any way improved by legislation ?—I do; as a physiologist I should like some legislation to save the physiologist from the outcry that has been raised by mis-statements and exaggerations. I think it would give public confidence if some means could be devised of getting at the truth without interfering with the progress of science; I think it would be desirable for the physiologist to have the protection which would arise from disarming the suspicions of the public.

4477. Have you at all turned your attention to the line which such legislation might take ?—I cast my eye over the Bills which were brought forward last year, but I cannot say that I thought that either of the two Bills which I read would very well meet the case.

4478. Have you turned your attention at all to another idea which has been suggested, that of inspection ?—Yes, I have; but here again I confess I found very great difficulties; a difficulty between its being a mere sham which would not give the public the necessary confidence, and its being an inspection of a kind which would really interfere with the progress of science.

4479. What I have understood you then to say generally is this: that you would look with favour upon legislation as a protection to those who are qualified to instruct in physiology, but that you have not elaborated in your own mind any scheme by which such legislation could be rendered effectual for its purpose, without being productive of interference with what you consider necessary; is that the state of the case ?—Yes.

4480. Now you have spoken of the students in Ireland as being impressed with sentiments of humanity. Do you think that there is any tendency in such experiments as are performed before them to demoralize them ?—No, I think not. It acts upon the student just as an operation upon the human being. If he is in contact with a teacher who has a noble object, and who impresses his class with the idea that he has a noble object in view, it has a humanizing and not a brutalizing effect.

4481. Do I rightly understand you to mean that it is the spirit of the teacher which causes the seeing of experiments to be demoralizing or the reverse ?—In some degree it is the spirit of the teacher; but I think it is exactly as Bishop Butler long since pointed out; if the mind is under the active idea that great good is to result from it, it has a humanizing and improving effect; if it is a passive spectacle of horror like a bull fight or an execution, it is demoralizing. The active is beneficial, the passive witnessing of suffering is demoralizing.

4482. Then it is impossible from your point of view to lay down a positive line of demarcation on the subject, except that the motives in view and the spirit in which the experiment is conducted, render it either demoralizing or otherwise ?—Yes.

4483. If then it is in the hands of proper and humane persons you think it not to be demoralizing; but if it were to be in the hands of persons of an opposite character you would think it demoralizing ?—That is precisely my view.

4484. (*Lord Winmarleigh.*) Do you know of any other places in Ireland besides that in which you oper-

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ate, in which experiments are made upon living animals?—I am acquainted with all the schools in Ireland more or less, and in none of our schools can it be said that experiment on living animals is carried on as a means of instruction in the lecture theatre.

4485. But by way of experiments for research I mean?—I am one of the few physiologists in Ireland who has done much in the way of experimental physiology, and my principal researches have been scientific researches at which I permitted my pupils to be present; but in none of the schools in Ireland is there any actual course of experimental physiology given.

4486. You said that in a very small minority of cases experiments cannot be made under anæsthetics?—Yes.

4487. Is it your opinion that even in that small number of cases which you say form the exceptions, the pain could be diminished by soporifics or any other means?—No, certainly not. In this particular class of cases the experiments are on sensation; the pain could not be avoided therefore in that particular class of cases.

4488. Are there any of those which you have practised yourself in which there has been prolonged pain?—No, not prolonged pain, but unavoidable pain. I have repeated at the time that they were quite new and *sub judice* the researches of Dr. Brown-Séquard upon the spinal cord of animals. In those cases I believe it is unavoidable to have suffering, but the animals were instantly destroyed.

4489. In your own experience, what is about the longest period for which pain has been suffered by the animal?—That is a question very difficult to answer, because of course there are some experiments in which it is necessary to keep the animal alive. Although you may save it the pain at the time of the operation, it is necessary to keep it alive in order to see the result. In such cases as that all pain would be saved at the time of the operation, and you would merely have the pain which a wound might cause.

4490. (*Chairman.*) You are not now speaking of experiments to test sensation?—No. I may say that I would divide experiments into three classes. First, those in which anæsthetics can be given, and in which immediately, before the influence of the anæsthetic passes away, the animal is destroyed; in which experiments, therefore, there is absolutely no suffering. Secondly, those in which the anæsthetic may be perfectly well given, but in which it is necessary to watch the result, and the animal, therefore, must be permitted to live for a period in order to ascertain the result; in that case the animal will be saved entirely the pain of the operation; but the subsequent and slighter pain of the smarting, and so on, which may occur afterwards, is, I think, unavoidable; but it is comparatively but slight. Thirdly, you have the very small class of cases in which, inasmuch as the physiologist has to test sensibility, it is a necessary part of the research that the animal shall be able to feel; there we cannot use anæsthetics, but in those cases the animal is immediately destroyed. I here refer only to experiments attended by cutting operations. The important class of experiments dealing with the action of drugs, the nutritive qualities of food, the development of entozoa, &c., would not come under these heads.

4491. (*Lord Winmarleigh.*) Do you think those experiments of essential importance for medical or other treatment of human beings?—I think they are among the very most important of the last quarter of a century.

4492. Could you mention one or two which you consider of most importance to the human race?—I should say that Dr. Brown-Séquard's discoveries, with regard to the artificial production of epilepsy in animals, would probably be looked upon as one of the most important.

4493. That has been discovered through these means, has it?—Yes.

4494. And could that have been discovered through any treatment of the human body?—No, clearly not.

4495. Would you mention another?—I would group together all his discoveries with regard to the conducting channels in the spinal cord.

4496. And those experiments could not have been made upon human bodies?—No, clearly not.

4497. You are quite clear of that?—Yes.

4498. And if anybody stated here that they could be so made, you would think that a mistake?—Yes.

4499. (*Sir J. B. Karslake.*) Do I rightly understand that you have a class of students to whom you lecture on subjects connected with physiology?—I was for many years a lecturer; I am not now. I am one of the Examiners in the College of Surgeons now, and it is not compatible with that position that I should be a lecturer.

4500. When did you cease to be a lecturer to a class of students?—About two years ago.

4501. And for how many years had you been lecturer?—Nearly 20 years.

4502. In the course of your lectures you never exhibited any experiments upon living animals?—Not in my systematic course; but in some cases where I was of necessity myself repeating experiments, I gave my pupils the opportunity of seeing them.

4503. But that was rather as a matter of favour than as any part of the systematic course?—Yes.

4504. In giving the students that opportunity, did you do so because you thought it would perfect their education in physiological subjects?—The class that attended me at that time were some of them young medical men who had already passed their examination, and who came from the same desire that I had myself to ascertain exactly the results. It was a new subject; it was one completely novel in the medical world at the time. I was repeating the experiments with a view to ascertain whether they were true or not, to verify them; and I gave some of my former pupils and the most intelligent of the students the opportunity of being present at these lectures. Some of my hearers at that time were already qualified as medical men.

4505. Have you formed any opinion yourself as to whether the education of a student can be satisfactorily completed without demonstrations of this description?—It is very difficult to answer in general terms such a question as that, because we must understand what is meant by "experiments" first. Take such an experiment as showing the circulation in the frog's web; some people might cavil at that as an experiment on an animal; or, for instance, rendering a tadpole insensible in order to place it under a microscope. Those are things which you could not educate a pupil, I think, without showing him.

4506. You think that there are some things in which you give a certain amount of pain to animals which it is necessary to show to students?—Certainly; I do not think you could give a medical man a proper education without it.

4507. (*Mr. Erichsen.*) Is there pain in those experiments?—I should say almost none. That is the difficulty, however, where to draw the line. There can be no pain that a reasonable person would object to I think in placing a frog's web under the microscope; but then some people are so sensitive; I have heard persons object even to that.

4508. (*Sir J. B. Karslake.*) What I was rather wishing to get at was your view of this subject, whether there are matters which cannot be fully understood by students, without the assistance of these experiments, and which it is necessary for them to understand?—I think there are some which it is necessary to show to students.

4509. Is there any particular class of animals upon which you generally make your experiments, or do you use dogs, and cats, and rabbits, and frogs like most other gentlemen performing these experiments?—We all use the same; but as a matter of fact it is hardly done with us at all; the experimenting upon

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animals as a systematic part of the training is very little done with us.

4510. And even for the purpose of physiological research is the number of animals that you sacrifice small?—Very few.

4511. (*Chairman.*) I suppose you consider that in Ireland this mode of research and of teaching is quite in its infancy, do not you?—I do not know that. It has been carried on for a good long time for purposes of research, but I think it is done with a great attention to humanity.

4512. (*Sir J. B. Karslake.*) You said just now that you did not think there was any reckless conduct on the part of students or young medical men in Ireland in making these experiments?—I need not say "no reckless conduct," on the part of the students for there is no conduct at all; they do not do it.

4513. As far as you know, excepting your own laboratory, do the medical students or young medical men enter upon these experiments at all in Ireland?—In my 20 years experience I never knew a medical student think of original research or of anything else except his final examination.

4514. Or a young medical man?—Young medical men after they pass their examination, if they are really earnest in their desire to advance the science of medicine and surgery, must begin sometime to do it. I did it myself when I was young.

4515. Did you do it in your own private study?—I did it in the laboratory of the school with which I was connected.

4516. It was done more or less, I suppose, under the supervision of the authorities of the school?—No, but with their knowledge and cognizance; they might have been present, but they had perfect confidence that I would not abuse the trust reposed in me.

4517. So far as others do the same thing in the present day, do they do it, do you think, in their private houses or in laboratories?—I think, from considerations of convenience, it may be said always to be done in laboratories. I have made some experiments in my own house, but very rarely.

4518. (*Mr. Huxley.*) I think we may gather from what you have said, that if you were desirous of carrying on a series of experiments, if the alternative in any case were offered to you or presented itself in performing a painful experiment of using anæsthetics or not using them, you would certainly use them?—Certainly, always.

4519. You have known a good many physiologists both of your own country and of these islands?—And America.

4520. Did you ever know any exception to that rule?—Never.

4521. You never knew a man who, if the opportunity offered itself, would put an animal to pain rather than sacrifice a little of his own convenience?—Never. I myself, and I believe all of us, would shrink from that with the greatest possible repugnance.

4522. I understood you to say that, so far as your knowledge of Ireland is concerned (and it seems to be extensive), you do not think that there are any abuses existing which would form a ground for legislation?—I should say certainly not.

4523. And if you desire legislation at all it is rather as a matter of protection to physiologists?—Yes.

4524. I think that when you began to teach 20 years ago, I may say that the modes of teaching physiology were not so thorough as they are now?—By no means.

4525. In fact, I might say that of most branches of science, for example chemistry, was not then taught in the thorough way it is now?—No; practical teaching of all kinds has been developed immensely.

4526. I suppose that when you were a student 20 or 25 years ago, it was not, I will not say possible, but at any rate it was not easy to obtain practical teaching in a chemical laboratory?—It was impossible you may say.

4527. And therefore, in asking you about your practice when you began to teach, and in suggesting that there are some things that you might think it desirable to do now which you would not do then, of course I am not for a moment throwing any doubt upon the teaching being up to the level of its time; but I presume at the present day, if you were now teaching physiology, there are certain things which you would imagine no student could properly understand unless he had had them put before him in a visible shape; take, for example, Weber's experiment on the pneumo-gastric nerve; I presume you would like a student to see that?—If I were lecturing on physiology now, I would consider it my bounden duty to repeat such an experiment as that each succeeding year. It can be done absolutely without pain, and it is an experiment of so much importance that I would look upon it as my bounden duty as a man desirous of promoting the interests of my fellow creatures, to repeat that every year.

4528. And on the ground that no man by mere description could form any adequate conception of the very striking phenomena which are brought out by that experiment?—Precisely.

4529. Various suggestions have been made by witnesses here as to the mode in which experimentation should be regulated. Now one of these suggestions is that no such experimentation should take place except in a theatre or large open room provided with a gallery, to which the public are to be admitted, a certain proportion of the public being persons who are not students of physiology; and it has been suggested that that would be an efficient check upon the supposed tendencies of physiologists to carelessness and cruelty. What would you think of that arrangement as a practical experimentor?—I should not have the slightest objection to it from one point of view, but it would be perfectly ridiculous in another. It would be no check whatever, because you could not prevent physiologists who did object to making experiments in public from making them in private; and they would make them in private.

4530. Do you think, supposing that a man were obliged to carry on delicate and difficult researches under those circumstances, that his attention to what he was about would be at all favoured by the fact of there being a number of persons looking on?—Many experiments could not be carried on in such a place; they are too delicate. If you are to use delicate instruments, as galvanometers for instance, you cannot have a crowd of people about.

4531. And would it not be another difficulty that persons not instructed in these matters (and on the hypothesis a certain proportion would be of that class) might form a most entirely erroneous judgment as to whether pain was inflicted or not?—Yes; no doubt. An ordinary observer cannot believe that reflex movements in a frog's leg are not evidence of pain, although you may have nothing there but the part below the middle of the body. I may mention that I recollect once myself being assailed for showing the pulsations of a heart entirely removed from the body of a frog, so hard is it for some persons to believe that there is not evidence of pain in that kind of movement, that kind of lurking vitality, which hangs about the heart of one of the lower animals, just as a rose will blossom in water after being pulled from the stem. Many people cannot conceive that that is not cruel; but in reality there is no cruelty there.

4532. If I may refer to an experiment with which you are very familiar made on a decapitated frog, in which a drop of acetic acid is put on the region of the loins, you are aware that the frog takes up a leg on one side and rubs the place?—Yes.

4533. And if that leg is held he will take up a leg on the other side and rub it, at great inconvenience. Now, I suppose it would be impossible to persuade anybody who did not understand these things that the frog in that case did not feel?—Yes; unless they saw the same thing done by a man whose spinal marrow was injured by an accident. The only way to

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convince them that it does not feel is to make a human being do the same, who assures you that he cannot feel. I have seen a case in which, on the surgeon attempting the introduction of the instrument, a patient unable to feel, has shaken his hand, and put it down like this (*describing it*) although he could not tell, if his eyes were shut, that anything was being done to him; therefore, when we meet with a case of that kind in a human being it convinces us that the frog does the same without any consciousness.

4534. (*Mr. Erichsen.*) And the condition of the human being in the case supposed is exactly similar to that of a frog without a head?—Yes.

4535. (*Mr. Huxley.*) Have you seen any of the documents which have been published by the Society for the Abolition of Vivisection?—I have; but I cannot say that I have read them with much attention.

4536. You have no judgment as to how far they are fair representations of what takes place within your knowledge of physiology?—I have seen statements made that I consider to be very gross exaggerations, and they really are so hurtful to men who are anxious to do the best they can for suffering humanity that I feel so sore about it that I do not read them.

4537. (*Mr. Erichsen.*) You are, as we all know, a practising surgeon as well as a scientific physiologist. Can you form any idea of what the position of practical surgery would be if many points in connexion with it had not been elucidated by experiments on animals?—I think the best answer that I could give to that for a popular purpose, which I understand is the object of your question, would be to ask any person to read the accounts given by some of the old surgeons, say, the account given in Ambrose Paré's book of the horrors of an operation in his time, and then to turn to an operating theatre as we know it to exist at the present time, and for any impartial person to ask himself how much of that vast improvement is due to experimentation upon animals. Chloroform itself was discovered by distilling a certain acid from red ants, and it is due therefore to experimentation upon animals. If we compare the horrors at the time when red hot knives were used in amputations with the practice at the present time, I do not think any impartial person could shut his eyes to the enormous advantages that have arisen.

4538. Taking the illustration you have just given us, we have had it in evidence abundantly before us that the circulation of the blood was in a great measure determined by experimentation on animals. Ambrose Paré lived in the years towards the middle of the sixteenth century, and the discovery of the circulation of the blood was made at the commencement of the seventeenth. Is it your opinion that these practices that you mention, amputation with red-hot knives, and matters of that description, which went on in Ambrose Paré's time, were in a great measure due to surgeons not being acquainted with the true mechanism of the circulation?—Yes; no doubt they were; but you must remember, as is stated by Cooper, Sprengel, and others, that for 100 years after Ambrose Paré discovered the ligature the cautery was used in the *Hôtel Dieu* in Paris; because Paré was born before Bacon, and it was not at that time the habit to demonstrate experimentally these things as it would be in the present day; but obviously if experimental physiology had been then in vogue as it is now mankind would have been saved for about 100 years the torture of the cautery. Though I quite admit that what you say is most important it would all have come about even sooner if experimentation had been in vogue.

4539. Take such a disease as that of aneurism; could its pathology and method of cure have possibly been known if we had not been previously acquainted with the mechanism of the circulation of the blood?—I think not.

4540. And, therefore, as the discovery of the mechanism of the circulation of the blood is due to experimental physiology, we could not have been acquainted with the proper treatment of such a disease as aneurism, except, indirectly at all events, through the medium of experimental physiology?—That is quite my view.

4541. Have you ever had occasion to make experiments on animals directly bearing upon surgical practice?—Yes.

4542. Would you mention a few instances?—The two most important practical points that I have myself experimented upon have been the torsion of arteries and the transfusion of blood.

4543. With regard to the torsion of arteries, which is now very greatly used as a substitute for the ligature, the knowledge that we possess upon that point was due in the first instance to experiments made by French surgeons and physiologists, I think, at the commencement of this century?—Yes, by Amussat and others.

4544. Will you kindly explain to the Commissioners what the torsion of arteries is?—After Ambrose Paré discovered the ligature it was usual to tie vessels with silken threads, and these threads are left in the wound to come away after some days. Nowadays, many surgeons, myself among others, and some of the leading surgeons in the hospitals of London, catch the end of the vessel and twist it with a pair of forceps in a peculiar way, and the twisting secures the end of the vessel completely, so that you can avoid the leaving behind any threads to irritate the wound; the artery gets sealed up as it were in a peculiar way at the end.

4544*a*. And that process, simple as it appears, is somewhat complex in reality?—Yes, it requires care and is really complex.

4545. And its complexity and the proper method of effecting it has been determined by experiments upon animals?—I have done so myself; Mr. Bryant and Professor Humphry, of Cambridge, made experiments on the same subject. I never could have ventured to do it on a human being until I had made sure that it could be done successfully on an animal first.

4546. In the event of surgeons not being able to experiment on animals they would have to make these experiments on human beings, or else the progress of surgery would be materially arrested?—Everything in the progress of surgery is tentative, and the trial must be made either on a human being or an animal.

4547. And you consider, I presume, that it is wiser and more humane to do it on an animal than on human beings?—Yes, that is my idea of humanity. On transfusion I also made some experiments. Transfusion, I may explain to the Commissioners, consists in replacing the blood of a patient who is dying from hæmorrhage, by the blood of an animal or a human being; it becomes necessary to inject it into the veins of the individual who is dying. I have performed this operation myself a good many times, and on some occasions with very brilliant success. Obviously an object of this kind must be investigated on the lower animals before one has the courage to try it on a human being.

4548. Are there not special dangers connected with transfusion, such as the entrance of air into the veins, and the entrance of clots into the veins, the dangers of which have been demonstrated by experiments on the lower animals?—Yes.

4549. Conditions which, unless they are attended to, would certainly render transfusion a fatal instead of a salutary operation?—Yes, certainly fatal.

4550. That question about the introduction of air into the veins of animals, mixed with blood, is a matter of great importance, is it not?—Yes.

4551. Is it not a source of great danger and of death in certain operations in certain regions of the body?—It is; it has been so.

4552. And the method of death in those cases was not understood until experiments were made, also by the French physiologists I may say, on the lower animals?—I think they threw light on the subject.

4553. It was suspected but not exactly understood?—Yes, and the same for clots.

4554. (*Chairman.*) Could those experiments in

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torsion and transfusion be performed under anæsthetics ?—Yes.

4555. (*Mr. Erichsen.*) The experiments on torsion, I presume, you have always done under anæsthetics ?—Yes.

4556. It is easier to do them so ?—It is positively easier, apart from motives of humanity.

4557. (*Sir J. B. Karslake.*) Is the whole of the experiment carried out under anæsthetics ?—No, because it belongs to those experiments in which the effect must be observed.

4558. How long is it necessary to keep an animal alive for the purpose of ascertaining the effect ?—In such a case as that the animal might be allowed to recover altogether, because the pain is slight. The pain of the operation is severe, but the pain of the subsequent part is so almost nil that it would be humane in fact to allow the animal to recover altogether.

4559. (*Lord Winmarleigh.*) But the severe portion could be done under anæsthetics ?—Yes.

4560. (*Sir J. B. Karslake.*) You would think it a cruel sacrifice of life after having performed that operation to sacrifice the animal ?—I should.

4561. And the animal might enjoy life if allowed to live after it ?—Yes.

4562. (*Mr. Erichsen.*) A dog, for instance, whose femoral artery has been twisted, is perfectly happy in the course of an hour or two afterwards ?—Yes, the suffering is not acute. The animal would not suffer more than if his ear had been bitten in a fight with another dog. We do not kill dogs to save them from such pain as that.

4562*a*. There is no sign of suffering ?—No.

4563. (*Mr. Hutton.*) I understood you to say in answer to Professor Huxley that, as far as your experience went, anæsthetics are always given to all classes of animals when painful experiments are made. Did you include frogs ?—Well, I constantly do use chloroform for them; I put them under the influence of chloroform, and even tadpoles.

4564. And have you found no difficulty in that; because we have had witnesses before our Commission who stated that they never thought it worth while to produce anæsthesia in frogs ?—It is not difficult. I have a simple mode; a little chloroform is diffused through water, and if the tadpole is put swimming about in it for a few minutes, there is enough of absorption through the skin to render it soon insensible, and it can be placed under the microscope for a time and will not stir.

4565. And you would recommend that in all experiments on frogs ?—Yes, I always do recommend it.

4566. I understand that you would disapprove of young men, at least before they had completed their education, being allowed to make any experiment at all ?—I am quite sure if it were attempted in our schools it would be at once put a stop to. We have no rule prohibiting it, but that is because the evil does not exist. I refer to painful experiments.

4567. And you would disapprove of it until the students were at least accomplished physiologists ?—I should disapprove of students experimenting as a rule.

4568. Do you or do you not find any great inconvenience from not having any experiments in the Dublin medical schools under chloroform by way of demonstration ?—I myself should approve of physiological lectures being even more demonstrated than they are at present in Ireland in all those cases where it can be done with the use of anæsthetics, and where it can be done without pain. I think that the demonstration experimentally is so very important to fix a fact on the mind of a student, that where it can be done with a certainty of saving pain, I would be in favour of its being done more frequently than it is at present.

4569. But you would disapprove of painful experiments before classes ?—As a general rule, certainly; I do not say invariably; I think there are some of that kind which it is necessary to show; but, as a rule, I should disapprove of it.

4570. As to those experiments you referred to on sensibility, what would be the longest time for which the animal would suffer before it would be possible to destroy it ?—That is a question very difficult to answer; because sometimes the shock of the experiment will make it necessary to allow an animal, to a certain extent, to recover in order to see the condition in which it is; but I should say never long, it is always a brief period.

4571. Were there not some of those experiments of Dr. Brown-Sêquard's in which the animal was made to rotate for a very long period after the puncture of some particular point ?—The animal is quite insensible during that experiment, it is an epileptic fit; and judging from what we know of human beings in that condition the animal I believe, is perfectly insensible. It is a convulsion accompanied by insensibility. I have repeated that experiment myself, and I am convinced that it is absolutely free from sensation.

4572. (*Mr. Huxley.*) I am not quite sure whether I understood the sense in which you object to students experimenting; whether it is that you object to their conducting experiments by themselves in a room, which is one view of the case; or whether you would also object to their making experiments in a physiological laboratory under the direction of a professor of physiology. Does your objection apply to both of those cases ?—No, but I am in a difficulty in answering questions on the subject because "experiments" cover such enormous ground. I do not suppose anybody would object to a student looking at the circulation in a frog's web, or watching the effect of a grain of mustard seed placed on the frog's web under the microscope. These are very important things to be understood, and we cannot too early accustom our students to observe them.

4573. May I interrupt you so far as to ask you to suppose that an objection has been raised here to the study of the circulation in the frog's web ?—Some of these experiments are absolutely free from pain. You cannot say probably that when a grain of mustard seed is placed on the web the frog is absolutely free from pain; but the pain is really so insignificant that we should have no objection at all to suffer it ourselves. Physiologists have over and over again subjected themselves to far more painful experiments; and the training of young men in a knowledge of the processes of acute inflammation is so important and the good to be obtained so great that I should be in favour of such experiments being done under the wise direction of a professor in whom you would have some confidence.

4574. (*Chairman.*) I understand you now to be speaking of an amount of pain so slight that if it would answer the purpose you would almost submit to it yourself ?—Yes; I have plunged my hand arm for some minutes into a pan full of melting ice and water, as Brown-Séquard has, and that involves torture; and I have given my own blood to be transfused, as Brown-Séquard also has; and I think that is the strongest proof that can be given that we have faith in our cause, that we believe we are doing good by experimentation.

4575. (*Mr. Hutton.*) Would you shrink from an exceedingly painful experiment for the sake of a mere physiological result, an experiment involving agony protracted for many hours for the sake of mere addition to physiological science ?—But we never know how great the result may be. I would not submit to it if I did not think there was some good in it. It is not from idle curiosity that this is done, but the man of science never knows how great the result may be. When Galvani first saw the frog's legs jumping by electricity he never knew that the result would be to bind the whole world together by the electric telegraph.

4576. But the result of that logic is that supposing any number of painful experiments, even of an agonizing kind (and we have had many such named before this Commission which have been tried abroad in a true scientific spirit), the prospect of the pain caused to the animals would not be a sufficient consideration

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to induce you to object to them at all?—I do not say that. That is what makes it so difficult to draw the line. One person may estimate the result as very important, and another person may estimate the result as very unimportant.

4577. Supposing anything like a physiological inspection was carried out, that a really great physiologist were appointed to prohibit experiments which he thought useless, of course in that case he might prohibit a great many that would be really useful, without intending to do so; but still would you not think that that would be desirable, considering that he would probably prohibit a very much larger number that would be really useless, and would cause a great deal of animal suffering?—It is precisely in that direction that I see the difficulty; because we are sometimes in ignorance with regard to the use of a discovery; it may be something enormously great, and it may be nothing.

4578. We are not in ignorance in regard to the pain, and supposing the experiments to be extremely painful, then you would be quite sure that you were getting rid of that evil by prohibiting them?—Yes, but you do not know what you may deprive mankind of. Chloroform was discovered by Dumas, the French chemist, by making a certain acid from the red ant. I suppose thousands of these creatures were distilled to get a little formic acid. When he made that discovery he never dreamt for one moment that the great blessing which chloroform has conferred upon mankind would be the result. The man of science never can know the precise benefit which will arise from the discovery of a scientific truth of that kind; he can only be led on by the hope that it will be a benefit some day. And that is what is really the difficulty in my mind, dealing with it in this point of view. I think it would be desirable to lessen the amount of suffering to animals, but that is where I see the very great difficulty of the subject of inspection.

4579. But supposing, as a great physiologist from Ireland has told us, that nine-tenths of these experiments are made by men who are really not competent to discover anything, and that only the remaining one-tenth are made by physiologists with the gift of discovery, would you not think that the enormous amount of pain avoided by prohibiting these nine-tenths would be more than an equivalent for the loss of a certain amount of discovery?—I should entirely differ from that assertion of the Physiologist from Ireland. I should not say for one moment that nine-tenths of the physiologists are people who have no distinct object in view.

4580. No gift of finding their object, no gift of discovery, I said?—Every man has a gift of discovery if he chooses to be a careful observer. The experimental physiologists of Europe and America are a very small number of all the persons who engage in scientific investigations; but, certainly, if you take them all altogether (and I have known personally a great many of the most distinguished of them), none of them can be said to be deficient in the gift of discovery; there is in fact no such thing as a gift of discovery, it is a patient, painstaking carefulness, and a desire to record certain facts truthfully; and a very humble man with limited intelligence may possess that. In fact I think it was Galvani's wife who saw the frog jump upon the plate, and made the discovery which has done everything for us in electricity. She was a delicate lady and was ordered frogs; they were upon a plate in the laboratory, and she drew her husband's attention to the fact that a spark from the machine made them jump.

4581. (*Mr. Huxley.*) You would be quite familiar with the publications in Pflüger's Archives, with those in Brown-Séquard's Journal, and in fact with the periodical publications on physiology and physiological subjects?—Yes.

4582. Now taking the papers which appear in those publications, would it not be an entirely misleading statement to say that not one-tenth of those are of any value?—I should certainly differ entirely from any such assertion.

4583. If one put it the other way and said that it is not one-tenth that would turn out to be of no value, that would be nearer the fact?—I should think that would be nearer the truth.

4584. (*Chairman.*) You do not like to give general answers upon the relative importance of experiments as compared with the pain which they inflict; you would prefer, I suppose, to know the precise case before you gave a positive opinion?—I should.

4585. But I do not understand you at all to differ from what you said in a former part of your evidence, namely, that of the experiments which you think ought to be made upon living animals, by far the majority may be made under complete anæsthesia?—Yes, clearly.

4586. That of the remainder, where complete anæsthesia cannot be obtained, many experiments are of a nature in which the greater part of the pain may be removed by anæsthetics, and the remaining part will be very small?—Yes.

4587. That what remains after those two portions have been deducted is chiefly the class of experiments upon sensation; that those are very rare, that where the result has been sufficiently verified before, you object to repetition; and that in the few cases which still remain you do not contemplate protracted agony. Have I rightly represented your views?—Exactly.

4588. That as regards the protection of the physiologist from undue suspicion on the part of the public, you think legislation would be desirable?—I do. I think legislation would be desirable, although I see great difficulties in the way of carrying it practically into effect.

4589. And that unless it could be carried into effect in such a manner as to prevent its interfering with the progress of scientific discovery in the hands of the most competent persons, you would hesitate to give it your approval?—Yes.

4590. But that if a mode of carrying it into effect were devised which should be free from that objection, you would then think it a benefit to the highest persons in science?—I should.

Mr. T. Hayden.

Mr. Thomas Hayden called in and examined.

4591. (*Chairman.*) I think you are professor of anatomy and physiology in the Catholic University of Ireland?—Yes.

4592. Are you a practising physician?—Yes.

4593. And a fellow of the College of Physicians?—Yes.

4594. Have you paid attention to the subject which is referred to us?—I have.

4595. Would you be so good as to state your views upon it to us?—I have been for some 20 years teaching anatomy and physiology. In the early part of my tenure of office as professor I thought it my duty to perform experiments upon animals, with a view to satisfying myself in regard to certain cardinal points connected with the science of physiology. I performed the experiments alluded to upon dogs, rabbits, the lobster, et cetera.

4596. Were these experiments performed under anæsthetics?—All those upon dogs and rabbits were performed under the influence of chloroform.

4597. And with regard to the lower animals?—No anæsthetic was used in the case of those animals. I doubt whether in their case anæsthetics are of any value. The experiments which I performed had reference to the rhythm, or cyclical movements of the heart, the peristaltic action of the intestines, and the rate of transmission of the contents through the intestinal canal. They had reference also to the effect upon the elaborated chyle, the product of digestion, produced by various substances containing fats and sugars, and substances not containing fats and sugars. These experiments were, most of them, performed in the presence of my class; but having satisfied myself upon the points that I wished to be satisfied upon, I ceased to perform these experiments, and for many years past I have not performed any upon living animals.

4598. Then I need scarcely ask you whether, for the instruction of students, you consider it necessary to perform experiments upon living animals?—My conviction is that it is not necessary; and furthermore, I may take the liberty of remarking that I do not think the majority of students profit much by it.

4599. You think that the profit to be derived from it arises at a later period of their professional career?—Yes, or when they are senior students.

4600. Now, you have said that your own experiments, so far as they relate to the higher class of animals, were all performed under chloroform or other anæsthetics?—Yes.

4601. Is it your opinion that now that chloroform has been discovered, it is necessary for educational purposes to subject animals to any experiments at all in which they are conscious of pain?—I do think it is necessary, but within limits. I think those experiments should have reference to subjects in which there is still something to be discovered. I may exemplify that by referring to the experiments which are now being performed with such benefit to science by Professor Ferrier in London, and by Professor Rutherford in Edinburgh, by the latter in regard to the secretion of bile, and by the former in regard to the convolutions of the brain. That is quite new ground, and I conceive it quite impossible without detriment to science to dispense with experiments in these departments.

4602. By that you do not mean to say of Professor Ferrier's experiments that those which appear to an unlearned person who reads the description to be very painful, are really performed while the animal is in a state of consciousness, do you?—I do not know that he has been in the habit of using anæsthetics in all his experiments; I am not quite aware as to that; I am not informed as to details; but it is well known that experiments performed on the surface of the brain are not attended with very great pain.

4603. Then may I say generally that your own opinion is that for the instruction of young students experiments giving pain to animals are unnecessary?—I would say so. There may be exceptions, but they are not at present before my mind.

4604. We have understood that in Ireland, among the educated portion of the Irish people, there is a very strong feeling on this subject; is that your view?—Yes, I am aware that there is a strong feeling.

4605. That the notion of subjecting animals to great and protracted suffering would be abhorrent to them?—Yes.

4606. Are there any other observations which you wish to make to us?—None that occur to me now.

The witness withdrew.

Dr. John Cleland, M.D., called in and examined.

4607. (*Chairman.*) Are you professor of anatomy and physiology at Galway?—Yes.

4608. Have you paid attention to the subject which has been referred to this Commission?—I have paid attention to it.

4609. Would you state to us the extent to which you have been acquainted with the practice of experimenting upon live animals?—I do not profess to be a vivisector myself; that is to say, I practice it to a very small extent; to some extent, but not to any considerable extent. I am familiar with the work that it has done for physiology, and I am familiar with the practice in the hands of others. For example, I have seen vivisection carried out in Leipsic, where there is the greatest facility for experiment probably that there is in any school.

4610. Have you been accustomed in your teaching to exhibit experiments upon living animals?—I have never exhibited experiments on any animal higher than a frog, I am sorry to say, but the reason is that I have not the facilities. You will observe that I am professor of anatomy as well as of physiology. The subjects are so very great that they are in reality more than any one man can fully overtake. The consequence is that I have to select my methods as well as my matter, and that is the reason why, as I have mentioned to your Lordship, I have not carried out vivisection myself.

4611. You have formed, I daresay, an opinion whether, when experiments are performed upon living animals, for the purpose of demonstration to students, they ought or ought not to be limited to such experiments as can be performed under anæsthetics?—I am not prepared to say that they should be limited in any way. I think that that must be left entirely to the teacher's discretion. Undoubtedly anæsthetics ought to be given in every vivisection where it is possible; that is to say, where it does not interfere with the object of the experiment; but I would be most unwilling to lay down any hard rule on the subject.

4612. Would the number that can be performed under complete anæsthesia be the greater number of experiments?—Yes, probably.

4613. Whether for original research or for demonstration to students?—I should say that the majority can be performed under an anæsthetic; that is to say confining ourselves to experiments on mammals.

4614. Have you ever considered how far frogs can be anæsthetised?—I have tried to make up my mind how far the frog feels at all; and I own that I have not come to any conclusion on the subject.

4615. We had a witness a little while ago who told us that he can anæsthetise frogs and tadpoles; you being in doubt whether they are sentient or not, would you prefer to employ such a method?—I frankly own that it never occurred to me to apply anæsthesia to a frog. One would not think of applying anæsthesia to a worm before using it for a bait in fishing.

4616. Then you admit that you are in doubt whether frogs feel or not?—I have very little doubt indeed that they do not feel pain in a sense comparable with that in which we are familiar with it ourselves, because it seems impossible that they can have consciousness at all comparable with ours; the degree of consciousness that they have I cannot say. I believe that consciousness goes down a great deal lower than the frog. But these are matters of speculation. What I feel sure of is that a frog's consciousness is such a twilight thing compared with our consciousness, that it is a difficult matter to estimate its degree.

4617. But you are of opinion that in mammals at least anæsthetics should be employed?—Whenever they can be.

4618. Have you ever considered whether any limitation ought to be imposed upon the power of teachers to perform painful experiments?—I have considered that, but have come to the conclusion that there ought not to be any.

4619. You think that every teacher should be at liberty to perform any experiments, painful or otherwise?—I referred to those whom one knows in schools usually as qualified teachers. By that I mean men who, from various considerations, may be supposed to be familiar with the subject that they are teaching.

4620. But if they were not so qualified, then you think they ought to be restrained?—It does not seem to me that further restraint is required than that which is provided for already by the Cruelty to Animals Act. The same punishment that would come into operation in the case of a boy being cruel to a cat for play would, I fancy, reach everything that requires to be reached. I cannot say that, after thinking over it very much, I see any reason for legislation on the subject.

4621. Are you aware that the Cruelty Acts only apply to domestic animals?—No, I did not know that. However, I suppose, a rabbit might be considered a domestic animal, and a dog certainly is.

4622. Then you think that at any rate some protection ought to be given to rabbits?—I think that there is no further need of protection for rabbits, the tame rabbits that are always used for these purposes, than what would be afforded by their being considered as domestic animals, and therefore as coming under the general Act.

4623. Would the same observation apply to guinea pigs?—I presume so; they are tamed animals.

4624. And to rats?—To tame an animal and keep it and feed it is, to a certain extent, to domesticate it. I think there would be no great stretch, therefore, in applying it to rats so tamed.

4625. But supposing that rats are not domestic animals, do you think that if it were proposed to leave anybody free to perform experiments upon rats, there would be any necessity for any change in the law?—I see no objection to including all mammals under the present Act; to widen the present Act to that extent.

4626. (*Lord Winmarleigh.*) What makes you believe that the frog is less sensitive to pain than one of the higher animals?—There are the anatomical considerations to begin with, the conformation of its brain and its spinal cord, the nervous centres. With regard to the conformation of the brain it has a very low organization in fact.

4627. You have convinced yourself of that by your own examination, not by what you have heard, I presume?—I am familiar with the anatomy of a frog's brain.

4628. That is from your own examination?—Yes.

4629. It is not from anything that you have read or collected of the opinions of others?—I cannot at the present moment say that I have actually made a careful dissection of a frog's brain; but I have of so many animals both higher and lower, and the anatomy of a frog's brain is so thoroughly well known, that I cannot imagine that the gap in my own personal dissections can make any difference. I have no rational doubt of the anatomy of a frog's brain.

4630. What is the exact circumstance which has chiefly led you to believe that the frog is less sensitive to pain than other animals?—The cerebral hemispheres in the frog are very slightly developed; and as you pass to animals that have less and less complex brains, you have got less and less sign of intelligence; and further, we know by experiments which have been made on both birds and mammals, that if you remove that part of the brain called the cerebral hemispheres in portions, the more that is removed the fewer signs of consciousness are left; and I do not think that there is any reason to believe, from the experiments that have been made on the subject by Flourens and others, that a bird that has had its cerebral hemispheres removed has anything that can be said to be comparable with our consciousness.

4631. I understand then that you have derived this opinion from the writings of others?—It is one of the things that we have from vivisections.

4632. The object of my inquiry was to ascertain whether your own experiments had led you to coincide with that opinion?—I have not made vivisections myself on the subject; I should not wish to put the results of any vivisections of my own before the Commission. With regard to frogs I may observe that, on the assumption that they feel pain in the same way that we do, we have a right to suppose that the seat of their feeling is in the brain; and in the great majority of experiments that are made on frogs we are in the habit of commencing by either pithing the animal or decapitating it. It may be my fault, but I never thought of including this practice under the head of giving an anæsthetic to a frog; I should certainly at once decapitate it or pith it, when that could be done.

4633. (*Sir J. B. Karslake.*) How long have you been engaged in lecturing to pupils?—One way or other I have been engaged in lecturing since 1857. I began as demonstrator of anatomy in 1857; I have held my present chair for 12 years.

4634. That is to say you commenced as professor of physiology 12 years ago?—Yes; at Christmas 1863.

4635. Do I understand you rightly to say that you have never or only rarely practised experiments on living animals before the class?—Only rarely.

4636. Did you say that you regretted that you were not able to do it to a greater extent?—I do say so.

4637. In your judgment is it essential for the satisfactory education of the class that they should see a further number of experiments upon the living animal?—I think that it would be more satisfactory. I think that I should do my duty still better to them than I do if I had the opportunity to illustrate much more fully than I do by means of actual experiments.

4638. Do you think that reading alone will supply that want?—I do not think that reading in anything will supply the want of actual observation on the part of students; thus, in anatomy, the other subject which I have to teach, no amount of reading can take the place of dissection of the human body. I give that as an example of the general proposition which I lay down, that direct observation is desirable where it is possible.

4639. And you think that it is as necessary to carry that on by experiments on living animals, if you can possibly do so?—The object of my teaching physiology is to educate medical students. I have got a great deal to teach in the six months course, and I must select my matter. I think that I should be doing wrong if I missed out a great deal of the matter that I now give them, to occupy them with experiments that require a great deal of time; but for any student who has got a knowledge of physiology otherwise, to have added to his information the observation of experiments on animals, I consider a very great advantage.

4640. (*Mr. Huxley.*) I presume, from what you have just said, that the curriculum of medical education is, in your judgment, very heavily weighted at present?—It is.

4641. And, therefore, whatever might be your desire as to the thorough teaching of physiology, I understand you to say that you would hardly feel yourself justified in occupying so much of the students time upon that subject as you would need to do if you taught it practically, having regard to the other things that they have to learn?—Quite so.

4642. But that does not in any way diminish in your judgment what I may call the abstract importance of demonstrative teaching?—It does not; besides, I frankly own that such teaching as mine does not produce physiologists. I produce medical men, but I do not produce physiologists.

4643. I would now ask you a question which might perhaps surprise you, but you will understand that I have a motive for asking it. If the alternative were put to you in experimenting upon one of the higher animals, in a case in which it was possible to use anæsthetics, whether you would use them, or whether you would not, would you have any hesitation as to what you would do?—If the experiment would not suffer by it, in the case of a mammal, I should use the anæsthetic.

4644. That would be a matter of course, would it?—Yes, certainly. The first experiments of the sort which I made many years ago (I was only operating along with another), were made on cats; and although there were very few of them (I cannot now tax my memory as to the number, for it was many years ago), and although it was a matter of sensation

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that we had to deal with, we gave chloroform to the cats before the performance of the operation, and then when the effect of the chloroform went off, tested the presence or loss of sensation in the parts supplied by the nerves that had been divided.

4645. You have not only been for some years professor of physiology, but you were for a long time I think one of the demonstrators in the Great Anatomical School of Edinburgh, were you not?—Yes.

4646. And you must have had an opportunity of seeing a great deal of what is done there?—Yes.

4647. Now did it ever come to your knowledge that a person pursuing an experimental inquiry would, in such an alternative as I have just placed before you, hesitate for a moment as to what he would do?—I do not believe he would.

4648. At any rate he would not justify it if he did? —He would not. It is perfectly possible that he might have something to do which he thought would give so little pain, that he might deal with it in the same way as we surgeons deal with cases every day of our lives, as I have often dealt myself in operating on living human beings, namely, take into consideration whether the amount of pain that was to be given was worse to bear than the giving of the anæsthetic. I am quite sure that anything that could be by any twisting of the word called torturing an animal would not be done without giving an anæsthetic. That is my own opinion.

4649. Have you seen any of the proposals that have been brought forward for the regulation of vivisection for scientific purposes?—I have not.

4650. (*Mr. Hutton.*) As to demonstrative experiments generally, has your attention been called to this Handbook of Physiology edited by Dr. Burdon-Sanderson, that has been recently published?—Yes, I am familiar with it.

4651. It contains a great number of painful experiments. Would you regard the larger number of those experiments as fit to show a class by way of demonstration?—If I were lecturing myself, and exercising my own judgment, it would be a very select class indeed to which I should show a very great number of those experiments; and that for the reason which I have already mentioned, that the great majority of students have their time taken up so much with other things. But if I were bent upon sending out thorough physiologists, I think I could have little doubt in considering that handbook as a very useful one to go by.

4652. And that even for purposes of demonstration and teaching; not for purposes, of inquiry, but for those of demonstration?—Yes; that they might see, and not take things on hearsay.

4653. Did I not understand you to say that you had studied under Ludwig, at Leipsic?—I said that I had seen their method of pursuing studies there. I have not myself studied under Ludwig.

4654. There, I suppose, a very much larger number of painful experiments are used in demonstration than are generally used in England and Scotland?—The majority of the experiments that are performed in the Leipsic establishment are for purposes of inquiry, for purposes of research, but they do also illustrate before a class.

4655. And you are in favour of extending the system, at least for classes of physiologists as distinguished from medical men, I understand, of a much larger demonstration of painful experiments?—I am in favour of every professor of physiology having nothing but physiology to teach; and where he has nothing else to teach I am sure that he would be better able to demonstrate it by vivisection than I am able to do; but from various motives the chairs of anatomy and physiology are combined in the Queen's Colleges.

4656. That you consider a great evil, as I understand?—I think it is too much for any one man to teach.

4657. But if you had a separate class, for separately educating physiologists as distinguished from medical students, you would not scruple to use a great number of painful experiments by way of demonstration?—I should certainly consider it my duty to perform certain experiments.

4658. (*Chairman.*) Did I rightly understand you to say, with regard to the frog, that though you did not administer anæsthetics, you either what is called pithed the frog, or removed the brain, or took away the sensibility of the frog in some manner of that sort? —Yes. I did not understand the question about anæsthetics to reach to that; I do not say but what the word may be used as applying to these procedures, but it did not occur to me at the time. I thought of giving chloroform, and so on.

4659. It did occur to you, however, as I understand, to remove whatever sensibility there may be in the frog by removing the brain?—Yes.

4660. You have just been asked whether you would, if you were a teacher of physiology only, perform a considerable number of experiments of a painful nature; but I want to know whether the consciousness of the mammal would not be removed by chloroform, or some other anæsthetic, before the experiment was performed?—Certainly, wherever that could be done; but I think there are painful experiments that may be performed even on mammals. I am not prepared to limit even the performing of painful experiments; I would not lay down a rule; I think it is for each physiologist to judge for himself. I think that the more educated physiologists are the better they will be able to perform experiments in such a way as to get the knowledge with as little pain to the animal as possible. It is always an object to damage the animal as little as possible in an experiment, and I think it is quite possible that in the present, I may call it transition stage of physiology in this country, there is many an animal treated with less skill than it would be if physiology were as well studied as it is in a number of Continental schools.

The witness withdrew.

Adjourned to to-morrow at 2 o'clock.

Wednesday, 3rd November 1875.

PRESENT:

THE RIGHT HON. VISCOUNT CARDWELL, IN THE CHAIR.

The Right Hon. Lord WINMARLEIGH.
Sir J. B. KARSLAKE, M.P.
THOMAS HENRY HUXLEY, Esq.
JOHN ERIC ERICHSEN, Esq.
RICHARD HOLT HUTTON, Esq.
N. BAKER, Esq., Secretary.

Mr. CHARLES DARWIN called in and examined.

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4661. (*Chairman.*) We are very sensible of your kindness in coming at some sacrifice to yourself to express your opinions to the Commission. We attribute it to the great interest which we know you take in the subject referred to us, both on the score of science and also on the score of humanity?—Yes, I have felt a great interest in it.

4662. I think you took part in preparing the reso-

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lutions of the British Association at their meeting in Edinburgh in 1871?—No; I had nothing to do with that. I was very glad to see them, and approved of them; but I had nothing to do with the framing of those resolutions; I did not attend the meeting.

4663. But you signed a petition which embodied them?—When they were sent to me I may have done so. I do not remember it; but if my signature is attached I must have given it; I had forgotten it.

4664. But you cordially approved of them?—I cordially approved of them. I had occasion to read them over lately at the time when this subject was beginning to be agitated. I read them over with care and highly approved of them then.

4665. I think you took some part in the preparation of a Bill which was ultimately laid before the House of Commons by Dr. Lyon Playfair?—In the steps preparatory to that Bill, but the Bill itself did not exactly express the conclusions at which after consultation with several physiologists we arrived; I apprehend that it was accidentally altered.

4666. But in the main you were an approving party?—In the main.

4667. You have never, I think, yourself, either directly or indirectly been connected with the practice of trying experiments upon living animals?—Never.

4668. Will you have the kindness to state to us the views which you desire to lay before the Commission in connexion with it?—The first thing that I would say is, that I am fully convinced that physiology can progress only by the aid of experiments on living animals. I cannot think of any one step which has been made in physiology without that aid. No doubt many surmises with regard to the circulation of the blood could be formed from the position of the valves in the veins, and so forth, but certainty such as is required for the progress of any science can be arrived at in the case of physiology only by means of experiments on living animals.

4669. Then I need hardly ask you what your opinion is as to the notion of prohibiting them altogether?—In my opinion it would be a very great evil, because many reasons, mostly general, but some special, may be assigned for a full conviction that hereafter physiology cannot fail to confer the highest benefits on mankind. Many grounds, I think, can be assigned for this conviction.

4670. Is it your opinion that most of the experiments can be performed while the animal is entirely insensible to pain?—That is my belief; but I ought to state that I have no claim to rank as a physiologist. I have, during many years, read largely on the subject, both general treatises and special papers, and in that respect I have gained some general knowledge, but as I have said, I have no claim to be called a physiologist, and I have had nothing to do in teaching physiology; but from all I can learn, the exceptions are extremely few in which an animal could not be experimented on in a state of entire insensibility.

4671. Then to hesitate to perform experiments, though painful in their nature, when the animal was rendered insensible, would not be, in your opinion, a judicious course to recommend to the Queen and Parliament?—Certainly not. It is unintelligible to me how anybody could object to such experiments. I can understand a Hindoo, who would object to an animal being slaughtered for food, disapproving of such experiments, but it is absolutely unintelligible to me on what ground the objection is made in this country.

4672. Now with regard to trying a painful experiment without anæsthetics, when the same experiment could be made with anæsthetics, or, in short, inflicting any pain that was not absolutely necessary upon any animal, what would be your view on that subject?—It deserves detestation and abhorrence.

The witness withdrew.

Mr. F. Sibson, M.D.

Mr. Francis Sibson, M.D., called in and examined.

4673. (*Chairman.*) Are you consulting physician to St. Mary's Hospital?—Yes.

4674. And a member of the Senate of the University of London?—Yes.

4675. And a Fellow of the Royal Society?—Yes.

4676. Now you have paid a great deal of attention, I think, to the subject which has been referred to us?—I have paid a certain amount of attention to it. Many years ago I did myself perform a few experiments, and within the last 12 years I have performed one short series.

4677. What is your opinion as to the necessity of such experiments?—I would say, as a physician and physiologist, that it does not admit of opinion, but that it is a certainty that experiments are absolutely necessary for the progress of medicine, including all its branches.

4678. The introduction of anæsthetics, I presume, has made a very great difference in the degree of pain to which animals are subjected?—A very great difference. The greater number of experiments that would be performed for the purposes of inquiring into the nature of disease, and the treatment of disease, would be performed under the action of anæsthetics; and I would say the same of those experiments, a great number at all events of those experiments, that are performed in pursuit of pure physiology; but of course in certain circumstances to use an anæsthetic would defeat the object of the experiment.

4679. Are those occasions rare?—Well, you see, I have never been a systematic physiologist, and therefore I should be speaking out of my province if I were to say whether they are rare or common. I should not feel myself competent to give an answer to that question. Most certainly in those points that come into my own cognizance every experiment which we as physicians can desire to perform to throw light upon disease, and the treatment of disease, can either be done under the influence of anæsthetics, or, so to speak, the experiment would be so trivial in its nature as not to require any agency so serious.

4680. That is to say, that the inoculation of an animal for the purpose of communicating to it a disease may no more deserve the use of chloroform than the vaccination of a child?—Certainly.

4681. But in that case the pain and inconvenience is consequent inconvenience, that is to say, being affected with the malady?—Yes, being affected with the malady.

4682. Now when animals are so affected for this purpose, is it necessary to keep them in distress for a long period of time?—It would depend entirely upon the nature of the inquiry, and the object. Of course if it is to inquire into a disease, let us take the instance of the cattle plague; the disease must run its course. If it is to inquire into diphtheria, erysipelas, small-pox, cow-pox, and the various diseases of that class, that have been inquired into, clearly each of them must run its course. But I need not tell your Lordship that the process of a disease is not one of constant pain, but very much the reverse in most instances; and as a physician I can say that in walking round the wards of a hospital, I very rarely see now, or did see when I was in the daily habit of going there, any trace of suffering on the face of any patient, particularly of a fever patient; because, so to speak, there is an anæsthetic within them in the very poison that saturates their blood and solid tissues and nerves; therefore they are lying there scarcely more than just conscious. Therefore I would say that that which applies to mankind applies also to animals; and that they are anæsthetised by the very malady that is produced, as a rule; I do not say in every instance.

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4683. Then your opinion is, that in experiments for the purpose of observing disease, the original operation is so slight that in a human being it would be disregarded ?—Quite so; it is scarcely felt.

4684. I understand you that the subsequent distress is in most instances not in your judgment excessive ?—No, I do not think it is, for the reasons I have given; I do not say in every case; as, for instance, in erysipelas; but the suffering from erysipelas is not great. I am taking now those involving the greatest amount of pain. If you come to a boil or a carbuncle, we do not give that in experiments. Scarlet fever gives no pain; certainly if it attacks the throat it does, but that disease is not produced in experiments.

4685. I need not ask you whether, in your judgment, whenever an animal is used for the purpose to which you have referred, the most humane possible treatment should be adopted ?—The most humane treatment should be adopted.

4686. Or whether the persons who are qualified to adopt the most humane treatment are not those who are most skilled in the calling ?—Precisely so. With regard to the question of the spirit that animates the operator, perhaps you would allow me to read a few words that have been put into my hands by one of the most distinguished experimental physiologists with regard to that very point.

4687. May we take them as representing your own views ?—Yes. "The 'rule of kindness' guides us" (that is the physiologists) "just as it guides other " good and reasonable men in dealing with animals. " In making the experiments, we" (the physiologists) " choose our method so as to give as little pain as we " can, and we spend no end of pains in planning and " preparing for them, so that we may obtain out of " each trial the utmost information that it is capable " of giving, and hence to avoid the need of repetition."

4688. Now has the question ever occupied your mind whether any measures might be adopted for limiting such experiments to the most highly qualified persons ?—I have thought the question over; I think that if you were to put anything like a strict limitation on this matter you would, so to speak, clip the wings of the advance of the science of medicine in its highest sense.

4689. But do you suppose that the science of medicine is advanced by inquiries conducted by incompetent and uninstructed people ?—No, certainly not.

4690. If then any restriction that were proposed kept clear of interfering with any but that latter class, it would not be open in your mind to any objection, I suppose ?—I am not fond myself of restrictions; I think that in this country we get on far better by being governed by our own conscience, our own sense of what is right, and public opinion. I think those are far better rules than any minute rules that may be laid down, such as would interfere perhaps with some of the happiest lines of research that have ever been undertaken; and perhaps put a stop to some of the most important investigations and prevent their being undertaken; and do nothing whatever to promote either humanity or the welfare of man by the advance of the healing art.

4691. But that is a rather large answer, and would include a great deal. We are very happy to live in a country which consists of very honest people, and to have a very sound public opinion; but we not only maintain our old laws, but we add a large volume every session to the laws by which we are governed; and I do not understand you to desire that we should entirely repeal all those Acts of Parliament ?—I do not quite see, if you will allow me to say so, how that applies to what I have been saying.

4692. I only applied it to what I considered the comprehensiveness of your answer in which I think you said that we get on best in this country by trusting to public opinion and to our own consciences. Now we have an Act of Parliament which prevents cruelty to animals in certain circumstances; would you object to that Act ?—No, certainly not.

4693. Then supposing that it were proved that there were abuses in regard to experiments upon living creatures, would you object to a law for the correction of those abuses ?—If there were abuses I should certainly not object to a law for the correction of real abuses.

4694. But you think there is no evidence that such abuses do exist ?—I am not at all of opinion that such abuses do exist to a sufficient extent for them to be called abuses requiring legal interference.

4695. But if there were such abuses, then you would think it reasonable to have legislation to restrict them ?—Yes, in a reasonable manner.

4696. (*Mr. Huxley.*) I think that you have been, at some part of your life, if you are not now engaged in teaching ?—Yes, I have taught for some 20 years.

4697. In what branches of medical science did you teach ?—The practice of medicine and clinical medicine.

4698. You have naturally paid great attention to the progress of medical education ?—Very great indeed; it has been part of my daily interest.

4699. I suppose I shall not be wrong in assuming that you consider the increase of practical teaching in the medical schools, in all branches of medical science, as one of the great advances that have been made of late years ?—That is the great advance.

4700. One of the great changes that have taken place ?—Yes, that is the vital change and the vital improvement.

4701. Not only do the examining bodies now insist upon practical study in medicine and surgery, but they also insist upon it in chemistry, do they not ?—Yes.

4702. And arrangements have been made now in all well organised medical schools in which such practical instruction can be given ?—It is so everywhere.

4703. And in accordance with this general advance in the improvement of medical teaching, practical teaching has been introduced into physiology, has it not ?—It has; within the last three or four years physiology has been taught in the schools in a totally different manner from what it was formerly.

4704. May I ask whether you consider that an improvement upon the old style of teaching or not ?—Yes; indeed I think that the old style was scarcely teaching at all; this is really teaching.

4705. And you attach very great weight, I apprehend from that answer, to giving to students such demonstrations as are requisite to put before their minds clearly the fundamental truths of physiology ?—I should put the work in a physiological laboratory and the work in a physiological lecture room precisely in the same position with the work in the medical lecture room, and the work in the wards. It would be as impossible to teach physiology in a lecture room as it would be to teach the practice of medicine in a lecture room.

4706. I presume that, as a scientific practitioner of medicine, you regard an accurate knowledge of the structure and functions of the healthy organism as the foundation of all your knowledge on the subject ?—It is the only certain foundation of a knowledge of disease and its treatment; and in every section of physiology, every single part of the body, the knowledge, and the experimental knowledge of that part leads up to the practical knowledge of the diseases of that part, and of the treatment of those diseases.

4707. Supposing that you, as a physician, wished to understand an abnormal action of some organ, to understand it thoroughly and scientifically, I mean, I presume you would hardly be content with a mere reading knowledge of the normal functions of the organ, but you would go to the thing at first hand, and make out for yourself what the exact basis of those statements was ?—It would be quite necessary to do so; I should not have the least idea of the life within unless I had studied it as an actual living thing elsewhere.

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4708. And the confidence with which you would reason from healthy processes to abnormal processes where you had such first-hand knowledge would be very different indeed from that which you would reason if you only had second-hand reading knowledge?—In the one instance I should have knowledge, and in the other it would be a matter of guess work.

4709. I think you have yourself made elaborate investigations upon the normal working of the organs of respiration, have you not?—Yes, I have, a great many years ago.

4710. May I ask whether the knowledge acquired in that way has been of service to you in your professional career?—It has been of the greatest service to me in the study of disease. I began that inquiry in order to study with greater advantage the diseases of the chest, and I pursued it with that in view; and to this hour, I may say every day of my life, the work that I did then I find to be of clinical profit to me.

4711. Then if any legislative measure were adopted which would seriously impede the demonstative teaching of physiology, I gather, from what you have stated, that you would consider it an evil?—It would be turning back the student into the old paths which we now feel to be no longer tenable. The old teaching was not teaching—we all feel that now. In our younger days, unless we did the thing for ourselves, as you very well know, we did not get a true teaching of any single structure in the body; we saw a series of dissections and pictures, but there was no true idea in our minds either of the actual structure or of the living organs which were contained in the body.

4712. I suppose your recollection will carry you back to the time, or at any rate you must know of the time, when even the knowledge of anatomy required by the examining bodies was of an astonishingly minute character?—Yes, it was very slight indeed in my early days.

4713. So that the change which has come over physiological teaching is simply a part of the great change which has swept over medical teaching in general?—It is a part of it, and it came in late. All the rest had got into a practical state when physiology was still merely being taught orally.

4714. It is the fact, is it not, that a great majority of the experiments which are required for demonstration of the fundamental facts of physiology can be quite well carried on under anæsthetics?—I believe so, or under parallel influences, or else they are experiments not requiring them.

4715. You have had the personal acquaintance and friendship of a great many physiological experimenters, have you not?—I have intimately.

4716. May I ask you, from your own knowledge of these persons and from your own work, whether it is not the fact that while they might possibly be inclined to pursue any line of experimentation which would be likely to yield valuable results, whether it involved pain or not, yet all the persons you have known would regard it as a matter of morality, I was about to say, to inflict no useless pain?—I am quite sure that all of them, without exception, are guided by humanity and kindness towards the animal they are experimenting upon as the leading principle, and their desire is to inflict the least pain that they can.

4717. Even if a man had, by carelessness or misadventure, made an experiment which was painful when it was possible that the animal might have been made insensible, if the fact were brought to his mind he would be ready to admit that it was a wrong thing?—Certainly.

4718. (*Mr. Erichsen.*) Have you ever found any English physiologist advocate the doctrine that the animal should only be anæsthetised for the physiologist's convenience irrespective of the relief of suffering to the animal itself?—No, I have never heard it.

4719. Such a doctrine is utterly unknown, so far as you know, in the schools and amongst the physiologists of this country?—Quite unknown.

4720. (*Chairman.*) And such a doctrine, if it obtained any footing in this country, would merit repression by legislative interference, I presume?—I cannot conceive it possible that such a spirit should come into this country. If it did come it must be repressed.

4721. (*Mr. Erichsen.*) Having been a teacher in one of the large schools of medicine in London for a very considerable number of years, do you believe that the students of that school, taking them as a sample of the medical students in London generally, would tolerate practices founded upon such a doctrine being carried out before them?—I am quite sure that they would not, judging from the spirit of humanity, and I would say a remarkable fondness among all the young men that I was acquainted with for the domestic creatures that they come in contact with.

4722. Many of the medical students, a very large number of them, come from the country, do they not?—Yes.

4723. And in the country they are addicted during their vacations to field sports and ordinary amusements as much as any other young men?—Yes.

4744. And they would be as likely as any other set of young men in your opinion to repudiate any unnecessary cruelty inflicted upon animals, such as dogs, horses, &c. with which they came in contact in other ways during the period of their vacations, and perhaps antecedent to the period of their studies in London?—Yes.

4725. So that I presume I may conclude that you would not think that an experiment would become attractive to medical students in proportion to its frightfulness?—I am sure that it would not; that it would be impossible to present a frightful experiment to them.

4726. That instead of being more attractive it would become less attractive?—It would be stopped by them I am quite sure.

4727. Many of the experiments that you made, I think, if I recollect right, had a direct bearing upon practical medicine?—Yes, I think they all had. One set was upon the action of opium; and I took part in another set as a member of the Chloroform Committee of the Medical and Chirurgical Society; another set was on the movements of the heart and the action of its cavities and valves, which were in direct and immediate relation to the diseases with which the heart is affected.

4728. And may I ask you whether it is the case that these experiments which you have made upon the action of the heart and its valves have or have not been quoted in works on disease of the heart as illustrative of various questions connected with the diseased action of the heart?—Yes, they have. A work just come out has given some of my experiments; in fact drawings which I published have been copied into the work.

4729. In speaking of the pathological aspect of experimentation on animals you stated that the majority of the diseases which were induced pathologically were not painful?—Yes.

4730. There is one disease that you have not mentioned, but which has been spoken of here as an extremely painful disease induced in animals, not that its induction is painful, but that the disease in its progress is attended with great suffering to the animal; I mean tuberculosis. Now is it your opinion that tuberculosis of the internal organs is a painful disease?—I know it not to be a painful disease, but a disease quite free from pain. I do not mean to say that in some occasional manifestations of inflammation there may not be pain; but it is remarkably free from pain, and you know tuberculous consumptive people are of all people most buoyant and full of life, and free from every idea of death, and so full of animal enjoyment that you cannot restrain them.

4731. Knowing that to be the case when the internal organs of men are saturated with tubercle, is there any reason to believe that when an animal has

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had tubercle communicated to it artificially it should suffer more than a human being who has either inherited that tubercle, or in whom, as it is now the opinion of some, that tubercle may have been implanted by actual contagion?—It is quite certain, indeed, it is a matter of scientific knowledge, that there is no difference between the tuberculosis so induced and that which is inherited or acquired by the habits of everyday life.

4732. So that I may take it as your opinion, as a physician of large experience, having seen much of tuberculosis, that tuberculosis of the internal organs in any way induced is not a painful disease?—Not a painful disease, but the very reverse.

4733. (*Mr. Hutton.*) I suppose you would think that the experimental method has increased very much in England since the time of Sir Charles Bell, would you not, as well as abroad?—I should say that it has certainly.

4734. If a method in general increases, I suppose you would expect its abuses to increase in the same proportion; if the number of experiments in all increased, the number of useless experiments would probably increase too, would it not?—That is one of those questions (you will pardon me, I hope, for saying so) that I think almost answer themselves. Confining myself to the subject in hand, I do not think that the increase of experiments has increased abuses in connexion with experiments.

4735. I mean simply that they have increased in the same proportion as the experiments themselves?—I do not know that.

4736. The number of useless experiments would probably increase in the same proportion as the number of useful ones, do not you think that that would be natural?—It may be natural, but it does not come into my observation; I would say so at once if it were so, but I have not seen it.

4737. I ask the question because Sir Charles Bell stated that in his time he thought the number of useless experiments performed was a large number, and we have had evidence from an intimate friend of his that to some extent that applies to England, namely to Mayo's experiments. Now, what was true in the time of Sir Charles Bell I suppose would be likely to be true in our own time?—I would say of Sir Charles Bell that what made his great discoveries incomplete was his neglecting to use those means that were necessary to establish them. Consequently, for the confirmation of his discoveries the experiments of other men had to be performed, and those not in England, but abroad. He having a prejudice in that respect, I should not myself regard Sir Charles Bell's statement upon that question as a statement of authority.

4738. You would attach very little authority to it?—I should attach no authority to it whatever on that point.

4739. (*Chairman.*) But if you were to compare Sir Charles Bell and M. Majendie with regard to the subject which we are inquiring into, you would scarcely hold up M. Majendie as the example which it would be desirable we should follow in this country?—I should say that you want a man quite different from either of them, not even between them, I should say much nearer to Sir Charles Bell than to Majendie. I would certainly say this of Majendie, that he was a masculine observer, and there was an object in every experiment that he did, but he was a ruthless repeater of experiments; and, if I may say so, I think one great reason for giving English physiologists the same right to make experiments that they have in France and in Germany, would be that they may set continental nations an example how to produce the same results in a scientific point of view with greater humanity. That has always been the aim of the English experimenter, and when Majendie was doing these ruthless things, Sir Charles Bell was erring in the opposite direction; and I believe it was the opposite polarity induced by the ruthlessness of Majendie which caused the over fastidiousness of Sir Charles Bell.

4740. (*Mr. Hutton.*) Are you acquainted at all with Dr. Mayo's experiments?—Yes.

4741. Do you regard them as useless, or as eminently useful?—They cannot be said to have been great experiments, they were in their way good at the time.

4742. They were justifiable experiments you think?—Certainly.

4743. And you think that all Majendie's were justifiable, only that their repetition was too frequent?—I think that Majendie might have made his experiments with much greater consideration for his animals. I do not think that the idea entered his mind that he had a suffering being under him. Therefore I think he might have made the same experiments with equally great results and with a less amount of suffering, and he repeated them too often; therefore I blame him for two points, not considering his animals, and injuring them too often.

4744. Now you estimate as very few the number of experiments in which material suffering is involved?—Yes.

4745. I suppose you would not deny that the sufferings involved in raising the temperature of animals till they died would be very severe?—That is a question that I happen to have paid a great deal of attention to, and I am not of opinion that it would produce great suffering, and for this reason, that by the time the animal acquires anything like a temperature of 110°, 111°, or 112°, at the outside (it may sometimes get up to 113° or 114°, but we have only one instance of it), the animal becomes unconscious.

4746. The intermediate period is one of great suffering, I suppose?—In the intermediate period the sufferings are not great. We see those cases in wards; the patient springs up to a high temperature within three days or two days, sometimes within six hours, from 102° or 103°, to 109° or 110°, and shows no mark of suffering or pain; he is in delirium, but he never complains of pain.

4747. But surely the difference between raising the temperature of the blood and raising the temperature of the whole air in which they are is very great?—Yes, but you know a hot air bath of 170° is not a matter of suffering.

4748. But it does not raise the temperature of the body to that?—The temperature of the body cannot be the temperature of the air in such a case, or we should die. If you raised the temperature of the body of the animal, it would probably die before it got to 111°. It would simply pass through the stages of a high fever with its delirium just as we do.

4749. Would you say the same of Mangili's experiments on freezing animals to death?—I would say very much the same kind of thing, nothing is more destructive of sensation than cold; and I have again and again told people who have cold hands, "You have got cold hands," and they have said that they had not. The nerves, when they are cold, lose sensibility; the best local anæsthetic that I know is cold; and in fact it is used as an anæsthetic; and one of the most popular anæsthetics for a short time was ether spray applied so as to freeze the surface which it played upon, and then the operator could cut into the frozen part. Therefore those experiments would be the reverse of painful.

4750. Would you say the same of Goltz's experiment of boiling a frog to death?—That is a horrible idea, and I am certainly not going to defend it.

4751. Would you say the same of Chossat's experiments of starving animals to death, that very little suffering was involved in those experiments?—I am very familiar with those experiments; I have been over them again and again, and I would say the very same of them, that there was very little suffering inflicted on the animals by the process of starvation which they were subjected to by Chossat. I would say with regard to those experiments that they were conducted with great care and skill, and with a sincere desire, I am quite sure, to spare as much as he

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could the suffering of the animal. He taught us that the withdrawal of food for a certain prolonged period inevitably led to such a lowering of the temperature of the body that the animal would cease to live; and would be incapable of being brought back into life again by food, unless the temperature were again raised. I need not say that that was a practical lesson to us as physicians which pretty nearly every patient we have had to do with has had more or less advantage from. It was considered that one of the greatest lessons that ever was yet given in physiology in relation to the treatment of disease was that lesson which Chossat gave us from the most careful starvation of these animals.

4752. What should you say of the experiment of establishing a biliary fistula not under anæsthetics, and trying the effect on dogs for eight or nine hours under curari?—I shall be very glad indeed to speak of that. I happen to be chairman of the Scientific Grants Committee of the British Medical Association, and we paid the cost of those experiments after they were performed. In my opinion (and I will avow that a physiologist may differ from it), under the influence of wourali after a short time pain is not experienced; and I hope I may now be allowed to say why. I have had an animal 7¼ hours under wourali myself, and brought it to life again. I have had another four hours; at the end of four hours it began to kick, and I then at once put it to death without stopping another moment. I have seen many animals under wourali, and I have made many experiments with it unpublished, but alluded to in publications, and therefore I speak of a subject that I do know. There is no question that under wourali there is complete paralysis of motion. There is no question also that there is no sign of sensation elicited from a limb through which the poison is circulating; but if you tie one of the arteries of the limb, then you may get reflex movements in that limb. Then further, Schiff is of opinion, from his observations, that the wourali poison in large doses not only paralyses the nerves of motion but also the nerves of sensation; that is the most recent view upon the subject, and I am more inclined to hold with that than with the other, although the nerves of motion are those that are strictly paralysed. Then again these animals under wourali lie there for hours subject to its influence. Granted that their sensation is not absolutely lost, but is capable of being roused by an adequate exciting cause, we know that when a person or an animal lies perfectly quiet, without one single movement of life (which a wuralised animal does,) even if there be the power of making that person feel, the whole mind and consciousness and sensibility gradually fades away; and in the course of perhaps eight or ten minutes after a person is in that still state, there would undoubtedly be no feeling whatever at the nervous centre, though there might be the power of rousing a reflex excitation. I would, therefore, on every consideration say that those experiments are perfectly justified, and that the wourali poison (I have thought it carefully over and studied the whole of those experiments as far as I have seen them) did completely annihilate the consciousness of the animal. At the same time that is a matter of my opinion.

4753. You are aware that experiments have been tried on man with wurari and that the effects have been just the opposite, that the suffering has been very great. That has been put in evidence before the Commission?—I decline to accept that.

4754. (*Chairman.*) Your justification of the particular experiments to which your attention was last directed, turns upon your belief that the wurari does in truth take away the sensibility to pain?—It does.

4755. (*Mr. Hutton.*) Since when do you know has it been believed and asserted amongst scientific men that wurari is something of an anæsthetic?—In a very careful lecture of Dr. Brunton's, one of his very valuable lectures on the pathology and treatment of diabetes, he relates Schiff's view as from his own knowledge.

4756. Do you remember the date of the paper?—I will send it down here.

4757. Can you give the date at all?—It is within the last year, but I will send the very paper. (The lecture was published in the British Medical Journal on the 10th of January 1874.)

4758. Has not this discovery been made in a great measure since wurari was treated by others who are opposed to vivisection as a thing that was not an anæsthetic, in fact, has it not been very much a discovery that was *motivé* by the wish to find out that wurari was an anæsthetic?—No, undoubtedly many physiologists practically work wurari and opiates together; that is, they put into the body both an opiate and wurari; and therefore they practically work upon the view which you put forth, namely, that it is possible that there may be pain there. Therefore from that point of view undoubtedly the painful part would not be defensible.

4759. As a matter of fact have not the speculations about the anæsthetic character of wurari all come up since the time to which I refer?—No. Dr. Brunton's edition of Schiff's view was before those experiments were made. I will send you down the journal and you can take it for yourself from the journal.

4760. Now supposing that wurari is in no respect an anæsthetic, would you say that a painful operation, lasting half an hour, and followed by experiments in which the bile ducts are acted on by medicines during eight and a half hours more, would be very painful or not?—I do not think they would be very painful. I am now looking at the experiments as they are, and putting aside the theory. I do not think they would be very painful because the animal would be perfectly at rest; there would be no struggling; the incisions would be made with great ease; the animals mind would be withdrawn from what I would call the domain of attention, and when there is no attention there is no sensation, no sensitiveness, no pain; and I do not believe that under those circumstances the animal would feel in the way that it would feel if it were not under the influence of wurari.

4761. Does it not come to this then, that the wish to find these experiments harmless, induces the judgment of scientific men to decide that experiments, which nothing would induce them to submit to for themselves, are painless when applied to other animals?—I do not at all admit that statement.

4762. (*Mr. Huxley.*) Now about this business of wurari which has come before us several times, I think the wurari question arose some twenty five years ago, did it not?—It was in 1843 (I have the book in my pocket) that I made the experiment which lasted four hours (I do not now speak of the seven hours experiment), and it was published in 1844. At the end of the four hours the animal kicked and I immediately killed it; as soon as it gave a kick I pithed it.

4763. Somewhere about the time I am speaking of Kölliker published his paper on the subject?—Yes, a great deal later than the date I have given; my observations were made long before that.

4764. The time is fixed in my mind because Kölliker's paper attracted my own attention a great deal, and I made a number of experiments on the subject myself. Now, the facts made out at that time were, first, that the administration of wurari prevented the animal from exhibiting any of the ordinary effects of muscular contraction?—Yes.

4765. That is to say that the animal was paralysed?—Yes.

4766. The question then arose to determine what that paralysis meant and how it was brought about, and Kölliker's experiments tended to show that what happened in the case of the administration of wurari was a paralysis of the terminations of the motor nerves?—Yes.

4767. And by certain very ingenious experiments he showed that if you kept the wurari away from one limb of an animal while the rest of the body was exposed to it, the sensory nerves of that side which was poisoned produced an effect upon the unpoisoned limb when they were irritated?—Yes.

4768. So that he drew the conclusion that while wurari affected the terminations of the motor nerves, it did not affect the sensory nerves, and did not affect the reflex centres?—Yes.

4769. But while drawing that conclusion, it is perfectly obvious that the facts which he brought forward could only go so far as to show that there was no serious impediment to the production of reflex action in the spinal cord itself?—Yes.

4770. And those experiments could not show, and were not intended to show whether wurari affected or did not affect the cerebrum, and whether it had any degree of sensation at all?—Yes.

4771. That is to say, it is perfectly conceivable (in fact that there are plenty of cases parallel) that a poison shall not affect the spinal cord sufficiently to prevent reflex action, but that it shall affect the cerebral hemispheres sufficiently to affect conciousness?—Yes.

4772. The common example is alcohol; it is possible for a man to take enough alcohol to make him entirely unconscious, and yet to leave all his reflex action unaffected?—Yes.

4773. Now Claude Bernard published a paper on the subject, and he, if I may venture to say so, jumped to the conclusion that wurari did not affect the cerebral hemispheres?—Yes.

4774. And that is a conclusion for which there is no justification?—Yes.

4775. And Bernard published in the "*Revue de Deux Mondes*" a very striking article in which he went upon this assumption of his, and drew a lively picture, perhaps more worthy of the pen of Victor Hugo than of that of a staid physiologist, of the supposed tortures which an animal under wurari undergoes. You may have seen that article?—I know of it, but I have not myself seen it.

4776. But in point of fact neither Claude Bernard nor anybody else has up to this time had any scientific justification for the conclusion?—That is so.

4777. The matter has therefore stood upon that footing; and if anybody 20 years ago arrived at the conclusion that wurari did not in any way affect sensation, he simply arrived at a conclusion for which he had no basis?—Yes.

4778. And with regard to its being the wish of physiologists to come to the conclusion that it was an anæsthetic, in point of fact there was simply no ground either for the one conclusion or for the opposite conclusion?—Yes.

4779. But it is a matter of fact that many physiologists, as you say, wishing to be on the safe side, have administered opiates while administering curari?—Yes.

4780. Then recently there have been some very ingenious experiments, published by Mr. Yale and others, which rather tend to show on the whole that wurari deadens the spinal cord as a reflex centre?—Yes.

4781. That is all they absolutely prove, and they leave the question of consciousness and unconsciousness untouched?—Yes.

4782. That is very nearly the state of the case now, is it not?—Yes, I should say so.

4783. (*Chairman.*) Do you wish to add anything to your evidence?—Yes; I should like to say that one of the great experimenters, Claude Bernard, having mastered the whole question experimentally of the nervous system, took opium in hand, and its six compounds, morphine, narceine, codeine, narcotine, papaverine, and thebaine; and he showed the exact property of each of these, in what respect each of them was more fatal as a soporific, and more fatal as a convulsant, that is to say, a giver of convulsions, than the others. And since then two physiologists, Dr. Crum-Brown and Dr. Fraser, have given, along with strychnia, the iodide of methyl, with the effect of diluting it 210 times, that is to say, 210 times the dose was required to produce death with the iodide of methyl. They have given brucia in increased doses with iodide of methyl at the rate of sixty fold. The whole of the rest were influenced more or less by it. There is another series of experiments that I think of great importance which have been made by Dr. Rutherford and Dr. M'Kendrick, in which they tried the antagonism between certain poisons with the effect of making out (I shall not go into the whole of the various agents examined) that chloral hydrate was an antagonist to strychnia and certain other substances.

4784. (*Mr. Huxley.*) With regard to the exposure of animals to a gradually increased or gradually diminished temperature, of course when an ordinary person hears that an animal has been exposed to a temperature of 140°, he pictures to himself the sort of pain it would give to his hand to come in contact with a body of a temperature of 140°, and thinks how horribly cruel that is; or on the other hand, when he hears of an animal being frozen to death, he pictures what would happen to himself in coming suddenly in contact with a very cold body. But I understand your contention to be, that if the lowering or raising of the temperature takes place gradually the animal in point of fact never does feel these temperatures; that is to say, it practically dies by the time the temperature comes to 112° or 113°?—Yes.

4785. (*Sir John Karslake.*) In your opinion, in the later stages of starvation, either in the animal or in the human being, is there considerable pain?—I would say that there is then no pain whatever. There can be no pain because the animal is at a temperature at or below 70°, sometimes as low as 65°, or perhaps a little above 70°, when the animal is in the anæsthetic stage; the nerves then have lost consciousness and sensibility.

4786. (*Mr. Hutton.*) That applies to all animals, does it?—Yes.

4787. But as to the earlier stages of starvation, what would you say?—I am not prepared to speak to that; I do not know. There must be some discomfort undoubtedly; at least we feel discomfort; but it is a very slight discomfort, I imagine.

4788. Human beings feel something more than discomfort, do they not, under those circumstances?—I am not aware that they do.

4789. (*Chairman.*) Then do you mean that when sailors are exposed to the process of starvation, from the loss of their vessel, what they go through can only be described as discomfort or inconvenience, and does not merit the name of extreme pain?—I should say so, certainly. In the first pangs of cold, if they get out of warmth into extreme cold, there is pain; but when cold has lasted long enough to act upon and deaden the nerves, then pain ceases.

4790. (*Mr. Huxley.*) You are excluding from your answer the consideration of sufferings from thirst?—Yes.

The witness withdrew.

Mr. JOHN MALLET PURSER, M.D., called in and examined.

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4791. (*Chairman.*) Are you Professor of Institutes of Medicine in Trinity College, Dublin?—Yes.

4792. We have been told that in giving your lectures you do not find yourself at all impeded in giving the instruction that you wish to give by the fact that the regulations of the University of Dublin do not allow you to exhibit experiments upon living animals, is that so?—It was Professor Haughton who gave that evidence, and that is a mistake. I must explain that I give two courses of instruction in the University of Dublin. I give a course of instruction in practical histology; and in that course I do not

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find myself impeded by the regulation in question. I give also a course of physiological lectures, and in that I do find myself considerably impeded by the regulation.

4793. You wish to perform certain experiments in order to show to your pupils certain processes which have been established by former experiments?—Yes, I do.

4794. Are all those capable of being performed under anæsthetics?—By far the greater number of them are.

4795. If you were permitted by the University to make the exhibition of which we have been speaking, provided that you always used anæsthetics, a great part of the difficulty of not being able to convey all the information that you wish to convey would be removed, would it not?—It would.

4796. And you would be satisfied perhaps with that power if it were given to you?—Well, I should be better pleased with it than I am with the condition which prevails at present.

4797. Would what would then remain over which you would still not have had conceded to you be the experiments upon the higher animals?—Partly on the higher animals, and partly on the lower. It is rather hard to say what are "higher animals." You mean, I presume, warm-blooded animals. Some experiments would remain that would probably have to be performed on warm-blooded animals.

4798. Without anæsthetics?—Without anæsthetics.

4799. Would they be experiments involving considerable suffering?—Yes, some of them undoubtedly would.

4799*a*. Then do you contend for the powers of lecturers generally to perform painful operations for demonstrations to students?—Yes, I do.

4800. Now, if you were told that in institutions like the University College, London, and like Guy's Hospital, there was no such want felt, would you think that there was a want which ought to be satisfied in Dublin, when no such want was felt at all in London?—My view is that the matter should be left to the judgment of the professor.

4801. Entirely?—Entirely.

4002. And that if the sentiment of humanity on the part of the public interfered, it would be an undue interference?—I think it would be an undue interference. I do not consider the public a good judge of such matters.

4003. Do you think that the teachers at Guy's Hospital and the teachers at the London University are good judges?—I think they are good judges.

4804. We have been often told that the students would be very much offended if experiments were displayed before them which did inflict considerable pain upon living animals, is that your belief with regard to your students?—I do not believe that if the experiments were performed as they should be, seriously and with a view to the instruction of the students, any right-minded student would be offended.

4805. In short, you give it as your opinion that your own discretion should be the only rule, and that any general sentiment of humanity in the minds of the public at large should be disregarded?—As I said before, I do not consider that the public is a fit judge of these things. The public, in the first place, do not know the way in which these experiments are performed, and in the next place, they do not know the object for which they are performed, and the value of the instruction to be gained from them.

4806. What I want to know is what your contention really is. I want to know whether you contend that in this matter, your own judgment and that alone should be the guide, and that any interference from any sentiment of humanity on the part of the public should be excluded?—I can conceive a case in which a professor would be extremely ill-judged, and perform experiments which were not necessary, and perform them in a way to inflict more suffering than necessary—under those circumstances I think the professor should be cautioned or prevented from so experimenting, but I do not think it is the public who should prevent him.

4807. But if there were some interference by a competent and high authority, would that be disagreeable to you?—It would be very disagreeable to me to think that I required any such interference.

4808. But we must not suppose that you do. I am now putting it in this way: if there were any teaching going on in this country of the character that has now been described, and if it were proposed that some such high authority, as was suggested in the Bill introduced last year in the House of Commons, should be called in to restrain that, would that appear to you to be a hardship?—I consider that it would be a very unsuitable subject for legislation.

4809. Altogether?—Altogether.

4810. In short you are so convinced of the good judgment of the teacher that you think it should be superior to every other consideration?—I think the teachers of physiology are the most competent judges.

4811. (*Lord Winmarleigh.*) How long have you held your present office?—I have been professor in the School of Physic in Ireland for nearly two years; but I was teaching physiology before that in a private school for nearly seven years.

4812. With regard to those to whom you lecture, the students who are before you, have you any reason to believe that while under your instruction they carry out any experiments upon living animals?—No, I am sure they never do.

4813. Supposing any one were to be known to do it, do you think there would be any strong feeling in Dublin against it?—There is a section of the people in Dublin who no doubt would feel very strongly against it.

4814. Supposing you were to ascertain that one of your own students was making an experiment himself, not under your direction, should you take any steps with regard to him?—That is a very hard question to answer in general terms. I would try to find out what the experiment was, and how he was going about it, and I would volunteer my advice and assistance.

4815. But do you think that if even as you suggest the professor should be left entirely to his own judgment as to the necessity of experiments, that professor should have any control given him by legislation or otherwise over the students till they had passed certain examinations?—I would certainly allow no student to perform an experiment in the school, except under the direction of the professor.

4816. You would see no objection to some restrictions being placed upon the students for that object?—I would make it the rule of every school that no student should perform an experiment in the school, except under the direction of the professor, or of some competent person.

4817. That would not include preventing their having the power of making the experiments elsewhere?—I do not think that the school could control that.

4818. You do not see any mode of giving the professor the power of having some control over the students in that respect?—That is a purely imaginary case I think; because I do not think that students are very likely to do it. Students as a rule are not fond of performing experiments.

4819. I am suggesting the propriety of some power being given to prevent these imaginary cases, if they exist?—I do not think you could control students when once they leave the school.

4820. You have no fear whatever that they would need such control?—I have not the very slightest fear; I do not think the case would ever occur, from my knowledge of students, of Irish students at least.

4821. On an average what is the number of students that you are in the habit of giving instruction to?—The students who enter for my lectures vary from about 50 to 60 each year, and I work the practical histological classes in batches of 20 men,

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We have two or three such courses in the year, according to the number of men who enter.

4822. That is at the University of Dublin ?—Yes.

4823. Do you know of any other schools in Ireland ?—Yes, I know all the schools in Dublin.

4824. But all in Ireland ?—There are schools in Belfast, and Cork, and Galway, but I know those schools merely by reputation.

4825. (*Chairman.*) Your objection to legislation upon the subject is founded upon the impression that all professors will be humane, and all students will abstain from experimenting ?—That is my impression, and my impression on that point is so strong that I think that legislation would be quite unnecessary and would do more harm than good.

4826. (*Sir John Karslake.*) Did I rightly understand you to say that you have given private lectures on physiology for many years past ?—I gave lectures in a private school.

4827. In giving lectures in a private school did you use demonstrations upon living animals ?—I did occasionally.

4828. Did you find it essential according to your view that you should demonstrate in that way for the purpose of fully informing those who attended your classes as to that which you wished to illustrate ?—Yes, I think it quite essential to good physiological instruction.

4829. May I ask where you received your education ?—I received my education partly in Dublin and partly in London.

4830. Had you the advantage, if it be an advantage, of seeing any experiments in Dublin when you were *in statu pupillari* ?—I had, but very few.

4831. Was that in a private class or in a public class ?—Not in a private class, but in a private school; in the same school that I lectured in afterwards.

4832. Did you also see experiments in London upon living animals ?—No, I never saw experiments in London; I never studied physiology in London.

4833. Then the only instance in which you have seen experiments upon living animals was during your study in Dublin ?—Yes.

4834. Do the governing body of the university interfere in any active way with the professors to ascertain the character of their lectures and so forth ?—No, there is no spy put upon us; none of the authorities of the university visit our lectures, but of cuorse any of them might come in at any time and, I was told when I joined the school that no vivisections were allowed, and it is put in the calendar as one of the regulations that vivisections are absolutely prohibited, and of course I promised to obey, and I have obeyed.

4835. Do you think, if vivisections were allowed, the supervision of the governing body would have the effect of preventing anything an improper use of them in the school in case the professors were inclined to use improper demonstrations ?—I quite think so.

4836. I understand you, that as far as you know, not only do the pupils in the school not practice these vivisections in their own rooms in private, but that the students in your class do not practise them ?—They do not.

4837. And never have done so ?—And never have done so; I am certain I should have heard of it if they had.

4838. At the time you yourself were a pupil in Dublin, when you say you did see certain demonstrations on living animals, was there any practice existing among the students who were your contemporaries of making experiments in their private rooms ?—No; I never heard of it, and I never performed experiments myself while I was a student.

4839. (*Mr. Huxley.*) I understand that you feel restricted by the regulations of the university in which you occupy a professional chair from performing any vivisection. Will you tell us how you construe that word "vivisection" ?—Vivisection means an experiment on a living animal; of course it means cutting an animal.

4840. But what I want to know rather is the meaning which you attach to the word "living." Is it the meaning of the authorities of the school in which you are professor that you should absolutely cut nothing which has any life in it; or do they mean that you shall not cut anything which has any feeling in it ?—They mean that I shall not cut anything which has any life in it. As I understand it I am not allowed to perform experiments on animals, no matter how insensible they may be rendered to pain by the use of anæsthetics.

4841. That is to say you may not show your class the circulation in the web of a frog's foot, because pinning its toes out would give a little discomfort ?—They would allow me to do that because I asked that particular question; and I do not pin the toes out but tie them; I put a thread round them.

4842. You may inconvenience the frog a little by tying its toes out with the permission of the authorities; but you must not cut the frog's head off and prevent its suffering any pain at all in order to do something else ?—Once the whole animal is killed they allow me to operate upon living tissues. I think they would allow me to cut off the head, and then pinch the toes to show reflex action.

4843. Will they allow you to show Weber's experiment of the influence of the pneumo-gastric upon the heart. That can be done upon a frog which is deprived of its brain, for example ?—I think they would not like that. But I must say that I have not asked all these questions; because I got a peremptory order not to perform any experiments on animals when I came into office; and then when the histological classes were started, and I began to work these subjects, I asked Professor Haughton if I might show the circulation in a frog's foot, and he said certainly, because that did not hurt the frog.

4844. If you can make it plain to the authorities that a frog without its brain, or with its head cut off, does not feel, you might do it if you like ?—I think if I could make that plain to the board they might allow me to do it.

4845. You do not seem to feel quite sure that you could not make it plain to them ?—I think it would be difficult to explain such matters to gentlemen who are uninstructed as to the requirements of physiological teaching.

4846. So that here we have an example of what inspection comes to, even when carried out by a highly educated body of men who do not happen to be physiologists ?—Yes.

4847. But in fact I may take it as your opinion, I suppose, that a frog under the influence of chloroform, or with its brain extirpated, has no more feeling in it than a board ?—I believe it has none.

4848. And in fact a frog under those circumstances, whatever you do to it, feels less than a frog whose toes you tie, because that frog does not feel at all ?—Yes.

4849. So that while you are prohibited from doing that which does not cause inconvenience to the animal, you are allowed to that which does cause a little inconvenience ?—That is my impression.

4850. And this practical experience of what non-scientific inspection comes to rather leads you to entertain a horror of further interference in the same direction, I suppose ?—That is very much my feeling.

4851. (*Chairman.*) With regard to this experiment on the pneumo-gastric, can that be performed on a frog that has been pithed or had its brain extirpated ?—It can.

4852. Is that a useful experiment ?—It is a useful experiment.

4852. It would tend to enlighten and instruct your pupils if it were performed, you think ?—It would.

4854. You have suffered very much in that respect from the prohibition to exhibit experiments you say ?—I have.

4855. And you have never thought it necessary to ask the board whether they would permit you to exhibit this experiment ?—I did not think it neces-

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sary. I thought I understood the feelings of the board so well by their first directions that I did not consider it necessary to ask the question.

4856. But you have told us just now that it could be performed without giving the frog any pain?—Except the pain caused by extirpating its brain.

4857. And your immediate superior, Dr. Haughton, told you, as I understand you, that if you did not give the frog any pain in your experiments, you might perform them?—That I might perform that one experiment which I spoke of.

4858. Had you ever asked him if he would allow you to perform this other one?—I have not.

4859. He is a very scientific person, is he not?—He is undoubtedly.

4860. And there is every probability that if you had thought it worth while to ask the question he would have given you an affirmative answer?—Possibly.

4861. But the inconvenience to which you have been subjected has been so small that you did not think it worth while to ask that question?—The inconvenience is not an inconvenience to me, but an inconvenience to the class.

4862. But you so little regarded the inconvenience to the class that you did not think it worth while to ask Professor Haughton that question?—I did not think it necessary to ask leave to perform every individual experiment?—I expressed my opinion at the time that these experiments were necessary, and that their performance should be left to my judgment, but they said that would not do at all, that they were to be the judges.

4863. (*Mr. Huxley.*) In fact I suppose you considered the intimation you received as an expression of the pleasure of the governing body that you should not do that sort of thing?—Yes.

4864. And you further felt that if you persisted in making applications of the kind you would probably put yourself in a very disagreeable position with regard to them?—I do not think that they would have made my position at all disagreeable. They have always acted towards me with the greatest kindness and courtesy.

4865. I say you would have put yourself in a disagreeable position with regard to them?—I think I should, because I considered that that prohibition at first was the final expression of their wish. And I understood it to mean that I was not to perform any experiments on animals before the class.

4866. (*Chairman.*) I understood you to say that provided the frog was insensible they permitted you to experiment on it; that is to say, that Professor Haughton told you that, provided the frog was insensible, they would allow you to perform the experiments?—No, they do not allow me to perform experiments under the influence of anæsthetics.

4867. But I understood you to say that in a case where one experiment was to be performed in a way that did not hurt the frog, you were told that you might perform it?—That was not a vivisection at all; it is not an experiment in which the animal is hurt in the slightest degree; it is not an experiment in which the animal is cut in the slightest degree, but it is a vivisection to cut off the head of a living animal, if I understand it rightly.

4868. Do you understand that the prohibition under which you labour is a prohibition which prevents your displaying experiments upon animals that have recently been killed?—No, I think they would allow me to perform experiments on animals recently killed.

4869. They would allow you to cut the head off a frog, and then perform experiments on it?—I think they would: but I should consider myself bound to ask permission before performing such experiments.

4870. Then why did you not cut off the head of the frog and then perform this pneumo-gastric experiments on it?—One of the reasons was that when I was lecturing on that subject, it was almost immediately after my appointment, and things were not sufficiently in working order; I had begun to lecture on a subject not much lectured on before. That is the chief reason perhaps.

4871. (*Mr. Huxley.*) I think there has been possible a little misunderstanding in respect of your relations to Dr. Haughton. I understand that he is dean of the school?—He is called registrar of the school.

4872. As such, he is in no sense your official superior, is he?—Well, I should find it very hard to define exactly what his position is with regard to me. He is the person who gives us the orders from the board; he is the spokesman of the board to us.

4873. Your orders and instructions however do not come direct from the board to you, but through Professor Haughton?—Through Professor Haughton. He sends us a printed copy of the resolutions signed by him.

4874. Then if you get a regulation from the board telling you that you are not to experiment on living animals under any circumstances whatever, you look upon that prohibition as absolute?—Certainly.

4875. But you may go to Professor Haughton, and say to him, "Now do you think I am to understand "this in such a way?" And he may reply to you, "Well, I think in this particular case you may;" but that by no means weakens the force of the general prohibition?—That would be the case. I should look to him as the interpreter of the resolutions of the Board.

4876. That is to say, what would be meant by his saying that would be that in this particular case you need not fear any interference, although you had actually committed a breach of the letter of the law?—Yes, that would be it, I think.

4877. Then it is possible that another point of view may have struck you, that a vast deal of pain is inflicted upon animals for various purposes; as, for example, for the castration of lambs, the spaying of sows and so forth; and it may have struck you that legislation which does not in any way interfere with those operations, which cannot be said to be for the good of the animal itself, might in fairness leave the comparatively small inflictions of pain, attributable to physiologists, alone?—That consideration has struck me, and must have struck others very much. Still I could not say that that was an argument against vivisection; two blacks do not make one white.

4878. Certainly not. The case which I put to you was, that if legislation was to take place you would think it fair that such legislation should have a general bearing?—Certainly.

4879. And that if it could be shown that pain was inflicted upon animals for no good purpose, you would allow, I presume, with every right-thinking man, that such a thing ought to be stopped; that if any person inflicted pain upon animals without some good reason, there would be a fair case for interference?—Yes.

4880. But that you would like to be under the same laws as everybody else in that matter?—Yes.

4881. (*Mr. Hutton.*) You have stated that you thought there were some experiments which are very painful on warm-blooded animals, which it would be desirable to use for the purposes of demonstration. Did you ever use any such in the private school in which you were a teacher at one time?—No, I did not.

4882. Could you give us at all any idea to what kind of experiment you referred?—Experiments on the sensitive nerves.

4883. Do you think that experiments showing the functions of the anterior and posterior roots of the nerves would be desirable for demonstration in a class?—I think certain experiments on the sensitive nerves which would involve pain to the animal would be highly advantageous for the class to see; but I think that there would be very few such experiments.

4884. (*Chairman.*) Is there anything further that you wish to say?—The only thing that I have to say is, I think there is an impression abroad that medical

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students are very fond of performing experiments on animals, and that performing experiments on living animals is a very amusing occupation, and a thing which not only all students but all doctors also are very anxious to do. Now my experience is quite the reverse of that; my experience is that as a rule students have no taste whatsoever for such pursuits. The operations are extremely disagreeable to perform, extremely difficult, and extremely expensive; and I think that all those reasons would make students not take to them. Besides that, I think students would not care to perform operations which they did not think themselves competent to perform, and which they did not think that some good would come from; and I am sure that any student in our schools thinking of performing experiments, would come to the professor and take his advice and get his assistance, and, if the professor thought it necessary, do it under his supervision.

4885. (*Mr. Huxley.*) The better instructed students are in the mode of experimentation the less likely they are I suppose to attempt to perform them without due care?—Yes.

4886. (*Mr. Hutton.*) I understand that when you come to the subject of the pneumo-gastric nerve again you think it very probable that you might obtain permission from the authorities to perform that experiment on the decapitated frog, without really infringing the rule of the Board?—I consider the rule of the Board is such that I could not do it without asking their leave. And experiments of the sort on a frog not decapitated but merely anæsthetised I consider the Board would not at all allow me to do.

The witness withdrew.

Mr. Arthur de Noé Walker, M.D., recalled and further examined.

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4887. (*Chairman.*) You were so good as to say that you would prepare a statement derived from published medical journals of foreign countries of examples of experiments, which you wish to bring under the consideration of the Commission. Have you those extracts with you?—Yes, but they are not exclusively from my experience of foreign laboratories. There are one or two done in this country which I shall lay before the Commission.

4888. Will you be so good as to do so?—I will do so at once in the hope of convincing you how urgently legal interference is called for in order to check and control the practice of performing experiments on living animals. The first instance is one I have found in a volume of the West Riding Lunatic Asylum Medical Reports, 1873, wherein the experimenter on the first page thanks the physician of the ayslum for the "liberal supply of pigeons, fowls, "guinea-pigs, rabbits, cats, and dogs," placed at his disposal for experiment. One of the necessary conditions which I have already adverted to for a proper control over experimental researches on living animals is, that the number allowed to each licensed vivisector should be limited. Now although I cannot prove that the "liberal supply" of so many victims placed at the disposal of the experimenter on this occasion, was more than was absolutely necessary, I nevertheless submit that the paragraph forces a disagreeable and painful doubt on the mind. If he had said "a sufficient number," it would have been different; the word "liberal" to me is uncomfortable. In the judgment and conscience of every thoughtful man, whatever his views may be regarding the practice, vivisection entails a great moral responsibility; first, as regards the least possible amount of suffering the poor victims can be made to endure; and secondly, as I have already said, regarding the number submitted to vivisection. I will make two more remarks on these experiments and then leave them. I have already recommended that every licensed experimenter should send in two returns, filled up with certain details I have particularised, in order that the board controlling vivisection may have every means at their disposal for doing this in the most efficient manner. The remark I would myself suggest to the board, had the return of these experiments been submitted to me, would, among other things, have verged on the following particulars. The experimenter appears satisfied that certain emotional and muscular centres are symmetrically placed in the two cerebral hemispheres. (I did not know myself that was doubtful; I thought it was established, but I may be mistaken.) Now I submit that the experimenter ought to have taken the opportunity of ascertaining, without subsequently recurring to other painful experiments, whether contemporaneous "stimulation" of two symmetrical or rather corresponding centres, intensified the phenomena. He should also have endeavoured to ascertain whether one emotional or muscular centre was able to neutralise or disturb another and antagonistic one. The last observation I will make on these experiments relates to an animal which lived five days with the greater part of one of its cerebral hemispheres exposed, during which it gave rise to a fungus. This animal at the end of five days underwent another operation or experiment which caused its death. Now whether this animal ought to have been kept alive five days, and then to have undergone another experiment would be a proper subject for inquiry, if vivisection were under control. Whether the action of the electric current, poured into any portion of the cerebral masses whatever, is likely to be identical, as the experimenter seems to believe, with the emotions and instincts aroused by a natural and ever varying environment, I leave to others to judge; whether, I mean, the electric current, stimulating certain parts of the cerebral masses, is likely to suscitate in the animal the same feeling and the same instincts naturally excited when the external world is acting on it. Before I proceed to advert to other experiments and abuses, I must, as clearly as I can, set forth a general rule neglected or ignored by some experimenters. Every experimenter should bear in mind that the conditions in which an animal is experimented on, as well as the nature and process of the experiment itself, should be as much as possible according to the order of nature, that is to say, that experiments should represent, as nearly as possible, that which may naturally happen to man or beast. If this rule is set aside we shall very often get information that cannot be of much, if any, use to us, or by torturing nature extort a reply that may deceive us. Every experimenter should bear in mind, that we cannot legislate for nature. All that we have to do is to find out what nature's laws are, and not to force nature to tally with our own fancies or our own theories. Now the next experiment I wish to make a few remarks about is the following: Two or three dogs are operated upon for gastric fistula; one or two of them generally die of peretonitis consequent on the operation. Now I may remark that some laboratories very greatly favour that unfortunate result, because the animals are confined in a damp and exposed places. When one case of gastric fistula however was established, the posterior half of a living frog was inserted into the aperture leading to the stomach of the dog, while the anterior half, head and legs, protruded externally, and were fastened there until half the frog was nearly all digested away. The object was to show us this,—the action of the gastric juice on living tissue. As the gastric juice gradually ate away the skin, the nerves, and the muscles, the frog made desperate efforts to escape by moving its anterior extremities very rapidly. (*See* Claude Bernard *Physiologie*, Vol. 2., page 409, 1856.) The first observation I have to make against this and thousands, literally thousands, of experiments

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is, that it is not in sufficient harmony with the order of nature; perhaps I could say with more truth that it was against the order of nature. Dogs do not swallow live frogs, and frogs do not jump down into the stomachs of dogs. However we will let that pass; but I think, secondly, a humane experimenter would have been quite satisfied when he had ascertained that the gastric juice had actually commenced to dissolve the common integument, and then removed the frog; that would have been the most humane proceeding, and perfectly sufficient as an experiment. The third objection which I have against this and other similar experiments is this. If he had not allowed habit to harden him he ought to have divided the medula oblongata or spinal cord of the frog, and thus he would have commenced and ended the experiment with precisely the same results, without causing his helpless victim any pain. Fourthly, as he had done the experiment before, and was certain of the results obtained, the repetition of it before his class was wanton and cruel, especially as a verbal account of the whole matter would be perfectly intelligible. This is all I wish to say of this experiment, conducted, in my humble opinion, with a total disregard of the claims of nature on behalf of her creatures. Now I beg leave to say a few words on experiments made chiefly by toxicologists. Some toxicologists are extremely cruel. These experiments are still very numerous, and I think, if I may use the expression, very much overdone, and require, I believe, as much legal control as any other kind of experiments made on living animals. An animal, for example, can be made to inhale a volatile or gaseous poison, and the effects noted. A non-volatile poison may be injected under the skin, into the rectum or into the stomach. These means appear to me to be such as to amply prove the effects of every kind of noxious or innocuous substance. Not content with this, a common practice is to make a solution of the substance, and forcibly inject it down the windpipe of a dog or cat. This causes the animal intense distress for no necessary purpose; for, in the first place, the absorbing and eliminating function of the lungs is well established; secondly, a non-volatile substance cannot be inhaled; and it is cruel, therefore, to make an animal undergo a most distressingly painful experiment, which does not represent that which can occur in the ordinary course of nature, especially as the action of the same poison had already been determined in every other way. The same judgment must, I think, be passed on another somewhat similar experiment. The œsophagus, or gullet, of a dog is exposed by an operation, and a thread is passed round it. A certain substance, for example liver of sulphur (this happens to come in my mind because I saw it) is then forced into his stomach, immediately after which, to prevent the animal vomiting it, the œsophagus is closed by the thread. Anæsthetics are not, of course, used on these occasions, and the dog lives as long as nature can endure the combined effects of the operation and of the poison, coupled with want of food and drink. In this and thousands of similar experiments nature is not fairly or philosophically interrogated, but rather tortured or violated; they represent nothing that can happen in the natural order of things, but rather an artificial and forced condition, from which nothing really practical can be learned. The same may be said of a very great number of experiments consisting in making an incision in the skin, and forcing some deleterious substance into it, or of opening the abdomen, tying a portion of the small intestine in two places, opening the intermediate portion, and then injecting a noxious fluid into it. I think that in neither of these experiments are the natural contingencies of life represented, and I have in vain endeavoured to learn something from these and hundreds of similar experiments performed by toxicologists in this and other countries. (See Christison or Taylor on Poisons, or any work on toxicology.) Perhaps it has been the bluntness of my understanding, or my incapacity to give a practical bearing to what I learn. Sometimes elements of failure in individuals take that form, and it may be so in my case. But I should like the Commission duly to consider the import of this and similar facts, and pass their judgment on them. Mine is, that they betray a wanton and cruel abuse of power in the nature of the experiment, and in the number of the victims experimented on. I am here reminded of another experiment, known to me. A number of rabbits were deprived of food until they ate dead frogs thrown into their hutch. The object of this experiment was to show that herbivorous animals could live on animal food. What use this information may be put to I leave to the judgment of unbiassed minds to determine. (See Schiff, *Physiologie de la Digestive*, vol. 1, page 67.) I know that the rabbits suffered very much before they were induced to eat the dead frogs. An experimenter (see *Medical Times and Gazette*, 3rd August 1861, page 104,) was once lecturing on the effects of poisons in the animal organism. In order to show us, however, that innocuous matter has the power of "disorganizing" animal tissue, he forced half a pint of boiling water into the stomach of a dog. The animal gave evidence immediately, of suffering great pain, vomited blood, and soon died. Another experimenter (see Handbook for the Physiological Laboratory, page 409,) divides the medulla oblongata of a frog and performs the usual series of experiments on the mutilated animal. Finally, in order to prove that a frog with the medulla oblongata divided does not always respond to stimuli, he places it in a basin or trough of water, gradually raises the temperature till the water boils, and the animal of course gradually stiffens and dies, without having made any efforts to escape. Now, to anyone who had not allowed his heart and intellect to get gradually enslaved and carried away by inordinate zeal and culpable indifference to pain, this experiment would be sufficiently conclusive. I do not think it was necessary to institute a comparative one, because the action of boiling water and of heat generally, is practically known to all the world, and boiling water has always but one effect on animals, whether dead or alive. The experimenter, however, thinks all this must be ignored. He therefore takes a sound frog and places it in the basin, with just enough water to cover all but its head. The temperature is then gradually raised up to 20 or 30 centigrade, and of course the animal soon makes desperate efforts to escape from the painful effects of the hot water. Now, I regard every experiment made on living animals, whereof the result is well known and established, as a criminally cruel proceeding. But when horrible pain is inflicted on a vertebrated animal in order not to prove, but simply to bring about, that which is known to all the world, I characterise such proceedings as aimless and useless, and as a wanton and cruel abuse of power. Had opportunities and other circumstances permitted me I would have summoned both these experimenters with hot and boiling water into the nearest police court, where, if I found that there was a law that regarded only cruel but oftimes hard-worked and provoked drovers, I should at least have succeeded in branding the Englishman that scorched the frog and the Frenchman that killed his dog with boiling water, as men who had permitted habit and lack of reflection to deprive them of that which most men hold in some estimation, I mean the feelings of pity and the sense of shame. I have seen much cruelty inflicted on animals from this unreasonable and unreasoning propensity to recur to a comparative experiment when, as in the case I have just adverted to, about the frog, no comparison could on any pretence whatever, be justifiable. A cat from which the glosso-pharyngeal nerves had been excised was once brought before us. Meat saturated with a decoction of colocynth was then placed before it, and after considerable difficulty about it, swallowed some of it. A sound cat was then brought and no coaxing would of course induce it to partake of the food so prepared. All the world would have anticipated that.

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The animal was, however, seized by the experimenter and his assistant, and then one of the frightful struggles so common in physiological laboratories commenced. The nauseous and bitter food was forced deep back into its mouth just to show how the poor animal would shake its head and reject the food with a profuse flow of saliva. This animal was cruelly treated and made wretched for nothing. I have taken, I may say, in perfect honesty, no particular pains in the selection of these experiments for the consideration of the Commission, but have selected them chiefly as typical cases of the various ways in which I believe abuses are perpetrated. I must now recur again to the useless and cruel experiment which I adverted to on a previous occasion, as I hope that the Commission will embody it in their proceedings, and bestow their attention to it. Doves or pigeons were totally deprived of food until they dropped from their perch on to the floor. As soon as it was evident that they could neither stand on their feet nor fly, their anterior or posterior extremities were snipped off with a pair of forceps, after which death immediately ensued. The object of this experiment was to show the effects of slight traumatic lesions on an organism attenuated and exhausted from want of food. (See "*Medical Times and Gazette*," August 18th, 1860, page 151.) Here again I must remark, that in my judgment, this experiment performed on a number of pigeons, was aimless, because I believe that not physicians and surgeons only, but every old dame in Europe knows about the combined effects of suffering and starvation, wounds and want, and of hardships and hunger. If this experiment was unnecessary, it was in my opinion aimless, and, if aimless, a cruel abuse of power over nature's creatures who have their particular rights and claims as much as mankind have theirs, even when one powerful race conquers and enslaves another; even slaves have a certain amount of rights, not legal but natural. Another experiment that has lately been made by a German strikes me as against the order of nature, and therefore, as far as my intelligence about physiological experiments goes, is quite aimless and cruel. He forced into the stomach of a certain number of rabbits at various periods, a quantity of Liebig's extract of meat, equivalent to about 700 to 1,200 grammes of fresh meat. This was done to test the nutrient qualities of the extract. They all died and proved nothing. (*See* scientific section of the "*Gazetta d'Italia*," 23rd August 1875). I have said that a proper supervision over animals, either destined for experiment, or of such as had been experimented on, was absolutely necessary, and that a half yearly return should afford ample information on this head. I shall never forget the first time my attention was drawn to the necessity of this. "This rabbit," said the lecturer, "has had the fifth "pair of cranial nerves divided. It will die in four "or five days, but it will not die from the operation, "severe as it is; it will die of hunger," and of hunger it did die. (*See* Claude Bernard, *Système Nerveux*, Vol. 2, 1858, page 101.) But this was not the worst of it in my estimation. Dogs and rabbits were every "season" allowed to die of hunger instead of being killed. Of course everybody can see that the animal could not swallow its food. I have watched them myself masticating food, and to a certain extent able to put it in the back part of the mouth, but it could not be swallowed, and the consequence is that they die of hunger. I did not see the use of that, and I protested at the time against it. I may here say that although after I was fully qualified and had been in practice some years, I went through these things, I had not the least intention of seeing or hearing them to detect abuses or malpractice. I have, since I last came before the Commission, come across a few particulars regarding the experiment made on a frog, and represented in this sketch (*handing it in.*) (*See* Claude Bernard, *Système Nerveux*, October 1st, 1858, page 188.) It appears that the object of treating the animal in this way was to prove the effects of exhaustion on the nervous system. The exhaustion was, as you see, brought about by exposing the two largest nerves, nailing the feet down to a board, and depriving it of food. The various increasing stages of exhaustion were tested by an occasional discharge of the electric current on one of the exposed nerves. I decline myself to criticise this horrible experiment. I feel too much contempt for the experimenter and disgust for experiment. I will, however, observe that if the experimenter had kept the animal in that condition ten minutes, I say ten minutes only longer than any competent judge deemed to be necessary, I would have deprived that man of his position as a lecturer and a teacher of physiology, as one who had forfeited every pretension to assume to himself, I had almost said, nay, I will say it, the sacred office of extorting truth out of the direful agonies of an animal, and as one, moreover, who had trifled with the moral training of the pupils committed to his charge. I do not know whether I need go on, I have a great number more to read, but the Commission must be very tired, and I think I have read enough to prove that abuses do exist which require your serious consideration.

4889. (*Lord Winmarleigh.*) In those remarks that you have made, I did not hear whether you stated whether any, or, if any, how many of those experiments were done under anæsthetics, and how many were without ?—The question of anæsthetics could not be met in that way. For instance, you might cut the throat of a dog and lay the gullet bare under chloroform, and push the substance you wish to test into the stomach, after which, he would live, according to the nature of the operation, a week, or two, three, four, five, or six days in great suffering. Anæsthetics may be used for the operative part, the part that is done with the knife; but the chief amount of suffering comes afterwards. In the experiment of boiling water forced into the stomach, there was no anæsthetic used. Then in the case of the cat, in whose mouth the meat saturated with a decoction of colocynth was forced, there was no chloroform used.

4890. What I want to ascertain is this, that those cases have been brought forward by you without any note attached to each of them as to the use of anæsthetics ?—No reference whatever is made to the use or not of chloroform. If you starve rabbits until they eat meat, you cannot do it with chloroform.

4891. (*Mr. Huxley.*) I should like to ask you two or three questions, really not with a desire of carping at your views, because there is a great deal of what you said with which I should entirely sympathise; but there are one or two questions which I should like to put to you now about that colocynth experiment. Although it is not a nice thing to force colocynth into a cat's throat, yet I suppose we have all had occasion to taste colocynth at some time or other, and you would hardly call that cruel, would you ?—I would characterise it as cruel for this reason. Two cats were brought into the laboratory, one of which had the nerves of taste divided, and unable to taste. As the taste of colocynth, as you very properly remark, is well known to most of us, I assumed that a cat's dislike to it might be equal to that in the human being. To take a cat therefore and forcibly put colocynth into the back part of the throat, to prove that which all the world knows, I call cruel.

4892. Was that really the purpose of the experiment ? The purpose of the experiment was to show the function of the glosso-pharingeal nerve. The experimenter says, "I have two cats; in one of "them the glosso-pharingeal nerves are divided, and "in the other they are not divided; you will observe "that when I give the one colocynth it does not take "it, when I give the other it does;" so that I take it the object of the experiment was not to show that a cat does not like colocynth, which all the world knows, but to show that a cat with the glosso-pharingeal nerves divided had lost, or nearly lost, the sense of taste ?—The impression made upon my mind at the time was that there was no necessity to force colocynth

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into the mouth, because it rejected it the moment it smelled it. The cat had been starved for some days before, and yet rejected it instantly when its olfactory nerves were affected by it. I thought that quite sufficient. In the next place, I think we can assume that the functions of the glosso-pharingeal nerves of a sound cat would have been instantly stimulated by a decoction of any disagreeable substance like colocynth.

4893. In fact you do not think it was worth while to show what would happen to a sound cat at all?—No.

4894. But granting for a moment that it was worth while to show how the glosso-pharingeal nerves of a sound cat are affected, you could hardly call that a very cruel way of doing it, could you?—We must remember that a cat is a very irritable animal, and to hold a cat down by force, open its jaws by force, and finally force meat saturated with a decoction of colocynth into its mouth, is a cruel thing to do, if not necessary. The cat was made wretched for days after. The impression on my mind was that the comparative effect was not a thing necessary to show.

4895. In fact you would assume that the cat would dislike colocynth?—Yes.

4896. But there might have been a little danger in that assumption as we know cats are very differently affected by certain substances from ourselves. You will doubtless be quite familiar with the very extraordinary effect of valerian on cats?—Yes.

4897. That certainly has not the same effect on the human being as on cats; and it is just possible that the colocynth would have a different effect on cats?—Yes, it is possible, at the same time it is an animal far simpler in its diet than we are; and any nauseous stuff of that sort would much more easily affect it than it would a human being. I would say that the case was possible, not probable; moreover a cat's dislike for colocynth is as natural and as easily perceived as its liking for valerian is natural, and easily perceived, without forcing it into its mouth.

4898. I understood you to speak of that experiment of feeding rabbits upon frogs. Did I understand you to say that that was in your view a cruel experiment?—Yes; I think it was an unnecessary experiment, and therefore I think cruel.

4899. But the two things surely are separate; an experiment may be superfluous, and yet not cruel?—I quite agree with you there, it may be superfluous, and not involve cruelty. So far it was cruel that the animals were obliged to be starved pretty hard before they were induced to eat dead frogs. The cruelty consisted in the starvation.

4900. I remember hearing a number of years ago (I do not know whether the fact is within your knowledge also) and it surprised me very much when I did hear it, that in Norway, where there is a very long winter, and where there are no very large root crops, the people are in the habit of feeding their cows during the winter with a mixture of stock fish and cow dung chopped up together?—Yes, I am aware of that, and have seen cows fed for months on lemons.

4901. By which diet the cows are maintained through the whole winter?—Yes.

4902. Then there was one other point with regard to the experiment which you have described of putting a live frog partly into the stomach of a higher animal and watching it being digested. Now, perhaps I may remark that I entirely agree with you that it would have been very much better if that frog had been previously pithed; but when you speak of that as being contrary to the order of nature, surely I may remind you that the order of nature, so far as frogs are concerned, is that they should be swallowed by birds alive?—Yes.

4903. And therefore they undergo that exact experiment every time they are so. You would not dissent to that, would you?—I would not dissent to that; only the experiment was made with a dog, and I thought it was against the order of nature, because dogs do not naturally eat live animals. Your remark, however, about some birds that eat living frogs, makes me think the experiment was really not necessary at all.

4904. (*Mr. Hutton.*) Your references have been chiefly to Foreign laboratories. Now, have you any reason for thinking that anything like the same extent of painful experimentation goes on in English laboratories?—I may explain the difficulty that I have had in making up my notes. I have made one or two attempts to get into physiological laboratories in this country with the wish of seeing what was going on there; but finding that I could not do this except under false pretences I gave it up; and the more so, as a gentleman of my acquaintance made a personal application to be admitted with the avowed object of seeing what was going on, and he was refused.

4905. Can you tell us to what laboratories that referred?—I should not like to say.

4905*a*. You would rather not?—I would rather not.

4906. But you were known as a physiologist, I suppose?—No; I was known as a physician in practice, and as one who had said and written a good deal against what I deemed to be abuses perpetrated in physiological laboratories.

4907. You have had some experience yourself as a vivisector?—Yes.

4908. Now I am sure you will not object candidly to state to the Commission whether you yourself have experienced any of the hardening effect which vivisection is said to have upon practical physiologists?—I can only repeat, what I have said just now, that nature will not suspend her laws to please me or any particular individual, or any particular theory, and that vivisectors are not exempt from the law of habit. I can bring you two or three cases to prove the hardening effect of habit. An observation, which first brought to my notice the hardening effect of habit, both moral and physical, is this. I used to dine very often with a lecturer on physiology, and one night I found that I could not enjoy either my cigar or my dinner, because the day before we had gone through the laboratory, and I could not get rid of the imploring look of the dogs which hoped for some food every time that they saw a human being, the patient suffering of the fowls, and of the desperate efforts made by some rabbits to allay the pangs of hunger with anything to engage the digestion; and it appeared to me that my friend was indifferent. He had been a vivisector some years; I was a beginner.

4909. That he did not feel that?—No; and I judged from this; I told him the reason why I could not either smoke or enjoy my dinner, and he shrugged his shoulders and smiled. I wish to add that my experience is, that it is impossible to argue the point with most professional vivisectors. They appear to me to ignore everything; they see no kind of abuse, and very often no pain. This is the result of habit and *esprit de corps*, from which no one is exempt, and of which we all must be warned about.

4910. Now, is it your view that demonstrational experiments on living animals should be altogether abolished, or only made when they are under chloroform?—I will answer it in this way; I would abolish demonstrations, and if my son was going through his curriculum as a medical student, I would not allow him to go on if he were to witness demonstrations. Let that suffice for part of your question. The remaining part of your question referring to the use of chloroform I would answer in this way, that if you could guarantee that the animal from beginning to end was unconscious, I could see no objection to its being used for demonstration, and provided the animal was immediately and invariably killed when the demonstration was over.

4911. Did you mean to say in your last examination that the only use of these experiments is for the purposes of diagnosis, and that they are hardly of any use to the physician for the purposes of cure?—What I said and what I meant was this, that the use of physiological knowledge to the physician is that it enables him in many cases to localize the seat of the

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disease. I will explain my meaning more by referring to an example brought forward by one of the Commissioners. Take the case of ringworm; my knowledge of the healthy functions of the skin cannot teach me what the cause of ringworm is; but pathology will. Neither will it instruct me as to remedial agents, whereby I may destroy the parasite, but chemistry will. Or take the case of typhoid fever. We have there a rapid and great waste of tissue; we have an ulceration of certain glands; we have the temperature of the body rising to 106 degrees and more; physiological knowledge, that is to say, knowledge of the healthy functions of the human body, cannot explain all these things, nor will it help in the diagnosis. The word "diagnosis" does not simply mean the knowledge whereby disease is localized, but it embraces the cause of the disease, the pathology of the diseases. If I have appeared to be a little undecided in some of my answers it has been from over caution on my part not to allow one truth to displace another, and to endeavour to keep every truth and every science in its proper place, that we may seek and find certain truths where they really can be found. Etiology, pathology, physiology, chemistry, and the action of remedial agents on the healthy human body have all distinct and different truths to reveal to us, and if these things are kept distinct and in the place where nature has placed them herself, I am quite sure that things will go very much easier with the medical student.

4912. (*Chairman.*) Is there anything further that you wish to say?—Nothing more, except to express the satisfaction it will afford me, if the scanty information I have laid before you, will in the smallest measure help you, not only in relieving science of some of the greatest abuses perpetrated in her name, but of sparing many creatures very much pain, and thus dispel also, that which is causing many persons much unhappiness.

The witness withdrew.

Adjourned to to-morrow at 12 o'clock.

Thursday, 4th November 1875.

PRESENT:

THE RIGHT HON. VISCOUNT CARDWELL, IN THE CHAIR.

The Right Hon. LORD WINMARLEIGH.
Sir J. B. KARSLAKE, M.P.
JOHN ERIC ERICHSEN, Esq.
RICHARD HOLT HUTTON, Esq.

N. BAKER, Esq., Secretary.

Mr. JAMES B. MILLS called in and examined.

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4913. (*Chairman.*) You are a veterinary surgeon in the Royal Artillery now at Woolwich?—Yes.

4914. You are aware of a paper which was communicated by you to the Royal Society for the Prevention of Cruelty to Animals, and which has been by them laid before us?—I am.

4915. You went through a college career at Edinburgh?—I did.

4916. Do the veterinary students and the medical students frequently associate for pleasure and for study?—They do.

4917. During your first term were you admitted to any private meetings where experiments were conducted by students alone?—I was.

4918. To how many do you think?—I could not definitely say the exact number.

4919. During the first term to how many were you admitted do you suppose?—Two at least.

4920. Were those private meetings?—Private meetings amongst the students themselves.

4921. Held in their lodgings?—Yes, in their lodgings.

4922. When you became a senior were you introduced to more of them?—Yes, I was.

4923. To a great number?—Yes.

4924. On some of those occasions did you operate yourself?—I did, I am sorry to say.

4925. Were those experiments made to discover any new facts?—No.

4926. What were they for?—Simply to demonstrate things that were perfectly well known to every student almost.

4927. What kind of animals were the subjects of your experiments?—Cats and dogs.

4928. And how were they obtained?—They were either hunted down by means of running after them in the street at night, or sometimes caught by a bait.

4929. Was that a poisoned bait?—It was.

4930. And how were they recovered?—Antidotes were given, and the animals were restored to animation.

4931. Was that a very painful process?—It was.

4932. Now do you consider that those experiments were due to no other motive than idle curiosity and reckless love of experimentation?—Nothing more.

4933. You have got the paper before you that you have signed?—Yes.

4934. Do you undertake to assure the Commission that that paper is only a plain unadorned statement of the facts of which you were witness?—It contains the simple facts.

4935. (*Lord Winmarleigh.*) How long is it since you have given up making experiments on living animals?—Since April 1872, when I obtained my diploma.

4936. Have you any knowledge of what is going on now in that way?—I have a slight knowledge.

4937. What is your belief on the subject?—I consider that a great many unnecessary operations are performed, which entail a considerable amount of cruelty, and no benefit I am confident ever will accrue from them.

4938. You think that is going on now?—I have sufficient evidence to prove that such was going on this last winter session.

4939. Amongst the students?—Yes.

4940. Have you any positive proof of that?—I have from the students there, and those who performed the operations.

4941. (*Chairman.*) Are you prepared to give us the names of the students to whom you refer?—No; I could not do that, because it would be breaking honour with them.

4942. (*Mr. Erichsen.*) Have you any other positive proof, may I ask?—The students were a class of about 70 or 80, and all, I should think, assisted more or less in it.

4943. (*Lord Winmarleigh.*) And you think that that is going on at the present day?—I have not the slightest doubt that it will go on this session again.

4944. To what period do you know positively it did go on?—This last winter session.

4945. When you say "class" you do not mean that

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they were in any positive class when they did this?—No; simply that they were students composing the class at the time in the college.

4946. (*Chairman.*) You only mean, if I understand you rightly, that there was a class of 70 or 80 veterinary students, and that some of them performed these experiments?—Yes.

4947. You do not mean that there were 70 or 80 people in one lodging?—No; this last one was done in the college yard.

4948. In public?—In public; in the yard of the college, the quadrangle.

4949. (*Lord Winmarleigh.*) Was there any professor or any person who had authority presiding over these experiments?—Not that I have any knowledge of. The principal, I believe, knew of it.

4950. But while you were there, you did it amongst yourselves without any superior officer of the college having control over you?—Yes, without any control.

4951. Who had charge of getting these animals?—Anyone that could get hold of them.

4952. May I ask you how you know that that is likely to go on again?—Unless there is a stop put to it, it will still go on.

4953. You think it is such a habit that it is sure to go on unless there is some stop put to it?—It is sure to go on unless there is a stop put to it.

4954. For how long a period do you believe that experiments have gone on such as you describe?—I can only speak from my college career and from the evidence that I have got from students who were there this last winter session.

4955. From your communication with your fellow students do you believe that it has been a practice that has been lasting for some time?—It must have been lasting at least for the last five years; because I entered as a student in the session of 1870–1, and I have evidence of it up till this last winter session 1874–5.

4956. Now will you state the nature of the college itself, the governing body, and the people who have control over the students?—The principal, of course, has the sole control over the professors and students; but those experiments are performed quite unknown to any of the professors there.

4957. I thought you said just now that they were known to some authorities?—In this last case I say, which happened last winter session, it was. The subject that was operated on was a horse, and it was bought for the purpose of dissecting. This animal was subjected during a whole week to operations, such as tenotomy and neurotomy and various minor operations.

4958. Were those operations performed under chloroform?—No; no chloroform whatever was given.

4959. Were no anæsthetics given?—No; none whatever.

4960. Were they painful operations?—Very; the animal was cast by means of hobbles.

4961. And were they operations chiefly on the nerves?—Nerves and tendons; and the animal was bled in different parts of the body for the purpose of the students simply demonstrating to each other things that could be learnt in every-day practice.

4962. What is the custom at the college when the operations are performed under the authority of the college?—As a rule where the operation is very painful the animal is supposed to get chloroform.

4963. Always?—Not in every case; but where there is any great pain, or where it is a delicate operation, and one which involves any minute structure, for the purpose of steadiness chloroform is given.

4964. As regards the college these, to which you have been referring, are entirely exceptional cases, and done without the knowledge of the public authorities?—The principal must have known that this subject was within the precincts of the college; because the animal was kept for a whole week and put out into a paddock adjoining the place, and allowed to live or die there, and was subject to those operations during that whole time.

4965. What was the state of the horse during the time that he was out there to all appearance?—It must have been a very poor appearance, because the subjects which they buy are animals bought at a very low price, and fit for nothing else; and this animal was the subject of a disease of the sensitive laminæ of the fore feet.

4966. And were the experiments made more particularly on that part of the animal?—They were made all over I believe.

4967. But you undertake to say that in the college itself, when under proper control, these experiments are only made under anæsthetics, except under very exceptional circumstances?—Operations of all kinds are performed; when there is any very delicate operation or one involving a great deal of pain, they always give chloroform; but in such cases as firing and minor operations they do not as a rule give chloroform.

4968. But experiments for purposes of research are always done under chloroform?—Yes.

4969. (*Sir J. B. Karslake.*) Was or was not that an unintentional answer of yours, "for purposes of research?" Were any experiments upon living animals exhibited for purposes of demonstration or research to the students?—In some cases they are performed for the purpose of demonstration and in some cases they are performed for disease.

4970. I want to know this, were animals bought and operated upon simply for the purpose of exhibition and demonstration?—They were.

4971. In the College?—They were.

4972. That was so when you were a student in the College, was it?—Yes.

4973. Was it done at lectures?—No.

4974. When?—It was done either during the dissecting hours between 10 and 12, or in the afternoon.

4975. Now what animals were operated upon simply for the purposes of demonstration?—Horses and donkeys.

4976. Were those experiments made frequently in the presence of the class?—Not frequently; because there is a great difficulty in getting subjects.

4977. Were they made by the professor or by students under the superintendence of the professor?—Sometimes by the students themselves, and sometimes by the professor.

4978. When they were made by the students were they always made under the superintendence of the professor?—Yes.

4979. And with their knowledge?—Yes, if within the precints of the college.

4980. I am upon that at present; now what number of years were you in attendance upon the college?—I was so in 1870–1, and 1871–2; two sessions.

4981. Were there any other animals operated upon for the purposes of demonstration except donkeys and horses?—Dogs.

4982. Now can you tell me in the first session that you were there, what operations were performed upon those animals?—Lithotomy was performed sometimes.

4983. On what?—On a horse.

4984. Bought for the purpose?—Bought for the purpose of dissecting; and previous to the animal being destroyed for the dissecting room, the operation was performed. The animal was under chloroform.

4985. And was he destroyed immediately afterwards?—He was allowed to live about 12 to 14 hours.

4986. And the effect of the chloroform had passed away before that?—Yes, and the animal lay in a semi-comatose state till he was destroyed.

4987. Was there any repetition of the dose of chloroform to keep him in a comatose state?—No.

4988. Then in your judgment after the operation was performed, and when the effect of the chloroform had passed away, was he suffering severe pain?—He must have been suffering a considerable amount.

4989. Was that operation performed by a professor?—It was.

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4990. Will you give me his name ?—I do not know whether that would be right.

4991. There can be no objection if it was done publicly ?—Principal Williams.

4992. That was an operation upon a horse of lithotomy ?—Yes.

4993. In your judgment was it necessary, with the experience and knowledge which you now have, for the purpose of enabling the students to understand the nature of that operation, that it should be performed on a living animal ?—It is a very rare case that lithotomy is performed on the horse ; and when it does get to that extent the disease calculus in the bladder, it is far better to destroy the animal, because if the operation is performed the animal is of very little use afterwards.

4994. That is hardly an answer to my question. I want to know supposing it should be necessary or expedient in the earlier stages of the existence of stone to perform the operation, would the student learn to perform the operation as well from dissection of a dead animal as from the operation upon the living animal ?—Well, I should think he could ; because the animal was as it were dead under the influence of chloroform, and while the animal was actually dead he would be exactly in the same position.

4995. It is the case, is it not, that horses do suffer very much from stone ?—It is not an uncommon disease ; it is not a very rare disease.

4996. That is the first instance you give of an operation upon a living animal, as I understand, for the purposes of demonstration merely ?—Yes, by a professor of the college.

4997. I am talking of those cases entirely now ; what was the next ?—In the next the subject was the opening of the jugular vein and blowing back of blood.

4998. What was that performed on ?—A pony.

4999. Was it for the purpose of destroying the animal ?—I cannot exactly explain the physiological effect, but it was for the purpose of demonstrating some fact which there had been some controversy about.

5000. By whom was that performed ?—By the principal and his assistant.

5001. Principal Williams ?—Yes.

5002. Now was that animal put under chloroform ? —He was not.

5003. What was the effect of the operation ?—The animal staggered for a time and then regained his feet, and then they pinned up the vessel and allowed the animal to live some time before he was destroyed.

5004. Did he seem to suffer pain after the operation had been performed ?—No, not any appreciable pain.

5005. And was the pony bought for the purposes of dissection ?—Yes, it was bought for the purposes of dissection.

5006. Was that in the first session in which you were a student ?—In the first session.

5007. Was there any other operation of the same class performed upon animals in the same way in that session ?—Not that I can remember.

5008. Were any operations performed upon dogs ? —Not that session.

5009. Now let us have the second session ; what was done then ?—A dog was brought into the college for the purpose of being poisoned. This is not exactly a case of vivisection.

5010. He was brought in for the purpose of having poison administered to him ?—Yes.

5011. What was done ?—He got a dose of hydrocyanic acid, that is to say prussic acid.

5012. (*Lord Winmarleigh.*) Was all this under the eye of a professor ?—I cannot say whether the professor saw this.

5013. (*Sir J. B. Karslake.*) This was done in the laboratory or theatre of the college ?—Yes, the dog got the poison in the surgery I believe, or in the dissecting room.

5014. He was brought into the college, as I understand, for the purpose of being poisoned ?—Yes.

5015. Was that with the knowledge of the principal ?—Yes, it was, or with that of his assistant.

5016. Then the dose of prussic acid was administered probably in the surgery, and was he then exhibited to the students ?—He was then dragged up to the dissecting room and allowed to lie there for a time, and ammonia was given to him as an antidote, and they tried to bring the animal to life again as it were, and he showed signs of life ; and then one of the students knocked his brains out.

5017. What was the object of this exhibition, as far as you understood ?—Simply nothing. It was after he got into the dissecting room that this was perpetrated on the animal.

5018. I suppose the professor did not bring the dog into the surgery simply for the purpose of poisoning him ?—He did ; the dog was brought to the college for the purpose of poisoning him.

5019. Was he brought to the college for the purpose of being destroyed ?—Yes.

5020. By the wish of his owner, do you mean ?— By the wish of his owner in all likelihood.

5021. Then having had the dose of prussic acid administered in the surgery he was dragged by somebody up to the dissecting room ?—Yes.

5022. Who dragged him in ?—I cannot say.

5023. Was it with the knowledge of the professor or his assistant ?—The professor might have known that the animal was being taken up to the dissecting room, but I am confident that the professor did not know that the animal was subjected to that fearful cruelty after he got up there.

5024. How many students were there in the dissecting room ?—About 40.

5025. Was it generally agreed that the dog should have a dose of ammonia ?—When they saw the animal was not exactly dead they rushed down and got some ammonia to bring him to life again, and he did partially come to life again.

5026. Did you understand why they brought him to life again, having known that he was brought there for the purpose of being destroyed ?—Simply to satisfy their idle curiosity.

5027. What idle curiosity ? to see if ammonia would have the effect of an antidote ?—Yes.

5028. That was done, as far as you know, without the knowledge of the professors ?—Yes, I am sure they did not know of it.

5029. How long was he kept in this state ?—Ten minutes to a quarter of an hour, I should think.

5030. Before his brains were knocked out ?—Yes.

5031. Was the same principal the head of the college at that time ?—Yes, Principal Williams.

5032. And how many assistants had he ?—He had one, that is to say, one clinical assistant.

5033. One who would be connected with the dissecting room ?—No, there were præsectors chosen from the students besides for that ; for the purpose of demonstrating in anatomy in the dissecting room.

5034. Was there any other instance besides that, in your second year, in which with the assent of the principal and the authorities of the college vivisection was exhibited ?—One case of tenotomy.

5035. On what ?—On a horse.

5036. Was that in the second session ?—Yes.

5037. Was that horse a horse that was bought for the purposes of dissection ?—For the purposes of dissection.

5038. Who operated upon him ?—The principal.

5039. Mr. Williams ?—Yes.

5040. And what was done ?—Tenotomy was performed on the tendons of the near fore leg.

5041. Was the horse put under chloroform for the purpose ?—No.

5042. Under ordinary circumstances, when an operation of that sort was performed upon a horse for the purpose of curing it, would chloroform be administered ?—It would be, as a rule.

5043. You have seen that done ?—No, I have not seen it done, not for the purpose of being of use to the horse.

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5044. (*Mr. Erichsen.*) Is there any appreciable pain in tenotomy in a horse ?—Yes.

5045. (*Sir J. B. Karslake.*) Is it anything comparable to the pain of firing a horse ?—It is not comparable to that.

5046. That is extremely painful, I believe, though not dangerous ?—Yes.

5047. As I understand from you chloroform is not administered when a horse is fired ?—In this college it is not.

5048. Do you administer it yourself ?—Yes.

5049. You find it useful ?—Yes.

5050. Is there any difficulty in it ?—None whatever, if done with care.

5051. In your own practice in firing horses do you always administer chloroform, or sometimes not ?—Sometimes not, and sometimes I do, depending entirely upon the extent of the operation. If it is a large surface that has to be fired, as a rule chloroform is given, but if it is a small minor operation, we do not do it.

5052. The minor operation causes considerable pain, does it not ?—Not to the same extent as firing a large surface.

5053. Now may I take it that those were all the experiments exhibited upon living animals, the three which you have mentioned to us, during the time that you were at college ?—They were.

5054. One on a horse and one on a pony in the first session, and this one on a horse in the second ?—Yes.

5055. Now I understood you to say that the experiments that were made in private were made by students in the veterinary college and in the medical college ?—A few of the medical college students were always mixed with the veterinary students.

5056. Did they meet and make arrangements that they would perform these operations ?—Yes.

5057. And you were one of them ?—I was one of the veterinary students, I am sorry to say.

5058. In the first session did you always meet in the same room ?—It depended entirely who had got the animal, and whose lodgings the animal was in.

5059. I infer from what you say that you met in different students' rooms ?—Yes.

5060. Was it the subject of conversation generally among students that these experiments were going on ? The subject of conversation when we were together quietly ; we did not make it a public thing at all.

5061. Did you ever communicate to the professors that you were doing this ?—Never those private experiments.

5062. Did you assume that it would have been distasteful to the professors if you had told them ?—I cannot say.

5063. Did you intentionally keep it secret from the professors ?—We did ; we kept it private from everyon eexcept ourselves.

5064. Was there any arrangement made before him as to what particular operations should be performed on an animal when you had secured one ?—None whatever.

5065. Now take the second session when you were an advanced student, how many operations did you witness do you think altogether ?—Three.

5066. And in the first ?—Two.

5067. That is the whole number ?—Yes.

5068. Was it in the second session that you yourself operated ?—The second session.

5069. What did you do ; had you secured the animal in the first place yourself ?—I secured one of the animals.

5070. Was more than one operated upon on that occasion ?—No, only one.

5071. Then what was the animal upon which you operated, and which you had secured ?—A cat.

5072. How many students were present ?—Six to eight.

5073. And of a mixed class, some veterinary students and some in the medical college ?—Yes, there were two medical students, I know.

5074. What did you do to the cat ?—We opened the thorax.

5075. (*Mr. Hutton.*) Is this the experiment described in the letter read to us by Mr. Colam ?—Something similar to that experiment. That seemed to be the favourite one of all.

5076. (*Sir J. B. Karslake.*) And what was done then ?—It was simply to see the pulsating of the heart.

5077. Was there any particular student (I do not ask his name at the present moment) who suggested the expediency of trying this experiment of opening the thorax ?—It was a general idea with us all.

5078. Do you know where they got the idea from ?—I have not the slightest knowledge, except from reading.

5079. Nobody in the college as far as you know suggested it ?—The students amongst themselves might have suggested it to each other.

5080. Nobody higher in the College suggested it ?—I should think nobody did.

5081. You operated upon the cat ?—Yes.

5082. Not under chloroform ?—Not under chloroform.

5083. What became of the cat ?—The cat died after the operation.

5084. How soon after ?—Seven to eight minutes, that is to say, after the pulsation had ceased.

5085. Was each of the operations, which you have mentioned as performed in the private rooms of the students, an operation of the same class and character, or was there anything else besides cutting the thorax ?—There was the picking up of different blood-vessels and nerves.

5086. How often was that done ?—It was done in two of the subjects.

5087. Was that done also without any suggestion, as far as you know, upon the part of the authorities ?—Yes ; simply to see the course of the nerve, and pick it up with the forceps and cut it across ; anything of that kind, simply for the purpose of dissection.

5088. Now you have told us, have you, all that was done in your time ?—Yes.

5089. And all that you knew of as being done ?—All that I know of.

5090. Were they generally the same set who carried on these operations ?—There would be a slight change the second session ; because there would be some students gone who had passed their examination in the following spring.

5091. And others introduced ?—And others introduced.

5092. Was it a sort of secret society ?—As a rule in a college like that, so many students have a sort of grinding class together, and associate together.

5093. What would be the whole number of students at that time in the two colleges, the medical school and the veterinary school ?—I could not say how many in the medical school ; I have not the slightest idea.

5094. In your own school how many ?—About 80 or 90.

5095. And how many of them in the first session were in the society or group of students which practised this vivisection ?—It would be from four to five, never more.

5096. And two or three of the medical school ?—Generally two medical students, friends of some of the others.

5097. Were the same medical students in the society in the second session, or were they different ?—There was only one in the second session, and he was a different man altogether.

5098. Then the two had gone, and one other man joined your society ?—It was not exactly a society.

5099. I do not use the word "society" except as meaning a vivisecting class, or group, or whatever you choose to call it ?—It was a certain number of students.

5100. Now, as I understand you, as far as you know, during the whole of the two sessions you and your friends or associates were the only persons in the two colleges who practised this sort of inquiry ?—They were all I knew that did.

5101. Now there was the operation which you referred to which was performed, or something which was done in the yard of the college, which you have heard of; last winter I think you said ?—Yes, last winter session.

5102. Was that information communicated to you by letter ?—It was communicated to me verbally.

5103. You decline to give us the name of the gentleman who communicated it to you ?—Yes.

5104. Is the same principal at the college now ?—Yes.

5105. Principal Williams ?—Yes.

5106. Let us just understand, if you please, what you gathered from your friend who communicated the facts to you, was done to this horse. He was a horse who was bought as I understand for the purpose of dissection ?—Yes.

5107. He had numerous experiments tried upon him in the yard of the college ?—The students performed several experiments on him.

5108. I will come to the number and character of them generally by-and-bye. Now, as I understand you, your informant gave you to understand that between whiles the animal was turned out in the paddock ?—Yes, the animal was turned out in the paddock adjoining the college.

5109. And was kept alive altogether for more than a week ?—Yes, more than a week. I can say positively for a week.

5110. I suppose during that time no operation was performed on him for laminated feet ?—Neurotomy, I think, was the first operation performed on him for the purpose of destroying the sensibility of the feet.

5111. That was really, I suppose, to put him out of pain as far as the feet were concerned ?—To see if it would put him out of pain.

5112. Supposing it was inconvenient to the professor to have the animal used in the dissecting room theatre for a time, and he was suffering severely from laminated feet, would it be in the course of business in the college to perform that operation in order to save him from pain in the meantime ?—No, because an animal that would be neurotomised on both fore feet, and especially one suffering from an acute disease, in all probability during the progress of the disease would have the foot sloughing off and different complications in which the disease terminates, which render the animal totally useless.

5113. I understand that; but I mean as a matter of humanity, if the animal was not to be used at once in the dissecting room, when suffering from a severe disease, such as laminitis, is it a means of relieving the pain in the feet to perform this operation of which we are speaking ?—It is a means of relieving pain decidedly.

5114. Supposing then that the professor had had the horse into the college, and did not want to use it for dissection for a week, and he was suffering under a severe affection of the feet, such as laminitis, would it be in the course of the professor's duty to perform this operation to prevent pain in the diseased feet in the meantime ?—Not if the animal is to be kept after the disease has terminated.

5115. I am assuming that he is going to be dissected; would it not relieve him of pain in the meantime ?—It would relieve him of a considerable amount of pain.

5116. It would relieve him of a considerable amount of pain if it was necessary to keep him a week for dissection ?—Yes, it would.

5117. That was the first operation that was performed on that horse ?—Yes.

5118. After the first operation was performed, how many other operations were performed according to your informant's statement before the horse was killed ?—My informant said that tenotomy was performed, and he was bled in different parts of the body, and several other operations, he said.

5119. Now I suppose that the marks of those different operations must have been visible on the body of the animal when he was killed for dissection? —Decidedly so.

5120. Who is intrusted in the Veterinary College with the care of these animals until they are killed for dissection ?—As a rule they are put into the paddock near the college, to graze there until they are required.

5121. Has any one of the authorities of the college the care of them ?—No, simply a groom sees that they do not get away.

5122. Did you hear whether this horse was actually killed and dissected at last ?—It was, after a week's time.

5123. Do you know at all whether there was any other horse suffering from the disease of laminitis at the same time ?—I did not inquire, but I am confident I can prove on this special subject that this was the animal that was operated on.

5124. I only want to know, for the purpose of identifying the animal, whether you are aware whether any other animal suffering from laminitis was in the paddock at the time ?—It would not be likely at that period of the year.

5125. As I understand you this was during the session of the college ?—During the winter session.

5126. So that dissections would be going on and the professors would be delivering their lectures and demonstrating during the time that these operations were being performed on the horse ?—Yes, it was during the session of lectures.

5127. (*Mr. Erichsen.*) What tendon was divided in the operation of tenotomy ?—The perforans and perforatis.

5128. Is tenotomy sometimes not done in laminitis ?—No.

5129. But neurotomy is a merciful method of relieving the animal from pain ?—It is.

5130. And is commonly employed by veterinary surgeons ?—Yes.

5131. And it was consequently not an experiment but a means of relief or cure ?—I am confident it was not so then. It was done by the students to see who could perform the operation best.

5132. The result, however, was relief of pain ?—It did relieve the animal; the animal went sound afterwards.

5133. That experiment was performed in the yard, you say. Is that yard overlooked ?—No.

5134. So that although it is a yard these experiments can be done in it in secret ?—Yes; it is a sort of square, a little bit from Edinburgh, and no houses near it.

5135. Is it in the college itself ?—In the buildings of the new college.

5136. You spoke of a dog having been brought there to be poisoned by the consent or by the wish of of his master, and you said that prussic acid was administered to that animal. Is not the administration of prussic acid about the most merciful means of killing an animal ?—Yes.

5137. There is no pain whatever, so far as you know, either in man or animal, after the administration of prussic acid, is there ?—There is not.

5138. How much prussic acid was given to that dog ?—I could not say the amount.

5139. Does not a very small amount, an amount of a very few drops, of prussic acid destroy an animal ?—A very few drops.

5140. So that it must have been a matter of accident if enough was not given to the animal to kill him ?—A considerable quantity was probably given to the dog, but he would pour out a good deal of it from his mouth.

5141. But even then have you ever seen an animal, into whose mouth a considerable quantity has been poured survive such an operation ?—No.

5142. Have you ever heard of such a thing ?—No.

5143. Will it be in the range of possibility to pour, say a tea-spoonful of prussic acid, into a dog's mouth, and for the dog to eject enough to live afterwards ?—

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No; but this particular dog cannot have had enough to cause death.

5144. (*Mr. Hutton.*) Was it diluted?—It was Scheele's prussic acid.

5145. (*Mr. Erichsen.*) About two or three drops of Scheele's prussic acid is enough to kill a man, is it not?—Yes.

5146. And a quarter of a drop will kill a dog, will it not?—No.

5147. How many drops are required?—I do not know the minimum dose.

5148. Scheele's prussic acid is very strong, much stronger than that which is used in medicine?—Yes.

5149. (*Chairman.*) I understood you to point to this, that this particular dog had not had prussic acid enough to destroy life?—Just so.

5150. And as soon as the students found that to be so they seized, I understand, upon him for the purpose of trying upon him an experiment with regard to an antidote?—Yes.

5151. And that experiment with regard to an antidote was an unauthorised experiment, done at the volition of the students themselves?—Yes.

5152. And you consider it to have been a cruel experiment?—Yes.

5153. And you consider that students ought not to have the power of performing a cruel experiment of that sort on their own hook?—Decidedly not.

5154. (*Mr. Erichsen.*) Will you tell me why the administration of an antidote is a cruel experiment?—The administration of this antidote would not have been cruel in itself but for the brutal usage the animal was put to afterwards.

5155. What was that "brutal usage"?—The brains were knocked out with a hammer, the skull was fractured, and the brains protruded.

5156. With regard to these experiments performed on cats in lodgings, I presume that there were other people besides students lodging in the houses in which those experiments were performed?—Only the landlady and her husband, as far as I know.

5157. Did the students occupy the whole of the rest of the house?—There were three students occupied apartments in the house.

5158. And did not the noise, the cries of the cat whose chest was being cut open, disturb the neighbourhood?—Yes, and we actually killed the landlady's cat, and very nearly got into a great deal of trouble about it.

5159. Was no public complaint made?—No public complaint was made, but we were threatened that they would make it public.

5160. Your experience is derived entirely from the proceedings of the Veterinary College at Edinburgh?—Yes.

5161. Not in any way from the Veterinary College in London?—No, I have never been there.

5162. Nor in any way from matters connected with the medical school in Edinburgh?—No, I do not know much about it.

5163. You have never attended the physiological laboratory there?—I have not.

5164. You know nothing of what goes on in the medical schools from your own observation?—Not from my own actual observation.

5165. (*Lord Winmarleigh.*) I think you said that that dog was sent by the owner to the college for the purpose of being destroyed?—Yes.

5166. (*Mr. Erichsen.*) It was a diseased dog I suppose?—I could not say whether the animal was diseased or not. It might have been an old dog, or it might have been doing something wrong.

5167. (*Mr. Hutton.*) I understood you to say, in answer to Sir John Karslake, that you only saw five private experiments in lodgings during the two sessions you were at the college?—Yes, only five.

5168. And I think you said that in none of those cases was there anything like chloroform administered?—No. There was, I think, in the first session.

5169. I read in your letter,—"In a few cases the "animals were narcotised when no suffering was "caused either in the process of poisoning or in the "after experimentations." Do you mean there, narcotised by poisoned bait?—Yes.

5170. You mean that the drug on the bait was a narcotic in some cases?—Yes.

5171. And that in those cases the animal did not suffer?—No; but the majority of cases were done without that.

5172. Do you mean that that applied to two out of five, or three out of five?—It would be about two out of the five in which this was done.

5173. Were you not lecturer on materia medica in this college for a time?—Yes.

5174. That was after the time when you were a student there?—Yes.

5175. Did anything happen of this kind while you were lecturing?—I never heard of anything. Of course I was not then associated with the students privately.

5176. But such a thing as a horse being a week in the yard and being experimented on would have come to your knowledge, I suppose?—It would have done if it had happened; but it was in the summer session that I was lecturing, and the consequence was that they did not dissect then owing to the heat.

5177. You were only lecturing for a single term of three months?—Yes.

5178. And during that time you heard of nothing of the kind?—No.

5179. Did you understand from your informant that in this case of the horse, which occurred last winter, the poor creature suffered a great deal?—Yes, a very great deal.

5180. Did he say which of the experiments were the most painful?—He did not.

5181. (*Chairman.*) You spoke of the four or five students who were associated with yourself for the purpose of experiments. Have you any means of knowing whether you were the only four or five who were associated, or were there other knots of four or five who were associated for the same sort of purpose?—I have not the means at present, but I might have.

5182. You do not know at all?—I did not know at the time.

5183. And do you now know?—I do not.

The witness withdrew.

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Mr. William Benjamin Archibald Scott, M.D., called in and examined.

5184. (*Chairman.*) You are in practice as a physician at Willesden, near London?—Yes.

5185. And you are a doctor of medicine of Edinburgh, and a licentiate of the Royal College of Physicians of Edinburgh, and of the Faculty of Physicians and Surgeons of Glasgow?—Yes.

5186. Have you been in communication, with a view to this inquiry which we are holding, with the Royal Society for the Prevention of Cruelty to Animals, and also with the Society for the Abolition of Vivisection?—I have with Mr. Jesse. I can hardly say that I have been exactly in communication with the Royal Society for the Prevention of Cruelty to Animals. I had a letter from Mr. Colam mentioning that he had mentioned my name to the Commission.

5187. You have taken an interest in the subject which is the subject of our inquiry?—Yes.

5188. Have you any experience in the subject of experiments on living animals?—I have witnessed two or three, and should have witnessed more, only I gave over attendance at a physiological laboratory.

5189. What was the reason of your giving it over?—Because I was disgusted.

5190. And where was it you saw those experi-

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ments?—The physiological laboratory I attended was in the University of Edinburgh, in the year 1871.

5191. And were the experiments that you saw during the short time that you attended there of a very painful nature?—I only witnessed one; I left after the first; I shall be happy to describe that.

5192. Will you do so, if you please?—Several frogs were poisoned with curari or wurari, which destroys motor power without destroying or even impairing sensibility (some think it even increases it). In that state their bellies were ripped open, the mesentery sufficiently exposed to be placed on the microscope, still in connexion with the living animal, and a class of certainly not less than 50 or 60, I should say more like 70 or 80 students, each had his turn at one of the microscopes to examine the circulation in the mesentery under the circumstances; the object being, I can hardly say to determine, but to illustrate a point which the professor, Professor Bennett, was very fond of maintaining in opposition to some others, namely, that during the process of inflammation the white cells of the blood did not pass through the walls of the capillaries and form the pus cells, but that the pus cells were formed anew outside the vessels in the extravasated liquor sanguinis. This experiment struck me as the more peculiarly wicked, inasmuch as the professor of pathology stated that in order to witness the extravasation of the cells, which he believed really took place, an examination of a good many hours would be necessary, even in one of the higher animals. Now the time that each one of the students could have examined this frog was certainly not more than two minutes. These creatures cannot have been in pain for less than two hours, including the time of preparation and examinations; and I took especial pains to ask the assistant, Dr. McKendrick, whether any means were adopted to make the animals insensible to pain, and he said he was sorry to say there were not. I thought after that I had had quite enough of it, and I did not attend the class any longer. Then as to the question of curari not rendering the animals insensible to pain, I may state that there is an experiment in a work of the highest authority, by Drs. Klein and Burdon-Sanderson and others, expressly designed to show that very point.

5193. Then speaking from the experience which you have told us, and from your general knowledge with regard to the lectures at Edinburgh, you consider that in respect of frogs the experiments are extremely painful, that no effectual measure is taken to diminish the pain, and that the suffering is very considerable both in its nature and in the duration for which the animal is subjected to it?—I do, and in regard to some other animals still more. I have heard the professor of pathology say that it might be necessary to keep an animal for, I think he must have meant at least 12 hours; not a frog, but a cat or a dog in the same circumstances, to determine a similar question; and in that work by Drs. Burdon-Sanderson and Klein it is mentioned that in watching the process of inflammation in the tongue of a frog it may be necessary to keep the animal alive for 48 hours, and as of course one person cannot examine 48 hours on end, it may be necessary to replace the tongue in the mouth, and have it out again when you wish to examine it.

5194. Have you any observations that you wish to address to the Commission?—I wish to address some observations to the Commission in proof of the assertion that the convenience of the operator and not the comfort of the animal is the sole object consulted, and in order to illustrate that, I would quote from chapter 31 of the "Handbook for the Physiological Laboratory," by Drs. Brunton, Foster, Klein, and Burdon-Sanderson, which is the chapter on "Urari poisoning and independent muscular irritability." The first observation in that chapter is this, "Introduce beneath the skin of the back of a strong frog a drop or two of a solution of urari (the exact strength of the solution and the dose required will depend on the source from which the urari has been obtained). In a short time the frog will be found perfectly motionless with its respiration arrested, but its heart still beating." In many cases respiration is kept up by artificial means. "Lay bare the sciatic nerve in the thigh, slip under it a pair of electrodes connected with an induction coil, and stimulate the nerve with an interrupted current, taking care that there is no escape of the current into the surrounding muscles. This may be effected by slipping under the electrodes a small piece of india-rubber sheeting; if the animal has been thoroughly poisoned, no contractions whatever in the muscles of the leg will follow upon the application of a stimulus, however strong, to the nerve. If contractions do make their appearance the poisoning is not complete; and the student must wait or inject a further quantity of the poison. The nerve having been proved to be insensible to stimuli, lay bare any of the muscles of the leg and apply the electrodes directly to them. Contractions will be manifest upon the application of a very slight stimulus." Then it goes on "the effect of urari is to destroy (or suspend) the irritability of nerves but not that of muscles." Now that expression, though perfectly correct, and not likely to mislead any medical student, might possibly lead non-medical men to suppose that by destroying the irritability of nerves was meant destroying sensory power. However, as you will see in a second it does not mean that, but only means that it destroys the power of conveying a motor impulse. Now I have made this note to that. In all the above not a word is said about giving the poor animal any chloroform, because (and I think this is noteworthy), the urari being injected before the experiment, the animal is from the beginning incapable of writhing in its agonies so as to disturb the operator. In the next observation it is coolly suggested that "a slight dose of chloroform" may be given to the animal during its preparation; why? because as it has not then had any urari, its writhings would interfere with the necessary and exquisitely painful preliminary dissection. The operator's convenience is clearly alone considered. This next observation is expressly designed to show that sensibility remains. "Observation 2.—In a strong frog make an incision through the skin between the ilium and coccyx along the line *k m*, figure 266, cut cautiously through the ileo coccygeal muscle until the peritoneal cavity is reached. The three nerves which go to form the sciatic nerve will come into view when the sides of the wound are held apart. Very cautiously, by means of a small aneurism needle, pass a thread under these nerves, putting it under from the outside, and bringing it out again on the median side. Be very careful not to wound the blood vessels. Repeat the same process on the other side, passing the same thread under the nerves of that side too, but putting it in at the median side and bringing it out at the outside. The thread will now be in the position of the line *o*, *p*, *q*, in figure 266, with the nerves of one side lying over it between *o* and *p*, and those of the other side over it between *p* and *q*." To make this intelligible in the absence of the figure, I may explain that this means that the ligature includes the vessels, but excludes the nerves. "Tie the thread very tightly round the abdomen so as to check entirely the flow of blood to the lower limbs. All this may be done under a slight dose of chloroform." The writhings of the tortured animal would otherwise impede the delicate and intensely painful dissection, since no urari has at this stage been given to paralysec motor power. It would not have done to give curari at this stage, because the object of the experiment is to show the difference in the parts of the system which have received the urari from those which have not. Now if the urari were administered before the arteries have been tied, clearly the limbs, as well as the body, would have received the poison. "The nerves thus form the only means of communication between the hind limbs and the trunk, the vascular communication being entirely stopped. Now inject a small quantity

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"of urari into the back and wait until the poison has "had time to produce its effects in that part of the "body to which alone it has access, viz., the part "above the ligature;" and it may be remarked that by this time the anæsthetic effects of the "slight" dose of chloroform, which it is humanely suggested may be given will have passed away. Then the professor goes on, "The following facts may then be determined. "Though there are no voluntary movements in the "body, head, or fore limbs," (those are the parts which have been urarised,) "some slight (voluntary?) "movements may sometimes be witnessed in the hind "limbs. Pinching or otherwise stimulating either "hind foot may produce movements in either one or "both hind limbs, but in no other part of the body. "Pinching or otherwise stimulating the skin of the "head, fore limbs, or trunk above the ligature may "produce movements in the hind limbs but in no "other part of the body;" because all the rest of the body has been curarised and is incapable of movement; and therefore the animal can only show its pain by those limbs that remain uncurarised. "These facts," the professor says, "are intelligible only on the "hypothesis that the urari has destroyed (or sus- "pended) the irritability of the motor nerves in that "part of the body to which, by means of the blood "current, it has had access, but has not destroyed "the irritability of the sensory nerves" (these are the very words of the book) "or of the central nervous "system. Pinching the skin of the fore limb gave "rise to an afferent nervous impulse." Then there is a third observation, merely describing a very painful operation to be performed on the frog, without one word of giving chloroform even during the preparation of the animal. The same remark applies to the fourth observation. I will just read the preparation in the third observation. "In a fresh strong frog lay "bare the sciatic nerve on one side, say the right, in "its lower course, place a ligature under it near "where it divides into its two branches, and tie the "ligature tightly round the leg above the knee. "The circulation of the lower right leg will thus be "completely arrested, but inasmuch as the nerve is "not included in the ligature there will be complete "nervous connection between the right lower leg and "the rest of the body. Poison with urari." There is not a word about giving chloroform there. The fourth observation is:—"In a fresh strong frog, "dissect out a gastrocnemius (or any other single "muscle), dividing both insertion and origin an "ligaturing its blood vessels, thus leaving it connected "with the rest of the body by its nerve only. Poison "the frog with curari." And it is not even said that a "slight dose of chloroform" may be administered. That is the testimony of quite the standard book in England on the subject; a work which in the preface is said to be "intended for beginners," for mere students, to seek a place in the laboratories. And I may just refer to a passage in the same work, page 162, to show the length of time for which animals are sometimes kept in pain. "The tongue is prepared as "for the study of circulation. The mucous membrane "covering the large lymphatic sac of the under "surface is snipped off with curved scissors. The "observation is necessarily tedious, often lasting for "forty-eight hours. It is therefore desirable to "replace the tongue in the mouth for a time after "each examination."

5195. Have you any further observations that you wish to make, about what you have yourself seen?—I once saw a frog poisoned with curari, and the heart exposed to show that there was what the professor called a living heart in a dead body.

5196. But at that time when he called it "a dead body" had the usual precaution been taken to pith the frog or otherwise destroy its sensibility?—Not so far as I am aware.

5197. What did he mean then by calling it a dead body?—He meant that the body was immovable, that the curari had poisoned the motor power, and the feet could not move at all. And frogs used to be handed round, I remember, in the Physiological Lectures in University College, to show the lymphatic circulation. I cannot say positively and distinctly that they had not been narcotised, but I am as sure as that I am sitting here, that nothing was said about it.

5198. Your opinion is that the natural sensitiveness of the frog is such, that experiments ought not to be performed upon it without some preparation to remove that sensitiveness?—I think not even on a frog certainly. But then curari is used with higher animals than frogs; it is used with cats and dogs and guineapigs.

5199. Do you mean that there are instances within your own knowledge of experiments, painful in their nature, made upon cats, dogs, and guineapigs, with no other anæsthetic than curari, if curari is to be supposed to be an anæsthetic?—Well, I can positively say this, that cats and dogs and guineapigs have been subjected to microscopic examination (in just a similar way to that frog I spoke of), in which chloroform or chloral was certainly not administered the whole time, and in which I believe it was only urari that was used, I did not see the experiments myself; but I have heard them referred to by the professor of pathology in Edinburgh.

5200. (*Lord Winmarleigh.*) On what occasion was it that you left the college when experiments were performed that you thought unnecessarily cruel?—I did not leave during the performance of the experiment; I never returned to the lecture afterwards. It was in June 1871.

5201. Was it the cause at the time amongst those who witnessed it of any exhibition of abhorrence?—No, not the slightest. I never knew an operation cause the least abhorrence to a medical student.

5202. It was done by the professor?—And his assistant. I cannot say whether it was positively the professor or his assistant who prepared the frog; I should think probably both.

5203. Was this in a regular course of lectures?—It was not exactly a lecture, because it was instruction in practical physiology in the laboratory.

5204. It was an experiment of research, as I understand you, then?—Well, it might be called one of research, but its value as an experiment of research would be positively nil. It was very well as showing the circulation, which could have been shown in a hundred other ways.

5205. Do you know with what especial object the experiment was performed?—The object was to prove, so far as it did prove, one of the professor's own points. There was a great division of opinion as to whether, in the process of inflammation, the white cells of the blood pass out through the walls of the smaller vessels into the surrounding tissues, becoming the pus cells, or whether the pus cells are altogether formed *de novo* just from the extravasated liquid. Professor Bennett was of opinion that they were a new formation; and he said that if we looked at these frogs we should see that, although this inflammation had been set up in the mesentery, there were no cells passing through the walls of the vessels. But as each student could not by any possibility have looked at the object for more than two minutes, and as the professor of pathology admitted that an examination of several hours was necessary even for the higher animals, it was quite ridiculous to call that an experiment of research; and besides it was not likely that any of the students would have contradicted the theory of the professor.

5206. But would the discovery of such a truth by such an experiment have led to anything material capable of application to the human frame?—Not in the most distant degree in the world. It was a matter of mere curiosity; nobody ever professed that it had the slightest bearing on anything whatever, except as a mere matter of curiosity. It had to do with the general question in dispute, not with the practice of medicine. Some people think that cells can only spring from other cells; other people think that they can be

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formed anew in a liquid, and Dr. Bennett was of that opinion.

5207. Then may we take it that there is no action in the human body of a similar kind?—There are the cells in the human body similar to those in the frog; but I mean this, that whether you consider with Professor Bennett that in inflammation these pus-cells are formed *de novo*, or whether you consider with some other observers that they are formed, at any rate in part, by the passage of cells through the walls of the vessels, which ever view you take of it, the treatment remains just the same, and it is not a point of the slightest practical importance, and nobody ever pretended that it was.

5208. But is it not one of the objects of research at the present moment to discover what is the origin of inflammation?—It is an object of research, but they have never shown that there is any practical result to be determined from a question of detail like that.

5209. You think that it does not form an element in the consideration of what is the origin of inflammation?—It may in the theoretical determination of the point. I do not say that it has no bearing on the theoretical question, but it certainly has no bearing on treatment.

5210. Was it an experiment that could not safely or under any circumstances have been performed upon the human being?—I do not see how you possibly could perform it on a human being; because as you have got to watch the inflamed tissue for hours and hours you could not expect a human being to submit himself to it.

5211. (*Mr. Erichsen.*) Is there any transparent tissue in the human being where you could perform that experiment?—No.

5212. (*Lord Winmarleigh.*) Do you give us your opinion as a medical man that that experiment was of no use to medical science?—To medical practice, certainly it was of no use. A thing may be of interest in a merely theoretical point of view, and an experiment may have some tendency to elucidate that, but yet it may have no influence in a practical point of view on the mode of treating the disease in which it occurs.

5213. In short I understand you to give your opinion as a medical man that it was a totally useless experiment?—A perfectly useless experiment.

5214. (*Sir J. B. Karslake.*) Do you say that it is at the present moment not established whether these cells do come through the vessels, or whether they are formed entirely outside the vessels?—It is hard to say whether it is established or not. The preponderance of physiologists certainly consider that the cells do pass through the vessels, that is to say Virchow and the leading physiologists do; but the late Professor Bennett and some others had a theory that they did not.

5215. Then may it be taken that even at the present moment the thing is not absolutely established?—I should think not; my own individual opinion is nothing on a subject like that; but there are good authorities on both sides.

5216. Is it your opinion that no advantage whatever to science will be derived from the conclusive establishment one way or the other, of the question whether these cells do pass through the vessels or are formed outside?—Not one particle.

5217. You cannot conceive that it will do any good whatever?—Not only that; but I do not think any of the people who perform the experiment ever maintain such an idea for a moment.

5218. I am asking you whether in your own opinion, supposing it to be conclusively established by experiment one way or the other, it cannot be of the slightest use to science?—Not the most distant.

5219. (*Chairman.*) Supposing it to be of value in the way of original research, would you think that the exhibition of it whilst still unproved to a large assembly of students was a likely way of settling the question?—Certainly not, especially when each student was informed of the professor's opinion beforehand, and could look at it for two minutes only.

5220. (*Mr. Erichsen.*) I suppose in that case it was an experiment of demonstration, not of original research?—Yes, of demonstration.

5221. Dr. Hughes Bennett would never think of performing an experiment of original research in a laboratory in the presence of 60 or 80 students; he would do it quietly by himself. It was an experiment of demonstration, I apprehend, to show the student the nature of the process of inflammation?—Yes.

5222. Is it your opinion that the advance of medical science has no bearing whatever upon medical practice?—I do not say that; I do not say that it has no bearing whatever in certain departments.

5223. But is it possible to say where its bearing upon medical practice ends; is it possible to say that any given advance in medical science may not at some future period have an important bearing upon medical practice?—I should say that using words in their ordinary acceptation, in common parlance, one might say so of an abstract question like that; to say what is inherently possible or impossible I think is very difficult.

5224. I would give as an illustration the discovery of the circulation of the blood; at the time that it was made it was a pure matter of physiological science, was it not?—Yes.

5225. And without the knowledge gained by the discovery of the circulation of the blood, we could not possibly have known how to treat aneurism. The treatment of aneurism was not known at the time, and we could not have known how to treat such a disease as aneurism if we had not had a prior acquaintance with the mechanism of the circulation of the blood; is not that so?—I do not say that the discovery of the circulation of the blood has had no influence on practice.

5226. I think that your statement has been almost entirely confined to a criticism of Dr. Burdon-Sanderson's work which we have already so frequently had before us?—Yes, but perhaps I may mention the reason of that. The editor of the "Echo" was misled by someone into making the assertion that curari was not used in English laboratories, although it might be abroad; and I thought it was important to correct the impression that that might have led to.

5227. And if I understand you rightly you only witnessed in a physiological laboratory the one single experiment that you have mentioned?—Because I left the next day.

5228. You have never consequently worked at physiology in any way whatever?—Personally, certainly not.

5229. So that you have no practical knowledge and no personal knowledge of the working of physiological laboratories, and you have never worked yourself practically at physiology?—Pardon me, I think that I have a very decided knowledge of the working of physiological laboratories, for I have read that handbook.

5230. But of course any non-professional man can form an opinion from that book?—Besides which I have attended lectures on physiology by Professor Sharpey and Professor Bennett, and on pathology, and I have heard the experiments referred to in them.

5231. But those lectures were class lectures, if I understand?—They were.

5232. I am speaking now of the practical working of a physiological laboratory?—But that was referred to in those lectures.

5233. But I am speaking of personal observation, independent of what you have heard from a lecturer or from anybody else, of the working of a physiological laboratory?—The extent of my attendance in a physiological laboratory is what I have described.

5234. The witnessing of this one single experiment?—That was all in the way of vivisection.

5235. (*Mr. Hutton.*) Would there have been any difficulty in etherising or chloroforming the frog, and still exhibiting the same experiment, if it were a

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desirable experiment to see; would that have been inconsistent with exhibiting it?—It would have been a difficult thing to keep the animal perhaps under chloroform quite so long. And another thing is that the curari certainly ensures greater immobility.

5236. But could not ether have been used with the curari?—I do not know whether it could have been or not, but it was not; because I took the pains to ask the assistant that question.

5237. You studied at Edinburgh, I think you said?—At Edinburgh and in London.

5238. Have you any knowledge at all as to whether the practice of vivisection goes on amongst the students at either of those universities?—Yes; among the students in their own rooms.

5239. Could you give us any personal evidence of that sort?—Only what I have heard them mention themselves. I never went to assist.

5240. You heard that as a matter of common conversation at the time?—Yes.

5241. Was that in Edinburgh or London, or both?—In Edinburgh certainly; in London I think. There was one whom I remember at the present moment at Edinburgh; he used to speak of vivisection on frogs which he performed in his own room.

5242. But you do not remember any cases of vivisection on higher animals than frogs, on cats or dogs, for instance, performed by the students amongst themselves?—No; beyond general report I could not name an instance.

5243. But you could name an instance, though you would prefer not to give the name, of a student who practised these experiments on frogs in his own room?—Yes.

5244. And you are not sure what the practice was at University College?—I am as sure as I could be of anything; I know perfectly well that it is a common thing where students are zealous.

5245. Do you apply that to University College?—To any college.

5246. Your impression is that it is a common thing in all colleges?—Among zealous students, not among idle ones.

5247. (*Mr. Erichsen.*) Consequently it is in the pursuit of science that it is done, and not as a matter of idle curiosity, or amusement, or cruelty?—The pursuit of science is one name to give it.

5248. The supposed pursuit of science we may say?—Or of notoriety.

5249. (*Lord Winmarleigh.*) What did you mean then by saying "zealous students"?—Those anxious to get medals and scholarships, and so forth, and those anxious to get themselves mentioned favourably in professional periodicals.

5250. (*Mr. Erichsen.*) Could you give the name of any one student at University College who practises in his own rooms?—No.

5251. (*Chairman.*) Have you any further observations which you wish to make?—I think not.

The witness withdrew.

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Mr. WICKHAM LEGG, M.D., called in and examined.

5252. (*Chairman.*) Are you demonstrator of morbid anatomy at St. Bartholomew's Hospital?—Yes.

5253. In that capacity have you tried experiments upon living animals?—I have made experiments, but not in the capacity of demonstrator of morbid anatomy.

5254. Have you tried those experiments at St. Bartholomew's Hospital?—In the hospital itself.

5255. I may take it then that you are familiar with the subject which has been referred to us?—Yes, in some degree. I do not suppose that I have performed more than four or five series of experiments altogether; I do not mean individual experiments, but series of experiments.

5256. (*Mr. Hutton.*) You made some experiments on cats, did you not?—Yes.

5257. Were they performed in St. Bartholemew's Hospital or in a private laboratory?—I do not quite know how to express it. There is no physiological laboratory, that can be so called, at St. Bartholomew's Hospital. The laboratory which we have there is a private room belonging to the lecturer on materia medica.

5258. This was a private room of Dr. Brunton's, was it not?—A private room of Dr. Brunton's.

5259. This series of experiments that I have got, mentioned in the ninth volume of the Hospital Reports, was a series on the effect of the ligature of the bile ducts, was it not?—Yes.

5260. Did that series result in the establishing of any new fact not previously known with regard to the effect of these changes on the bile duct?—Undoubtedly.

5261. Were those experiments made anterior or posterior to the experiments on which this manual or handbook edited by Dr. Burdon-Sanderson is founded?—I cannot answer that question; I do not know the date of that publication.

5262. Dr. Burdon-Sanderson's manual was published in 1873, and your paper was also?—The observations themselves were made in the spring. The St. Bartholomew's Hospital Reports were published in October 1873, and my experiments were made in the January and February of the same year, 1873.

5263. Then I suppose this handbook, which was published I think early in the year, was founded on experiments anterior to yours probably, was it not?—I do not know; of course that would be matter of date.

5264. Is that book familiar to you?—I have a copy, and I consult it whenever I wish for information; but I cannot say that I am very well acquainted with it.

5265. Was it in your opinion necessary to operate on so many as 16 cats to ascertain these facts?—Yes, because the facts got out were quite new, such as were unknown I think to anybody before; and of course to demonstrate with anything approaching to truth you must have a large number of observations to make your induction from.

5266. You mean that simply on the doctrine of averages you would want to verify the thing in a considerable number of cases?—Yes, just so.

5267. Would sixteen be enough for such a purpose as that?—I do not know about the doctrine of averages; it commends itself to my mind that that number would be enough.

5268. Now do you think that the cats in this series of experiments were all destroyed as soon as they might have been for the purposes of the experiment; they were allowed to die, were they not?—No, not all.

5269. I think there is hardly any case in which they were not?—You will find, I think, there are two such cases, the last two experiments but one.

5270. But mostly I find they linger for various periods, sometimes for eight, ten, seventeen, and twenty-three days; they linger during a period in which they appear to have been very much diseased. I suppose they were all very much diseased as the result of the operation—Undoubtedly.

5271. Were the diseases which were the result of the operation of a painful kind or not, do you suppose?—Judging from what we see in man, jaundice is not a painful disease; on the contrary, it is attended with a considerable tendency to sleep.

5272. I have had the jaundice, and it struck me as the most painful period of my life; there was sickness with it?—It depends altogether on the kind of jaundice; these animals are not sick.

5273. Do you suppose that they have not the sen-

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sation of sickness although they do not actually vomit?—I cannot tell.

5274. Does it not seem to you that in some cases these cats were rather neglected during their last days. For instance, I find that in one case the cat was said to have been found dead something like four days I think after you believed that he died. Does not that look like a kind of neglect on the part of subordinates?—No; the animal escaped from the room in which it was kept and could not be found.

5275. And when it was found it had been dead apparently for some time, and was in a state of decomposition, was it not?—Yes.

5276. But now take one or two individual cases. At page 170 I find this; Experiment 11:—Black tom cat weighing 6 lbs. and 1¼ ozs. operated on January the 9th. It died on January the 25th. Weight only 3 lbs. 7½ ozs. That is to say it lived about 16 days, wasting all the time. Do you consider that that kind of wasting is a painful thing to an animal?—I should think not from what we see in man. Of course we can only judge from what we see in man.

5277. Would it not have been possible with the same result to kill a great many of these cats after they had the disease three or four days?—No; that last experiment that you are speaking of was perhaps one of the most instructive cases which I came across in my experiments.

5278. The cat must have suffered very badly I suppose?—I doubt if it did.

5279. You speak of "the gall bladder being immensely distended; it is over 75 millimetres in length; it is full of green bile. The ducts outside the liver much enlarged; so also those inside, but they are filled with a thick, almost colourless mucus." All that I suppose implies considerable suffering?—Not judging from what we see in men. You see men in the same state who complain of no pain during the whole of the illness.

5280. And do you mean that men die very much in that state and without any amount of suffering?—Without any amount of suffering expressed by the patient.

5281. Take this case at page 175.—"Experiment 16, June 27. Black and white cat, well nourished, full grown; bile duct tied double and piece cut out. July 3rd. As the cat was now very weak and seemed about to die, it was determined to make the diabetic puncture. The cat was therefore laid prone, a cut made through the skin over the occipital protuberance, and the chisel applied immediately underneath this." Now is that puncture a painful operation in itself, or was it performed under chloroform?—It was performed under chloroform, and I doubt whether it be painful, because as soon as it comes out of the chloroform it lies in a helpless state and does not move at all; nor does it give any signs of feeling.

5282. Then the cat lived for two days longer and you examined it to see the result. Now do you suppose that a new experiment, made on a cat already so diseased as that was, would yield a pure result, that is to say, a result not confused by the other causes acting upon it?—That is just what I desired to have, the experiment confused with the disease.

5283. You wished to see what the effect of diabetic puncture would be on a diseased animal as distinct from a healthy animal?—Yes, that is just what I wished to see.

5284. Do not the two results, that you appear to have established by these experiments, open a great many more problems than they solve? The one is the increase of the connective tissue in the liver of these animals, and the other is that glycogen is not produced after ligature of the bile duct. Those are the two main results, are they not?—Yes.

5285. Now do you not consider that they open a great many more problems than they close, and that they involve the further sacrifice of an indefinite number of cats in order to solve those problems?—That may be said of all increase of knowledge; I consider that these are two very important points to be established.

5286. I find in this manual of Dr. Burdon-Sanderson's, at least in regard of connective tissue, and I think also in regard of the other point, that both those results were regarded as established when the manual was published. Were they new to you when you performed them?—Yes. It is my custom before performing any experiment to see the state of knowledge on the subject. In fact before undertaking any research I make myself acquainted with everything that has been written on the subject: and I am quite unacquainted with any researches which establish points similar to those which I have established.

5287. As to the treatment of the cats, who was it that looked after the cats while they were under this treatment?—I looked after them myself, I did not feed them myself, though I used occasionally to do that; but I went to see them always every morning.

5288. There was an uncertainty it seems about the time when several of them died?—I did not go down on Sundays you must remember.

5289. And is that the only cause of uncertainty except in the case of the cat which escaped?—Yes, I believe that is the only cause of uncertainty.

5290. Then in your own opinion these experiments were all necessary—they were none too many?—I think not.

5291. And they were as mercifully performed as they possibly could be, you think?—I believe so.

5292. And that the results established were new and very important?—Yes.

5293. Have you been associated with Dr. Brunton in any other considerable series of inquiries?—No, I have never done any work with Dr. Brunton at all.

5294. This work was only performed in his room, not with him?—Not with him; he knew what I was going to do and the outcome of my experiments.

5295. What were the other series of experiments?—Some experiments upon the anatomical changes in fever which I published I think a year after.

5296. What was the nature of those experiments?—The object was to investigate the changes which occur in the internal organs when the temperature of the body is artificially raised. Fever is simply (we all know the old definition) *calor præter naturam*, and the experiment brought on a state quite like that of fever.

5297. You raised the temperature of certain animals until the point at which they died, I suppose?—No, I kept them for various numbers of hours, some six, some as much as nine; but the animals were all anæsthetised with chloral during the experiment.

5298. So that they were really not suffering at all?—Not at all.

5299. They were under chloral hydrate do you mean?—Yes.

5300. And were they killed?—They were killed at the end of the experiment.

5301. That was the second series, and that was on the temperature of the body, as I understand you. Would you mind telling us how many animals were used in that series?—Four I think in that series.

5302. Were they rabbits?—One was a rabbit, I think, and the other three were guinea pigs.

5303. Then can you tell us what the third series of experiments was?—Some experiments to investigate the action of a new substance, which was discovered by Dr. Matthiessen in his chemical laboratory. Dr. Matthiessen considered that this new substance might be useful as a remedy in medicine, and he asked me to investigate its action; and I investigated it upon dogs and upon myself.

5304. What was the drug?—It was the hydrochlorate of cotarnamic acid; it is a derivative from narcotine.

5305. Is it a narcotic?—No, it is not a narcotic. It turned out to be a very peculiar poison. My experiments were brought to an end by Dr. Matthiessen's sudden death.

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5306. Did you suffer from it much yourself?—It was a very disagreeable process, I assure you.

5307. What sort of result did it produce?—When injected under the skin it caused a very active inflammation; I injected some of it under the skin of my arm, and I had no sleep for two or three nights.

5308. Was the number of animals large, besides yourself, which you experimented upon in that series?—No; only three or four dogs, I think.

5309. Now that accounts for the third series; what was the fourth series?—On the inoculation of cancer. The animals were chloralised, and then a piece of newly cut-out tumour still warm was introduced under their skin; then the skin was drawn together with a ligature, and the animals allowed to live. In none of those cases was the inoculation successful. They lived three or four months, and at the end of that time they were killed, and there was nothing found.

5310. The inoculation produced no effect?—No. That experiment had never been performed before.

5311. Those experiments, where any serious suffering was caused, were all performed under chloral, as I understand, and the suffering from the wound after the recovery, I suppose, was immaterial?—It would not be very great.

5312. What was the last series?—Upon the cause of the appearance of bile pigments in the urine. Four animals were used in that experiment, and they were under chloroform at the time of the operation and during the experiment, and they were killed afterwards.

5313. So that really the only important series was this cat series; I mean that the only one in which you have used a considerable number of animals was this 16 cat series?—Yes.

5314. (*Sir J. B. Karslake.*) Why were the dogs in the fourth series killed; you had kept them, as I understand, for some time to see whether cancer was communicated?—You could not judge if it were communicated unless you saw the internal organs.

5315. Did I rightly understand you to say that you lectured at all at St. Bartholomew's Hospital?—I do not give regular lectures. It is my custom to demonstrate upon the results of the postmortem examinations.

5316. Do you use vivisection for that purpose occasionally?—Never.

5317. (*Mr. Erichsen.*) You demonstrate in the dead house?—Yes.

5318. (*Sir J. B. Karslake.*) Do you know anything about the regulations as to bringing in live animals at St. Bartholomew's, or the mode in which they are procured. Are there any regulations existing on the subject?—I know nothing of that; I never inquired into that point.

5319. You do not know of any printed or written regulations on the subject?—I never heard of any.

5320. (*Mr. Erichsen.*) Your experiments have chiefly been of a pathological or therapeutic character, rather than of a strictly scientific physiological nature?—Yes.

5321. They have all been conducted, I presume, with a direct bearing on the elucidation of some point in practice or in treatment; some point, at all events, connected with disease that may occur in the human subject?—Yes.

5322. Is it the case that those experiments, to which special attention has been directed, on the biliary secretion, were performed with a view of elucidating morbid conditions that do occur in the human subject?—Yes, certainly.

5323. There is a great difference of opinion, is there not, still amongst physiologists as to the various points connected with the biliary secretion?—Yes; and it seems to me that it is a most important subject in its present state to be worked at.

5324. We all know that diseases of the liver are amongst the most common that occur in the human subject?—And jaundice is certainly one of the most obscure that exists.

5325. And is it your opinion then that we may possibly arrive at some elucidation, or some true knowledge as to the real nature of diseases of the liver and of jaundice by making experiments upon the lower animals?—I have no doubt whatever of that. I think it is a most important means of research; because in the case of animals you may give them the disease and kill them at divers points in the progress of the disorder, which cannot be done with man. When a man dies, you find in his body nothing but the débris of the disease; you do not find the disease actively progressing as you do in animals which you have killed.

5326. I suppose we may take it that you find in the dead-house a condition of things incompatible with life?—Yes.

5327. But you do not find in the dead-house that condition of disease that is still compatible with life, and that is still in a curable state?—Just so.

5328. In order to discover a disease at that period of its existence when it is still amenable to treatment you must do something more than examine the body of a man who died from that disease, when it has passed beyond the reach of treatment?—Yes, that is precisely the case.

5329. I presume then we may take it that it is only by performing these pathological experiments, and, as you say, destroying animals at different periods of the disease, that you can arrive at the knowledge of what one may term the natural history of the disease, the progress of it *ab initio* to the time of fatal termination?—Yes.

5330. And did you perform these experiments with such an object as that in your mind, or were they performed at hap-hazard, or at random, without any very definite idea?—My object certainly was to make out the causes and nature of the disease which I gave to the animals. They were not performed at hap-hazard or at random.

5331. So far as you are in the habit of experimenting, do you, whenever practicable, employ anæsthetics in your experiments?—Invariably.

5332. So that you do not put the animal to any more pain than is absolutely necessary for the elucidation of the experiment; I mean in the progress of the diseased action?—Just so. Of course the first inoculation or communication of the disease to the animal can very often be done under anæsthetics and without the animal suffering any pain that we know of; but the further progress of the disease must be attended with whatever pain the disease may bring with it.

5333. With that condition of physical discomfort, pain or *malaise*, which the disease itself may entail?—Yes, I would like to observe that disease is the common inheritance of all of us, and that unless accident ends his days, every living creature must die of disease, and that disease itself is not of necessity painful.

5334. And you think that the same condition of things is found in animals that are artificially diseased?—Yes.

5335. We have been told that certain diseases induced in animals are productive of great pain to the animals, such for instance as the artificial production of tuberculosis. Now is it your opinion as a hospital physician that tubercle in man is a painful disease when existing in the internal organs?—It depends a great deal upon the organ which is affected; in the substance of the brain I think it is painless, and in the kidneys, spleen, and liver.

5336. I am speaking of the internal organs?—Yes, I should say quite painless there.

5337. And it is reasonable to suppose that it is the same in animals?—Yes.

5338. (*Sir J. B. Karslake.*) With reference to your answer that it is painless in the brain, are there not cases where a great deal of pain is caused by tubercle?—Yes, there are such cases, but tubercular meningitis is seen without sometimes.

5339. (*Mr. Hutton.*) I want to know whether it is not true that these surgically induced diseases offer rather doubtful analogies to the naturally induced diseases. What I mean is that this jaundice for in-

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stance, induced by a surgical operation, would be probably very different in its course and results from jaundice induced as it is in the human subject?—I do not see the least difference in the conditions of the two states. I think that a ligature put round the bile duct produces the same result as a tumour pressing on it.

5340. Do you not think that the pain inflicted in the operation would very much affect the general conditions in the patient?—I think not.

5341. You think that what jaundice means in the human patient is some ligature of the bile duct?—Yes.

5342. (*Chairman.*) You have told us that all your experiments have been performed under anæsthetics?—Yes.

5343. In short, we may take it that the animals have been treated with as much tenderness as the object of the experiment permitted?—Certainly.

5344. It is your belief that that is the habit of those who perform these experiments generally in this country?—I believe it.

5345. And that at any rate it ought to be?—It should be, and my belief is that it is their custom.

The witness withdrew.

Mr. Arthur Gamgee, M.D., called in and examined.

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5346. (*Chairman.*) I think you are a doctor of medicine, and a Fellow of the Royal Society?—I am.

5347. And the Brackenbury professor of physiology and histology in Owens College, Manchester?—I am.

5348. And examiner in physiology in the University of Edinburgh?—Yes.

5349. And examiner in forensic medicine in the University of London?—Yes.

5350. I think we may take it for granted that you are familiar with the subject referred to us for consideration?—I believe myself to be so.

5351. Do you consider experiments upon living animals to be necessary for the progress of science and the improved application of medical and surgical science to the human subject?—I do very distinctly think so.

5352. Are you prepared to illustrate that opinion?—I think it is not a difficult thing to illustrate the opinion. Perhaps the statements I am going to make are merely the expression of my own opinion, but it appears to me that there is no department of physiology, no even very limited branch of physiology, in which the most valuable facts have not been determined by experiments performed on living animals.

5353. Are those experiments performed at Owens College, Manchester?—They are.

5354. Are they performed with all the tenderness of which each case admits?—Certainly.

5355. Are they generally for original research, or are most of them for demonstration to the students?—If I might be allowed to do so, I shall give a short account of the mode in which the teaching of physiology is conducted in Owens College.

5356. Will you do so, if you please?—As professor of physiology in Owens College I deliver two courses of lectures. The first is a course of lectures on systematic physiology, and is intended for the instruction of junior or first year medical students. It is a course in which lectures are delivered every day, and these lectures are abundantly illustrated, but as a rule only by the aid of diagrams or models or microscopic preparations, though occasionally some experiments are performed on living animals; never, however, unless the animal has been rendered insensible by the use of anæsthetics. I deliver a second course of lectures, or I carry on a second course of instruction in practical or experimental physiology and histology. The lectures which I deliver in this course are intended for second-year students, and the course of instruction consists in lectures which are delivered twice weekly, and in demonstrations given in the laboratory. The demonstrations in the laboratory are confined, I may say, entirely to histology, and the students there individually examine the tissues of human and animal bodies. In my bi-weekly lectures in the lecture room I illustrate, generally by experiments upon living animals, all the great facts of physiology. I try as far as possible to make my students acquainted with every method of investigation which is pursued by the physiologist, always taking care that no pain is inflicted. In no case, I believe, since my appointment to the chair of physiology in Owens College, has any pain been inflicted for the purpose of demonstration in my class. As a general rule the animal is subjected to the action of anæsthetics, the experiment is then performed, and the animal is destroyed before it has had time to recover from its condition of insensibility.

5357. And I may take it to be your decided opinion, as shown by your practice, that all experiments upon living animals for the purpose of demonstration to students may be made without inflicting any pain upon the animals experimented on?—I believe without exception. There are certainly some experiments which might be instructive, but which entail a great amount of animal suffering, and I think they may therefore well be omitted from a course which is otherwise abundantly illustrated.

5358. Now, speaking of original research, most of the experiments that need be performed for the purpose of original research may be done while the animal is perfectly insensible, may they not?—The great majority, although there are, of course, numerous exceptions.

5359. Of those numerous exceptions another portion will consist, will it not, of experiments where the most painful part may be done upon the insensible animal, and only the watching afterwards be the subject of any pain to the animal?—Certainly. The experiments to which I chiefly referred as not admitting of the use of anæsthetics were experiments conducted to discover the action of drugs. In these experiments it would appear difficult often to employ an anæsthetic without complicating the action of the substance under investigation.

5360. But speaking first of that kind of experiment in which the action of the knife inflicts the pain, you agree in the tenor of the question?—Clearly.

5361. Coming then to another class, that where poison is to be tested, it is evidently impossible to do that without letting the animal suffer the pain of the poison. That, however, is limited to original research?—Yes, clearly. It is not the pain which is the object of investigation; but it is impossible, as a general rule, to give an anæsthetic in a case where one requires very carefully to study the action of poison on the various functions. There is undoubtedly a disturbing influence brought about by the action of anæsthetics.

5362. With regard to those latter experiments, those upon poison, are they generally very protracted?—They may be so.

5363. But not in the majority of cases, I suppose?—Not in the majority; and a large number of these experiments are not at all necessarily connected with the infliction of pain.

5364. Now there is a third class of experiments, that consisting of the inoculation of a disease for the purpose of watching the progress of that disease in the animal. I suppose there the inoculation may either be treated, as like vaccinating a child, too small a thing to be regarded as painful at all, or, if it amounts to anything like serious pain, you can perform it under anæsthetics?—Clearly; the mere inoculation in such experiments is but a very slight matter.

5365. The only pain that is inflicted upon the

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animal then is the consequent pain of suffering from the malady?—Yes.

5366. And of that we cannot, I suppose, ask you for any other definition than that it must be the inconvenience and discomfort of the malady, whatever that may be?—Yes.

5367. In those cases you do not permit the malady to run its whole course. The object of the experiments is to test it at various stages of its course, is it not?—Clearly; although one can well understand that where the result of the malady might be sought for, the experiment would be pursued to its end.

5368. That would be a rare case, would it not?—Decidedly a rare case.

5369. The shorter period would be generally the period at which the animal would be destroyed for the purpose of anatomy?—Generally.

5370. Do you at all suppose that it is the practice of any of your students to perform experiments upon animals in their own lodgings or in secret?—I would wish to express myself unhesitatingly on that matter. I believe it is not at all the custom. I believe that at the present time vivisection is only performed by very few persons in the kingdom; and, except in very rare instances, in the physiological laboratories attached to, unfortunately, very few public institutions.

5371. Then I may take it that you consider it to be performed by the most competent persons, with the most definite object, and in the most humane manner?—I do.

5372. (*Lord Winmarleigh.*) How long have you held your present office?—I have been professor of physiology in Owens College since the month of September 1873.

5373. Are there any rules connected with the study of physiology there which could restrain experiments by others than those especially devoted to that subject?—In virtue of my position I have the entire direction of the physiological laboratory of Owens College, and have absolute authority over all persons working in that laboratory; and I have certainly never permitted anyone to perform any experiments, except such as have been sanctioned and generally superintended by myself. No medical student, as such, has ever performed a physiological experiment. All the experiments for purposes of tuition are performed by myself, as a general rule after an introductory lecture in which I have stated what has been ascertained on the question, and the experiment has been performed for the purpose of illustrating my statements, the animal being in a state of anæsthesia and afterwards being destroyed. No student has ever, except for the purposes of research, (not as a medical student proper,) performed an experiment.

5374. Then you do not permit under any circumstances a student to operate even under your own direction in the laboratory?—Not unless the student is so competent that I should think him a person well fitted to perform an original investigation. Under these circumstances students have performed experiments, and I shall be most happy to inform the Commission exactly as to the details, the students whom I have entrusted with this power, and the results of their experiments.

5375. But any such experiments of that kind, conducted under your superintendence, would always be performed under anæsthetics and without cruelty?—Except in cases where the physiological action of a drug, or some other important fact is to be investigated, which makes the employment of anæsthetics impossible. Now it appears to me undoubted that such cases will arise in the future as they have arisen in the past. A highly competent student of Owens College who holds a physiological scholarship attached to my chair, Mr. John Priestley, has made an elaborate investigation into the compounds of a very rare metal, vanadium. This research was entirely planned by myself, and the experiments were first of all performed several times by myself; and I believe that every good teacher of physiology, before setting a student to any investigation, always takes that precaution. Mr. Priestley then with great care and humanity repeated the experiments which I considered it desirable should be performed, in order that the action of this poison should be known.

5376. Do you think it would be prejudicial to the scientific object of your department to introduce any regulations restricting experiments to persons properly qualified?—I am not prepared to answer that question. Perhaps the Commission will allow me to say this, that in reference to physiological laboratories I think there is no need for any legislation. I am sure from my pretty extensive experience that great humanity characterizes the practice of all who have the superintendence of such laboratories. I am, however, on principle, quite prepared to say that it might be advisable to restrain the possible performance of vivisection by persons not competent, and who are not working under the eye of a responsible person.

5377. Now could you suggest to the Commission any mode which would carry out the object which you have just suggested with the least possible detriment to the prosecution of science?—The question takes me rather aback, as I have not thought of it. As a rule I think that physiological science will not be advanced by private researches carried on by individuals with their own private resources. I would explain that statement; most physiological experiments are now performed with instruments of great delicacy, which require for their use long training, the training which as a rule can only be obtained in a physiological laboratory and under the guidance of an expert physiologist; few private individuals, except those who have really undergone this long training, could I am sure advance the science by private research.

5378. Could you draw any line, or could you suggest to this Commission any line which could be drawn, below which experiments should not be made? In other words, could you suggest any qualification on the part of a student or other person without which no experiment should be made?—It is extremely difficult to draw such a line. Speaking of students, I would say unhesitatingly that they should be entirely disqualified from performing any independent experiment. I have no hesitation in giving an opinion on that matter. On the other hand, there are a few (I am just now thinking of one) highly qualified physiologists not connected with phyiological laboratories, and possessed of such great skill that it would be a great hardship to rob them of the privilege of contributing to the branch of science which they have already adorned.

5379. (*Mr. Hutton.*) Would you mind mentioning the name?—The name which first suggests itself is that of Dr. Fraser, the Medical Officer of Health for Mid-Cheshire, a gentleman who has made researches on the action of many drugs.

5380. He however is a well-educated medical man?—Yes, a man of the highest culture.

5381. (*Lord Winmarleigh.*) Do you think that a system of licenses could be introduced which would not be prejudicial to science?—I can quite conceive of it, if no vexatious interference were exercised. If no vexatious spirit guided those who gave these licenses, I think they might be given with great advantage.

5382. But those licenses must be given with certain restrictions, of course; and could you suggest to us what those restrictions should be?—It is very difficult for me to give a decided answer and commit myself, as it were, to a decided opinion. I should be most happy at another time if the Commission will give me the opportunity, to express my view on the matter. I simply object to answer because I should not like to express a rash, and, it may be, an unwise opinion to the Commission.

5383. But you do think that a license might be given for experiments upon living animals which might protect the practice from abuse?—I believe so. I wish however very decidedly to express my conviction to the Commission that vivisection in England has not been practised so as to lead to any abuses. In fact, as I believe that it is very essential for the teaching of physiology and for the pursuit of physiological investigations, and as I believe that physiological investigations are of very great use to the whole human race, I think that vivisection has been practised almost too little.

5384. Are your lectures at Owens College attended by a numerous body of students?—My lectures to junior students are attended this session by 40 students. Those to second-year students by 42 students, I think. So that our medical school is now one of the most considerable in the country,—the largest provincial medical school.

5385. We all know that Owens College is increasing day by day, and most probably will become even more celebrated than it is at the present moment?—I may say that we are making great efforts in Owens College to encourage qualified persons to engage in physiological research; and for that purpose we have provided a laboratory, and we have a physiological scholarship which was endowed by Mr. Platt, for the purpose of encouraging original physiological investigation; it is a condition required of the holder of that scholarship that he shall have passed an examination in physiology and comparative anatomy, and it may be in some other subjects, and that he shall have carried out an original investigation in physiology. As yet we have only elected one Platt physiological scholar, Mr. John Priestley; and the investigation which he performed, which I just now referred to, is one on vanadium, which is about to be read before the Royal Society.

5386. (*Sir J. B. Karslake.*) I think I infer from what you have said that in your judgment it is essential for the proper tuition of the students under your care that you should exhibit from time to time experiments on living animals?—Certainly.

5387. In your belief could they follow or appreciate your teaching efficiently without having those experiments performed?—I am convinced that they could not.

5388. You have given the name of one gentleman who as a student has engaged in physiological researches under your supervision in your college, Mr. Priestley?—Yes.

5389. All his experiments, I understand, were first of all suggested and practised by yourself?—Yes.

5390. And afterwards he again went through the experiments with a view to qualify himself for this scholarship, is that so?—I am sorry that I should have conveyed such a very erroneous impression to the Commission, and if you will allow me I will correct that impression. In carrying out any investigation in any branch of science it is absolutely essential that observations should be numerous. That is really true of observations in any department of physical science; no single observation, except under the most rare circumstances, is trusted by competent scientific men who exercise due caution. Now physiological experiments are more liable to fallacies than any other experiments; there are so many circumstances which belong to the particular animal which is being experimented on which may lead to a fallacy in judgment, that it is most essential that many observations should be made. In carrying out this research I followed the plan which I believe is followed by all the conscientious physiologists of Europe in the directing of investigations. I selected in the first place a man whom I considered qualified by his previous training to undertake such a research. I caused him to make the previous dissections required for the intelligent performance of the experiments, and I then showed him the way of doing a particular experiment. For example, in taking up a particular kind of investigation, after I had thoroughly instructed him he repeated that experiment, or that kind of experiment, not by any means to *qualify for the scholarship*, but in order to acquire for science the facts which I wished acquired. These facts could not have been acquired by a single experiment, often not by five or six, sometimes not by 10 or 12 experiments. I wish very distinctly that it should be understood that in no case would I be guilty of such conduct as to have an investigation performed *in order that* a person might qualify for the scholarship. If the conditions of the scholarship were such as to involve that risk, I should at once go to the founder and ask him to consent to a modification of the conditions under which the scholarship was given.

5391. Might I ask you this: Did not Mr. Priestley perform experiments, with a view to ascertaining results, which had not been performed by yourself?—Yes, clearly. I only indicated the method to be pursued; I could not say the result that would follow.

5392. So that he, in your laboratory, and under your supervision, did perform experiments and make observations which you yourself had not performed or made?—There is a certain amount of truth in that way of putting it, but it is not entirely correct. I had made the experiments, and, in fact, in a few cases were the experiment was one of great complexity I performed it every time. There was one set of experiments which I thought so difficult that it would require practising upon animals for a considerable time to give Mr. Priestley the due facility, and therefore I performed that part of the research myself, he helping me. I was the operator. I do not say that I was the observer. Mr. Priestley was perhaps more the observer than I was.

5393. Mr. Priestley's name has been introduced by you as that of a gentleman who has been in one of your classes of students in Owens College, and whom you considered a person quite efficient to be intrusted with physiological research and making experiments, at all events under your care, is that so?—Certainly.

5394. Now with regard to what you said about licensing, and the performance generally speaking of these experiments in physiological laboratories, what would be the objection at the present time to Mr. Priestley making independent researches in his own private laboratory?—I think that those who read Mr. Priestley's paper, which is yet unpublished, will say after reading it that he is exactly one of those persons who might very safely be entrusted with a license.

5395. And I suppose you would hope that other persons like Mr. Priestley will be turned out of Owens College?—Yes, although I confess that I am not sanguine that we shall have a great number of young men who will, as Mr. Priestley has done, devote themselves for years to the pursuit of physiology.

5396. Still the thing is possible?—Yes, it is possible.

5396*a*. Have you at all considered the question, supposing a license were given, who is to suggest the name of the person who should be licensed, who is to say on what terms he is to be licensed, whether he is to be licensed to carry on experiments in any particular place, and any other questions which are involved in the licensing question?—That forms rather a part of a previous question which I declined to answer. If I had had a very short time to think over it I might have given a consistent answer. I should scarcely like to commit myself now on so important a matter.

5397. (*Mr. Hutton.*) Did I not gather that you do not regard it as necessary to exhibit the action of painful poisons in your ordinary class as a demonstrational experiment?—Certainly not at all important. I have never done such a thing.

5398. You do not exhibit for instance the action of strychnine on an animal?—I have never done so.

5399. And you do not think it necessary to do so?—I would not like to condemn another physiologist who thought it advisable.

5400. Still you do not in your own opinion regard it as advisable?—I do not in my own opinion; still if I were giving a special course of lectures on the action of poisons to young medical men, I would not think it wrong to perform such experiments. I would think that the end to be gained justified them.

5401. Do you find it necessary to perform experi-

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ments on arterial pressure in your own class ?—Yes, often.

5402. Are they all under chloroform ?—All under chloroform or chloral, or the combined influence of chloroform and morphia.

5403. You do not regard curari as an anæsthetic? —I have never used curari in a class experiment. Certainly in the early stages of its action curari is not an anæsthetic. I think that undoubted ; there are a number of facts known to me which lead me to say so. There are other facts, however, which lead one to think it probable that as the result of the administration of large doses after a certain stage is passed sensation is affected.

5404. Has Mr. Priestley got a medical degree ?—He has not yet got a medical degree.

5405. Would you think it very inconvenient if such a qualification as this should be required, that any man operating for himself, and not operating under the inspection of a physiologist like yourself, should have a medical degree, should have passed through a physiological laboratory, and should have got the certificate of the physiological lecturer that he was qualified to perform experiments and was likely to perform them with all possible humanity ?—I think it would be most inconvenient to require the possession of a medical degree, because physiology is now commencing to be studied as a special branch of science for its own sake. For example, in Cambridge, many are, I believe, pursuing its study for its own sake; and I have no doubt that the study will be in a little time generally pursued. In Mr. Priestley's case, I think it would be a very great hardship if a medical degree were required from him ; because he has qualified himself by devoting himself for two years to the study of physiology. In qualifying for this Platt scholarship he had to subject himself to a most stringent examination in physiology and comparative anatomy; and I should consider that if such a restiction were imposed upon such a man it would be a very vexatious one.

5406. (*Lord Winmarleigh.*) Have any experiments been made on the human frame of the effects of curari ? —Yes.

5407. What was the nature of them ?—I performed some experiments myself on the action of curari. It had been stated by a German physician, whose name I do not remember just now, that curari in very small doses was a serviceable remedy in the treatment of some nervous affections, more especially of epilepsy; and being connected with the Children's Hospital at Manchester, I commenced some very careful experiments with this drug in the case of children who were almost hopelessly epileptic, carrying on the administration of the drug with very great care, first giving very small doses, and then increasing them very gradually. At first no result whatever followed; and as the disease was not in the least affected, it appeared desirable to produce some physiological effect to show that curari was being given in such doses as to affect the system, and in these cases some very interesting phenomena were produced. Naturally I only induced the very earliest effects of curari in the human subject, knowing by my experience on animals that to give the poison in excessive doses would have been to imperil the lives of my patients; but in two cases where a certain dose had previously produced no effect, so that I fancied I was within perfectly safe limits, a repetition of the dose produced some slight and very transient phenomena of paralysis. One little girl, for example, who was a peculiarly stolid and unexcitable child became obviously frightened; a slight paralysis of the muscles of expression occurred; she complained that she could not raise her arms, and on rising to walk she fell. I was an eye-witness of the symptoms produced. We removed her to a bed ; the circulation went on quite satisfactorily, the respiration becoming almost anxiously shallow. The symptoms very rapidly passed off. Now in this little patient I was able to determine very decidedly that sensibility was not at all impaired ; although there was a certain amount of paralysis of motion produced by the curari there was no affection of the sensory nerves. And this observation I was able to confirm by others upon another little patient. After the performance of these two experiments it appeared to me that I had evidence, firstly, that the poison did not in any way control the disease, and secondly that even in small doses it was so dangerous as to render it inexpedient to pursue the observations. I think it right to add that the observations were carried on with extreme care, and with every appliance ready in the event of respiration becoming arrested.

5408. Were any of those cases attended with pain at all ?—No.

5409. You have not made any experiment of the sort in a case where the patient was suffering pain, and could tell you the effect of the curari afterwards ? —No. I believe very few experiments have been performed in which the administration of the drug was pushed as far as in the cases to which I have been referring. I had the opportunity of being for some time this summer with the most distinguished French physiologist, Professor Claude Bernard, to whom we owe the first accurate investigation of the action of curari, and he was extremely interested when I described to him my observations, and he thought them almost singular from the extent to which the poison had been pushed. Indeed, I promised to send him the observations, so that he might publish them in any future paper which he writes on that subject.

5410. M. Claude Bernard did not tell you of any experiments which he had himself made with curari on the human frame ?—No; he had distinctly not performed any. I believe he told me that experiments had been performed with it at La Salpêtrière in Paris, but that the doses had not been pushed to the same extent as in the cases which I described.

5411. It is your experience that curari does not diminish pain ; is that so ?—I would not go so far as that. There are some facts which would almost show that at a later stage the spinal cord and the brain are affected by it. It is, however, conceivable, I should say, that an anæsthetic action is not an essential result of the introduction of curari. I think it is probable that curari may paralyse the motion of the body without affecting the sensory nerves or the nerve centres of the body.

5412. (*Chairman.*) But with reference to the more immediate subject of our inquiry, you are of opinion that in inflicting operations in themselves very painful upon living animals, curari ought not to be trusted as taking away sensibility to pain ?—I should think not. I would add this: I do not believe that physiologists use it for that purpose. It is used in order to eliminate a series of fallacies which obtrude themselves in physiological experiments; and it is conceivable that curari might be used (in fact I have no doubt it might be), if required, even in connection with anæsthetics. Many of the experiments in which curari is administered are not very painful experiments. It is not given to prevent the cries or the movements of the animal, but as I have said to eliminate certain disturbing influences.

5413. But if the object was to use an anæsthetic to relieve the animal from pain, it would not be right to trust to the operation of this particular drug ?—It is my individual opinion that it would not.

5414. (*Mr. Hutton.*) May I ask whether your answer as to the absolute anæsthesia of the animals experimented upon in your lectures applies to frogs also, or only to the higher animals ?—It applies to all. In experiments on frogs, however, I have very rarely administered or used anæsthetics, but I shall explain to the Commission why. These experiments have been performed when the animals have not been capable of suffering. In the immense majority of cases experiments on frogs may be performed after destruction of the nerve centres, after decapitation and, if need be, after destruction of the spinal cord. Now it is obvious that decapitation or destruction of the

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nerve centres may be performed with such rapidity as to cause very little pain indeed. When decapitation is practised, I always crush the brain, and I am in the habit of pointing out to the students that I take that precaution for certain obvious reasons. It might be that in a cold-blooded animal the structures still in connexion with the brain might retain their sensibility even after a great hæmorrhage. There are innumerable facts which make it almost certain that if a person were decapitated the head would not feel. Now it is conceivable that things may be different in cold-blooded animals. Their tissues are capable of living for a longer time when deprived of blood; and therefore I have always been in the habit, after decapitating a frog, of at once crushing the head.

5415. (*Lord Winmarleigh.*) Are there many experiments on frogs in which you cannot relieve them from pain in the way which you have just described?—I think there are very few. I should like to point out this fact, which is known to all physiologists, that in cold-blooded animals the life of the tissues persists for a long time after the life of the body has ceased; death of the tissues in no way corresponding in point of time with the death of the whole body. But anæsthetics might be used; for example, placing the frogs in a watery solution of chloroform is an admirable method of rendering them very rapidly insensible to pain; they remain insensible for a long time if placed in water which contains very little chloroform.

5416. (*Mr. Hutton.*) Have you studied much abroad?—Not for a long time; but to a certain extent both in the University of Leipsic, in the University of Heidelberg and in Paris.

5417. Has it occurred to you to know much of the foreign physiologists, I mean as distinguished from medical men?—Yes, I know several of them pretty intimately.

5418. Do you not think that a good many of them perform experiments which the English pysiologists would mostly regard as needless and even wanton?—I confess to you I do not believe it. I do not know one such. I will say all I know on the subject. If by "wanton" you mean experiments which are not absolutely required for the purpose of scientific discovery, then, perhaps, the expression is true. For example, I was referring a short while ago to a late visit to Paris, and to my interviews with Professor Claude Bernard. On that occasion Professor Claude Bernard did perform certain experiments simply for my instruction; but these experiments were by no means wanton experiments. I was very anxious to know how to perform certain experiments which he had been the first to devise, and he performed these; and amongst others he was particularly anxious to show me the effects of certain anæsthetics, or certain modes of administering anæsthetics, which made it possible to perform almost any experiment painlessly on animals. I have referred to Professor Claude Bernard; I might refer to Professor Ludwig of Leipsic, as a man who, I am certain, is as cautious in the performance of any experiment on a living animal as any English physiologist that ever lived, and who yet has been the teacher of nearly all the physiologists of Europe, and has indoctrinated nearly the whole of them in the methods of physiological inquiry. I think in the several laboratories of Europe, with which I am acquainted, anæsthetics are made use of, except in cases where their use would distinctly interfere with the investigation.

5419. (*Sir J. B. Karslake.*) I infer from what you said just now that Claude Bernard seems to have paid particular attention to anæsthetics and the proper mode of administering them?—He has. He has very lately published a book on the physiological action of anæsthetics, a book of great interest and of great use to persons who have to perform experiments on living animals, so admirable are the directions for properly anæsthetising animals.

5420. (*Chairman.*) You hope to see the day when laboratories like that of Owens College will be multiplied in this country?—I do.

5421. And you regard them as affording means of making physiological investigations which cannot be afforded elsewhere?—Undoubtedly.

5422. Therefore where such investigations are performed by private persons and in private places, you regard them as exceptional cases?—Yes.

5423. And you regard those exceptional cases as likely not to increase but to diminish in proportion as what you wish to see increase?—Yes, very decidedly.

5424. And although you have not considered the detail of the subject, yet having such an increase of great schools in view you see no objection but quite the reverse to the establishment of some reasonable regulations for such institutions?—I see no objection to it.

5425. I need scarcely ask you whether if some regulation were proposed on the subject which should, without limiting the progress of science and the efforts of competent scientific people, restrain the performance of operations upon living animals by incompetent persons, for no definite object and without proper precautions, there would be any sentiment on your part that you had been affronted by such a regulation?—Not at all.

5426. Nor would you at all sympathise with anybody else who thought it would be an affront to the promoters of the advance of science in this country?—Certainly not.

5427. All therefore would depend upon the reasonableness of the measures and the question of whether they really tended to interfere with the progress of science?—Clearly.

The witness withdrew.

Mr. GOERGE JAMES ALLMAN, M.D., called in and examined.

Mr. G. J. Allman, M.D.

5428. (*Chairman.*) Are you Emeritus Professor of Natural History in the University of Edinburgh?—Yes.

5429. You have paid, I think, a great deal of attention to the subject which has been referred to us?—A great deal in a general way, but not specially; and I should be rather sorry, considering that I have not applied myself specially to physiological research, if the Commission should be disposed to place more value on my evidence than I do myself.

5430. Is it your opinion that the practice of trying experiments upon living animals should be altogether put an end to?—No, certainly not.

5431. You consider that those experiments are necessary for the progress of science?—I do.

5432. Do you think that it is possible to carry on those experiments in such a manner as to advance science and its application to the alleviation of suffering in the human family, while at the same time taking measures to exclude any abuse of the system?—I think it is quite possible.

5433. Now have you thought at all of what limits you would be prepared to recommend?—I should certainly recommend that no experiments on living animals should be performed without throwing the animal first into a state of insensibility by some means or another, except in cases where the state of insensibility might interfere with the value of the results.

5434. Is it your opinion that in the majority of experiments upon living animals for the purpose of observing the healthy process it is possible to perform those experiments while the animal is under complete anæsthesia?—I think it is quite possible.

5435. We have been told that in the majority of other cases, though the whole of the proceedings cannot be conducted under anæsthetics, the most painful part of them can be, is that your opinion?—Unless

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an actual case was put before me I should hardly be prepared to give an answer to that question.

5436. We have been told that there are important experiments tried to test particular modes, for instance, the tying arteries, and that while the original operation is painful the process of recovery in the human person is not very painful, and it is contended that it is quite legitimate to operate upon an animal under chloroform and then to observe the process of recovery, without of course its being possible to continue the anæsthetic operation ?—I think it is quite possible to perform the operation under the influence of chloroform.

5437. And would you say that a case like that was a case which might be permitted ?—Yes, but you could not continue the animal under the influence of chloroform so as to observe the subsequent progress of the case.

5438. That I assume. The question I put to you is whether such an experiment falls within your notion of being a right and proper experiment ?—Yes.

5439. Have you considered the bill which was laid before the House of Commons last session by Dr. Lyon Playfair ?—Yes, I have read it.

5440. And do you consider that in general it is a suitable bill ;—I do ; upon the whole I think the bill a good one. I think there are some points in which it might be improved, but I think it very much better than the bill brought in by Lord Hartismere.

5441. Do you attach importance to the keeping of a register of all the proceedings in regard to living animals ?—I do.

5442. In which everything that takes place should be recorded with accuracy?—All cases at least which are performed without the application of anæsthetics.

5443. Do you think that for the purpose of education it is at all necessary to exhibit experiments which are not performed under anæsthetics ?—Certainly not ; I think there is no necessity for such.

5444. Where a doctrine is already established, and the experiment, if performed, must necessarily give considerable pain, do you think it is legitimate to repeat it ? —It depends there very much upon what we regard as a doctrine being absolutely established. There are many cases I should say in which the evidence brought forward for the truth of certain doctrines stated may not bring conviction to a physiologist's mind, and I think under such circumstances he would be justified in repeating the experiment.

5445. It depends then upon the fact of whether the doctrine is firmly established or not ?—Yes.

5446. But where a doctrine is firmly established, and the experiment is necessarily very painful, take for instance an experiment upon the roots of the nerves, to repeat that which Sir Charles Bell firmly established, do you consider that legitimate ?—I do not.

5447. Are there any other observations which you desire to offer to the Commission ?—I think not.

5448. (*Sir J. B. Karslake.*) With regard to one answer that you gave, let me put this question : You yourself have never used experiments upon live animals for exhibition to a class ?—Not for exhibition to a class. In some investigations of my own I have tried experiments upon frogs, but nothing more.

5449. With reference to the actions of poisons we have been told that in certain classes the action of strychnia upon an animal has been exhibited, because it is suggested that students cannot obtain accurate knowledge of the symptoms attending poison by strychnia without seeing a case. Would you say that persons expressing that opinion are incorrect ?—I should be inclined to think so. I should not like, however, to speak very positively, as it is not a subject on which I have had any personal experience ; but I should think that descriptions of the tetanic results of strychnia are so plain and intelligible that it does not need the thing to be actually exhibited to the students in order to give them a correct idea of it.

5450. That would be your opinion ?—It would be my opinion.

5451. But you say that you do not express any very strong opinion upon it, not having had any experience in exhibiting to a class yourself ?—No ; I merely give that as my opinion, for what it is worth.

5452. (*Mr. Hutton.*) You seem to say that the register of vivisections should be limited to experiments made without anæsthetics. Now do you not think that there is enough doubt about the completeness of the anæsthesia to make it desirable that the register should include all experiments on living animals, with a specification whether they were under anæsthesia or not?—Of course that is a practical point for consideration. If we were sure of the experiments being performed under anæsthesia, then I should say there was no necessity for recording them.

5453. But one cannot be sure ; it is so easy to give anæsthetics for the first few minutes and to let the effect of the anæsthetic pass off before the pain passes off ?—If the effect of the anæsthetic passes off before the pain passes off, I think the animal should be destroyed, or, if possible, thrown under a state of anæsthesia again immediately.

5454. Do not you think, therefore, that it would be better to include in the register all experiments, whether under anæsthesia or not ?—It would probably be the simplest way after all.

5455. And as to the killing of animals immediately after the experiment, I suppose you would enforce that in any case where they had been seriously injured ?—I would.

5456. Unless it were absolutely necessary for the result of the experiment to watch the progress in the living animal ?—Yes, with that exception, provided the injury was likely to produce prolonged pain to the animal.

5457. (*Chairman.*) Have you anything further you wish to say ?—No, I have not ; except that I should almost feel inclined to suggest the appointment of a Board of Control. I think that if physiological experiments are tried in licensed places, inspection should be allowed, and that a board should be appointed with a power of visiting and seeing that there was no abuse. I should think that all difficulty would be got over by such a plan as that.

The witness withdrew.

Adjourned to Saturday next, at 12 o'clock.

Saturday, November 6th, 1875.

PRESENT :

THE RIGHT HON. VISCOUNT CARDWELL, IN THE CHAIR.

Sir J. B. KARSLAKE, M.P.
THOMAS HENRY HUXLEY, Esq.
JOHN ERIC ERICHSEN, Esq.
RICHARD HOLT HUTTON, Esq.
N. BAKER, Esq., Secretary.

Sir WILLIAM WITHEY GULL, Bart., M.D., called in and examined.

5458. (*Chairman.*) Were you lately teacher of physiology at Guy's Hospital, and also teacher of medicine there ?—Yes.

5459. And are you now consulting physician there? —Yes.

5460. And were formerly Fullerian professor of physiology at the Royal Institution ?—Yes.

5461. I need hardly ask you if you are a physician in extensive practice in London ?—Yes, I have seen a good deal of disease.

5462. You have paid, I believe, a great deal of attention to the subject which has been referred to us? —I believe I have unusual attention.

5463. Would you have the kindness to state your views upon it ?—First I should like to say that I have inquired whence the movement sprang—what, so far as I could make out, were the facts which have excited public attention, and so far as I can learn I know of no facts in England that would have excited such attention, but so far as I can learn (but this I shall only say as hearsay) there have been reports of experiments on animals abroad which have excited a sentiment against the practice of vivisection. But I should like very much to be guarded in that expression ; because the persons who have performed those experiments are not here to answer for themselves ; and, besides, I believe it to be so contrary to the spirit of scientific inquiry to give any unnecessary pain, or in any way to give licence to a cruel spirit or to an indifferent spirit even, that I should be very slow without exact evidence to believe those reports. I should also be more slow to believe them, because I know that those who have reported them are not people in whom I should have confidence. I believe that the public feeling has been excited by reading a recital of experiments asserted to have been made, which, if made at all, were not made in this country, but which the public have interpreted as having occurred here.

5464. Are you of opinion that experiments upon living animals are necessary for the progress of surgical and medical science ?—I am certainly so.

5465. Are you of opinion that the tone of sentiment in the minds of those who practise such experiments in this country is a sentiment of indifference or a sentiment of humanity ?—I should say essentially a sentiment of humanity.

5466. Is it the case that the great majority of such experiments may be performed when the animal is entirely under the influence of anæsthetics ?—Certainly, and they are so.

5467. That of the remainder a majority may be performed in such a way that the most painful part is done under the influence of anæsthetics ?—Quite so. I think there are but few experiments performed on living creatures where sensation is not first removed ; I think the removal of sensation is necessary to the success of an experiment very often or indeed most often.

5468. Is it your opinion that the students as well as the professors are impressed with a sentiment of humanity, and would be intolerant of the infliction of pain which they believed to be unnecessary ?—I believe that in a medical school anything like cruelty or indifference to suffering would be scouted by the public opinion of the students. I have never seen any indifference.

5469. Is it your opinion that of that small remaining portion of experiments, those where anæsthesia cannot be induced without destroying the success of the experiment, there are many in which anything like protracted agony need be, or is, inflicted ?—I personally know of none where suffering forms any necessary part. In all the experiments on living creatures, so far as the mere suffering goes it complicates and perverts the results.

5470. Take for instance experiments where sensation is in question, those I presume must necessarily be painful ?—Yes, there are very few of such ; there are hardly any that I could at the moment recall. Of course science must have its own progress and I cannot forecast what line it may take ; but there are very few experiments on nerves of sensation.

5471. Then we may assume that if experiments on the nerves of sensation were repeated for the mere purpose of examining into facts which have already been established, like those established by Sir Charles Bell, you think that such repetition would excite the reprobation of the teacher and the pupils, as well as of the public in this country ?—I quite think it would.

5472. Have you directed your attention to the question of whether any legislative measures may be introduced upon the subject without interfering with the progress of physiological science, and if any such measures can be introduced whether there is any objection to them ?—If legislation were calculated to hamper either scientific enquiry or the pursuit of science I should think it most deplorable.

5473. But if it were clear of that objection then you would see no difficulty about it, would you ?—Even then I should see a moral and very serious objection. It would imply that the scientific men of this country and the students of science required legislation, which I deny.

5474. But supposing that the object of the legislation were not to impede the progress of science in the hands of competent and scientific and studious persons, but to prevent vivisection being attempted by incompetent and uneducated and unscientific people, what would be your opinion of such legislation ?—Then I should think it superfluous, because I know no such people who do it ; so that I should be on the horns of a dilemma.

5475. But if it were established to your satisfaction that such things were done, would you then see an objection to legislation which should repress the practice in those particular cases ?—I think the particular cases in which it would need repression would be cases of cruelty to animals which would be dealt with by the common law. I have thought a good deal on the question how far any possible legislation would not throw a shadow upon the fair prospects of science, and I feel this, that as all legislation must hamper and must hinder, which all legislation is intended for, because it is intended to restrict, such restriction where it was necessary might fairly, and might I am sure, as regards public morals and public decency, be left to the high sense of the teachers or the students of science, and the public ; that whatever legislation could do would be repressive, and would be a distinct assumption that there was a public scandal in a certain way, limited more or less, but still a public scandal calling for legislation ; because legislation could only be called for if there were such a scandal, and the prevalence of it were sufficient for legis-

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lation. Now I must say that with my personal knowledge and long experience, both in public and private life, from living at a hospital for fifteen years, being acquainted with one of the greatest schools in this country, having been occupied with physiology for fourteen years of my life, and having a full acquaintance with medical men and medical students, and with the difficulties that science has to contend against, and looking at the whole subject, I should regard any legislation as repressive or hampering, or as casting a shadow where it is not deserved. There may be isolated cases of abuse, but I doubt even that; I doubt whether anybody but a madman would seek to inflict pain for the pleasure of it; I do not know of such cases. I should regard it as an evil day for this country if the Parliament of England should think itself called upon to interfere.

5476. You would think it implied that there were abuses in the country which you did not believe to exist?—Yes, that is what I should think. I should regard it as throwing a shadow, as I have said, upon our fair fame, which fair fame should not, I think, receive such a blot. Moreover, I think there is such a tendency to ignorance; there is such a tendency to be satisfied with the knowledge we now have; there is such a desire to be governed by what I should call weak sentiment—I do not speak unfairly or unkindly, but there is such a desire that I am sure that even the mildest legislation would do harm to the progress of science; not to the progress of science in a higher sense, because I think it would not hinder the scientific men of this country from pursuing their inquiries; but I think it would prevent the spread of scientific knowledge amongst students who are already too glad to get their knowledge out of books.

5477. By that do you mean that students ought to be encouraged to perform these experiments?—No, but I think they ought to see them.

5478. But supposing every opportunity were afforded for students to see the experiments, do you then think that to repress any tendency to experiment either in an indifferent and reckless manner, or to conduct experiments through the means of uninstructed and incompetent people, would be a slight upon those who were highly competent and well instructed, and with whom the law did not at all interfere?—It would assume that there were such people, which I doubt.

5479. That is the foundation of your objection?—That is the objection that I have.

5480. If it were shown that there was any sufficient cause for it would you then object to legislation?—Then I think you must legislate; then science is gone in this country, and public morals. I really think that. If there is such an area of indifferent, ignorant, and cruel experiments on living animals as would warrant legislation, then I seriously think that we are in a state of great decadence; but, as I believe that does not exist, therefore it is really for the fair fame of our country that I am pleading. I think it would be a grievous thing that we should be put into a false position in this matter.

5481. In many countries there do not exist, I believe, laws for the prevention of ordinary cruelty to animals. In England there is such a law. Do you think that the character of England for humanity has suffered in comparison with those other countries by the enactment and continuance of that law?—In the first place I do not know of the law; I only know of the action of the Society for the Prevention of Cruelty to Animals; and if I were asked I should say that that Society has not done great service, or no more than would have resulted without it.

5482. I do not understand you to think that England has suffered in reputation from the existence of that law?—No, I think not. I ought to add, however, that those laws would be made for the ignorant, and not for the best people in the country. I mean to say that there might be very different legislation for the ignorant and the indifferent, as compared with the pursuers of science and the students of science.

5483. Then you assume that the question is whether the law shall operate upon that highly elevated and excellent portion of the community who really would act humanely without any law?—I think so.

5484. But if the reason in this case should be the usual reason for a repressive law, that it shall operate upon a totally different class of persons, what would you say?—Then I should see less objection. I will put it thus: I can understand that a father would have an Index Expurgatorius for his children, but I should think it a very serious thing if he had it through the whole of their life. That is how I should think of it.

5485. Then in short it is that you feel a difficulty lest it should be understood that the highly estimable and highly instructed people, with whom you have been principally acquainted in connexion with this question are the persons pointed at by the Statute?—I should feel this difficulty.

5486. But if that were perfectly clear your objection would comparatively vanish, would it not?—Well, not altogether; because I should be troubled with the assumption that there was still a prevalence of scandal, which I doubt. But of course if the Commission or Parliament is satisfied that there is such improper practice they must legislate; I however doubt the existence of it.

5487. I suppose it consists with your knowledge and experience that the disposition to study physiological science by the use of experiments upon living animals has by no means attained its maximum in this country, but that on the contrary it is now making rapid progress?—Even that I could not say. I do not think that is so. What I think is making progress, and what I hope will spread, is that men should acquaint themselves more with the actual facts of life as far as is becoming. I do not know that scientific inquiry into living phenomena upon animals by the highest teachers of our race is more going on now than it was in the time of Harvey; I should doubt it. I may tell the Commission (but most of the members of it probably know the facts better than I do) that there was a time when there was a great deal more vivisection than now; for instance, when the circulation of the blood was discovered there was a very curious excitement about vivisection; with regard to the transfusion of blood the whole thing came as a most strange revelation upon the public. Many vivisections were then practised.

I think that the desire of all good teachers to bring their students into relation, as far as it is becoming, with living phenomena is increasing, and I hope it will increase; and I think that accounts for physiological laboratories; but I think that the public should know that that is quite apart from torture or cruelty.

5488. (*Sir J. B. Karslake.*) I think I have already inferred from what you say that you think it essential to the due teaching of students that they should witness these experiments on living animals to some extent?—To some extent, or to any such extent as the state of our knowledge or the state of science may make desirable. I should not like to limit it by saying to what extent, because to-morrow there may be a different idea from to-day; I cannot say what the limit should be.

5489. Now supposing it were to come to your knowledge that there were in this country persons largely engaged in physiological research and pursuits who avowed that they thought they might perform operations on living animals without taking the trouble of administering anæsthetics, except for their own convenience, would that alter your judgment at all as to the expediency of legislation?—Well, it would not. I should say, though I should not agree with such individuals, and though I should think it a thing, so far as I was concerned, to deprecate, if a man who had more knowledge than I have, or more love of science than I may possess, should think it desirable in the high purposes of his mind to perform a given

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experiment, I should certainly not think that the outsiders should have any right to rush upon him and say "You shall not." I am supposing a man who in his high purposes has a distinct experiment which he thinks should be performed in the interests of humanity; and I am supposing that that experiment might imply a great deal of suffering. I should not think it desirable to hinder him, though I might say to him, "Could not that be done differently," or might express my opinion about it, still I should think it my duty to defer to his intentions, believing that he would not, and that no student of science would, as a student of science, do that which was not worthy of him. I will put it thus, and answer the question in another way: Would it be considered right to take an unwilling man and an innocent person, and slaughter him for the public safety? Yes, decidedly; it is done every day in war. You take an unwilling man, and you slaughter him for the public safety. I see no reason in the nature of the thing why an animal should not be slaughtered for the public good, for the teaching of science, if it should so seem fit to a man in whose intention I had confidence. If he were a madman I think he should be brought under the law regarding insanity. But take a man in the pursuit of science—I will take, for instance, Harvey—and I will suppose that another Harvey should come; if he should say, "I see my way to a certain inquiry, and I am going to do it," I think you could not, or, at least, should not hinder him.

5490. I think I did not make myself quite understood. I am supposing a case where a person avowed that he could perform the operation which he desired to perform after having put the animal into a state of anæsthesia, but that he says, "I consider that I am " at perfect liberty, without reference to the animal's " feelings, to perform the operation I desire to per- " form, and which I might perform under the influence " of anæsthetics, without resorting to them at all"? —I think such a man should be blamed, and is open to public censure.

5491. (*Chairman.*) Do not you think he should be repressed?—He may have said something of that sort out of bravado; but as a matter of fact his colleagues would not allow him to act in that manner; they would say, "Do no such a wanton thing as that"; I feel sure of this, if I know humanity at all.

5492. You think that the moral control of his acquaintances ought to be brought to bear upon him?—Certainly; and it would be.

5493. And you would not hesitate, perhaps, to use words which we have heard from a very eminent authority, that indifference to the sufferings of animals is worthy of all detestation and abhorrence?—Certainly; animal suffering is as much to be considered, ceteris paribus, as human suffering. Certainly, an animal should not be wantonly put to suffering.

5494. (*Mr. Huxley.*) I understand that you object to legislative interference with scientific men on the ground that there is no evidence of the existence of any abuses in this country which could justify such interference?—Quite so.

5495. There is another consideration which has been brought before us, and which perhaps has led scientific men to feel more strongly than they otherwise might do upon the question; they have a feeling that there is not only a want of necessity for such legislation, but that if such legislation takes place it will be the worst kind of legislation, unequal legislation, inasmuch as it will propose to inflict restraints, or perhaps pains and penalties upon them for doing certain things, while the other classes of the community, for totally different motives, and for what many will say are far less worthy motives, are doing precisely the same things without any legislative interference. Are you acquainted with the existence of any feeling of that kind?—Yes. I think that is a feeling which does prevail. It is a feeling which has prevailed with one of my friends very strongly in reference to the whole enquiry. But though I should decidedly think it would be very unequal legislation, and though I think that aspect of the case is one which the Commission and Parliament cannot leave out of consideration, I think that what is due to science and the position of science is much above requiring even the aid of what I should call such collateral help. That is the *tu quoque* argument. I agree to it quite, but I think that we can fairly justify our position, and more than justify it, upon its own merits.

5496. It is sometimes asked whether such legislation as that under which cruelty to domestic animals is prevented, and such as the Factory Acts, is not legislation directed to the good of the thing protected. and therefore whether a corresponding legislation in regard to scientific experiments might not be permitted. But then, on the other hand, such an operation we will say as spaying a sow for the sake of making it get fatter a little faster could hardly be said to be a thing done for the good of the animal, and therefore it would need legislative interference just as much as scientific experiments?—Yes.

5497. Not to mention a hundred other instances that occur to one?—Science I think justifies itself. But if I wished to justify it on the lower ground that there are other kinds of vivisection, if you call it so, which are not prevented, one might even go you know very much further. I quite think it would be, as your question suggests, unequal legislation, but I was anxious, if I could, to keep the claims of science apart from those low grounds. I should like it to be understood that I did quite enter into that argument, but that I thought science should be kept from any interference on such grounds.

5498. As regards the increase of demonstrative teaching to which (if I may say so) you justly attach so much importance, may I ask you, as having so much experience as a teacher, whether it is not your opinion that the spread of careful methodical instruction in laboratories is probably more calculated than anything else to diminish any tendency which may exist towards the wanton infliction of pain?—Certainly. I should like to add that ignorance is wanton and cruel, and not knowledge. They are opposed in this.

5499. (*Chairman.*) May I ask you then whether the effect of legislation would not be to act upon ignorance and to leave knowledge untouched?—I should like to know what ignorance, because I do not know the ignorance thus referred to. I can understand that as Parliament is omnipotent it may legislate for anything, but I do not see how it could legislate for a thing which, as far as I know, does not exist; that is what I mean. I may observe that noticing the number of clergy whose names were appended to a public address on this subject, and reflecting what motive (for knowledge it could not have been) could have actuated them, I thought I could see the same spirit as animated the Papal authority in the repression of intellectual progress, and I feel as sure as one can be of an ordinary impression that there is this animus in the matter. We have many motives of which we are unconscious, and I suspect that if we could analyse all the circumstances we should find there is a feeling that we ought not to be enquiring into the profound mysteries, as they are called, of living things.

5500. (*Mr. Huxley.*) From what you have told us there are probably few persons who are more intimately acquainted with the medical students of London than yourself?—Perhaps very few. I lived 15 years in Guy's Hospital with them, and I was occupied in all 30 years with them there.

5501. A statement has been made publicly which I will read to you directly, that the English medical student is attracted by sights of suffering, and is likely to throng exhibitions of animal torture as he would throng no other exhibition. Is that in accordance with your experience of the English medical students? —I should say utterly contrary, and a very serious libel upon medical students.

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5502. The statement to which I refer is contained in a paper called "The moral aspects of Vivisection," written by Frances Power Cobb, and the statement is this: After speaking about students trooping out to see an animal killed, it goes on to say, "The same "keenness of observation, or a memory of their own "youthful insensibility, ought to teach all professors "of physiology that they are indulging a maleficent "tendency which already exists in their pupils dis-"position when they invite mere lads of the Bob "Sawyer type to watch their frightful experiments, "the more frightful, so much alas! the more attrac-"tive." In the whole course of your experience have you ever seen anything which could cause even the remotest justification of that phraseology?—I have not.

5503. (*Mr. Hutton.*) Did I rightly understand you to say that you thought the law against cruelty to animals was made for the ignorant, and not meant for the educated in case they should be cruel?—I cannot say, of course, what was the animus of such legislation, but I should suppose that the Society for the Prevention of Cruelty to Animals was chiefly intended for those who were cruel.

5504. I speak, not of the society, but of the Act with reference to cruelty to animals. Do you not think it should be applied to the educated if they were cruel?—Wherever there is cruelty it should be applied, of course.

5505. I understood you to say that experiments of a painful kind were hardly in any cases desirable in the interests of science; that they were so few, in fact, that the number was insignificant. Was that your meaning?—I said that as regards the teaching of physiology such experiments were few and insignificant.

5506. But I understood you to say that even as regarded physiological inquiry they were so?—I said that as regarded inquiry I could not say that; but whoever should be pursuing an inquiry must be left to his own mind and his high purposes, and to the consideration how far pain was necessary.

5507. But you would not deny that a very large number even of demonstrative experiments may be painful, and that many teachers would regard them as necessary, while others would not?—I could not answer for all teachers; but I should deny that there were painful experiments usually required in scientific demonstration.

5508. What do you say as regards the exhibition, for instance, of the effect of poisons upon animals. They clearly cannot be made always painless?—That depends upon the poison.

5509. Take strychnine for instance?—But we do not poison animals often with strychnine. There may be such instances, but that is not often done.

5510. We have had various lecturers before us saying that they thought it necessary for their classes to see those experiments?—If they were men worthy to occupy the position of teachers, and high minded men, I would not stand in their way, though I might not myself think it necessary to show those experiments. I think there are not many such experiments.

5511. What would you say of the experiments on the nerve system—on the sensory nerves?—If you will tell me what experiments are made on sensory nerves at this moment to demonstrate to students I will answer the question.

5512. Take such experiments as Sir Charles Bell's?—But they are not now repeated before classes.

5513. But we have had evidence that such an experiment might be a legitimate one to show to the ordinary medical student?—If a man is worthy of the position of a teacher, and is open to public opinion, if he should think it desirable to perform any experiment I would not stand in his way. If he erred against public decency he would be open to animadversion. I think it is quite beyond my power or knowledge to say to any teacher of character, "You shall only teach in my way"; it comes back again to the papal authority which says "You shall only read these books." It would be saying in fact that there is to be a physiological Index Expurgatorius.

5514. Supposing it were recommended to try some of these experiments on human beings you would not be in favour of that; there is a papal index as you call it there?—But if that were done the law would repress it, therefore we do not want legislation.

5515. It is not any more a papal index in one case than the other; it is a question of degree, is it not?—Everything is a question of degree. I think you and I are but questions of degree; all is but degree.

5516. I want to get at this—how many of these experiments which are really painful you think desirable? You say that for demonstration very few; but are there not very many performed for the purposes of inquiry?—I may state that I sent to Dr. Pye Smith to ask him to inform me what was going on at Guy's on this matter. I said, "will you inform me what amount "of physiological demonstration goes on at Guy's hos-"pital day by day;" he said, "I am sorry to say, "little or none at all;" I said, "do not the students "come?" he said, I only get about two students out "of 300 to come and see physiological demon-"strations."

5517. But I say, putting aside the question of demonstrations altogether, are there not a very large number of exceedingly painful experiments that have been tried, and are being suggested every day and will be tried for the purposes of investigation?—If you come to the question of investigation that is a thing which I think you must not touch. If the investigators of science are to be legislated for, then science must go to some other country. If we are to lay down how investigators of science are to make their investigations they will have to go elsewhere, that is quite clear, therefore I should not dare to put any limit to investigation.

5518. Now would you hold to that even if it were proved that there are biologists who regard the sufferings of animals as perfectly indifferent, and regard the waste of a few minutes of their own time in administering anæsthetics as of more importance than the agony of the creature?—There are so many "ifs" you see there, but I doubt the fact.

5519. Would you kindly answer the hypothetical question which I have put?—I think that the hypothetical question is so far contrary to the fact that any categorical answer to it assumes the condition that there are such people. I see the logic at once. The object is to assume a hypothetical condition, and to get a definite answer to the hypothetical condition, and then to use the answer as if such things occurred. You first put a hypothetical condition, and you want a categorical answer, and from the categorical answer you would argue towards the hypothetical condition.

5520. Perhaps I may inform you that we have had a witness, of very high scientific character indeed before us, who did assert that view of the case, and who asserted also that he believed that a very large number of scientific investigators on the Continent shared his view; now would that alter your view of the matter?—It would not alter my view of the matter as regards this country; because it does not touch this country as far as I see. It would affect me as it would affect you. I should think that a man who talked so was talking at random, and laying himself open to very serious animadversion.

5521. The point of my question was this, that hitherto we have been acquainted with physiologists who are also medical men, and who take naturally the humane view that medical men do take of all suffering, but that a large school of physiologists are now springing up whose sole interest is the investigation of the phenomena of life, and who might fairly regard, as this gentleman to whom I have referred did regard, the question of suffering as immaterial; what I wished to ascertain was, if there was any chance of that, would you object to putting limits on that kind of inquiry?—I should certainly say that when such a state of things occurs, if it cannot be repressed by the public feeling of those around such

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a man (which I feel sure it would), then it would really be a matter for legislation. You put it hypothetically that there is coming such a race, but I do not know that.

5522. Do you not suppose that the investigation of the phenomena of life, without any association with the alleviation of human suffering, is not unlikely to give rise to a less careful school than that of the scientific medical men ?—Well, I am not so certain of that; because to the scientific inquirer pain is a fact like other things; pain is a fact which he has to consider.

5523. But it does not follow that he wishes to prevent the pain, does it ?—But I do not think it follows that he would not prevent it.

5524. No; but that he may regard it simply as a scientific fact. However, I only wish to put this question: do not you think that there is a danger that where a school of physiologists whose only view is the investigation of the phenomena of life is springing up, there may be much more carelessness of suffering being inflicted than there would be in schools of physiologists who had always been associated with the alleviation of suffering ?—If I were to say I did I should be saying what I do not believe; because that would be saying that I believe Mr. Huxley is more cruel than I am; and I have no reason to think that Mr. Huxley would be more cruel than I should be, although he is a physiologist who has not to do with the cure of disease, and I have.

5525. May I ask you, as you are a great physician, whether you can enumerate to us any considerable number of therapeutic remedies which have been discovered by this process of vivisection; I am of course distinguishing between the diagnosis of disease, upon which everybody knows light has been thrown by it, and therapeutics ?—The cases bristle around us everywhere. Our knowledge of dropsical affections, of pulmonary apoplexy, of engorgement of the liver, and the whole category of such affections is due to Harvey's discovery of the circulation. We knew nothing about them before. Knowing therefore their causes we are able in the same directions to apply the remedies. Then again that same discovery of Harvey's taught us the use of transfusion of the blood in case of hæmorrhage, which is a cure for it. Sir Astley Cooper's experiments on arteries showed the surgeons that they could cure aneurism of the larger vessels, even to the extent of tying the largest vessel in the body; he tied the aorta of a dog, and the dog recovered. Hall, by his experiments on the nervous system, pointed out to us the theory of all the spasmodic affections, and how to apply our treatment by removing the exciting cause; it might be a tooth; it might be a spicula on a bone; it might be the irritation of a wound; all of which is essentially therapeutic.

5526. But have you any improved mode of healing a wound which has resulted from these experiments ?—Yes. For instance, suppose a man stuck his leg through a window by accident, and some glass got in it, you would twenty times more carefully examine that wound to remove the particles of glass with our present knowledge of reflex action than you would have done before.

5527. (*Mr. Huxley.*) Are there not some very remarkable cases of epilepsy arising from local causes of that kind ?—Yes; and which are entirely cured in that way. So that it is essentially therapeutic. If I went into therapeutics as connected with the functions of the nerves I should have to extend my remarks to the widest extent; I do not know where they would stop. Our knowledge on this is all due to experiments on living creatures, and could not have been gained otherwise, but they were not attended with great suffering.

5528. (*Mr. Hutton.*) What was the nature of the therapeutic remedies discovered ?—I have mentioned removing the cause of irritation; a worm in a boy's intestines for example; a spicula on a bone; and so on; cases of people liable to asthma by reflex irritation; the examples are of the widest kind. Then our present theories of inflammation which is the monster fact of disease, have been elucidated more by experiments on animals than in any other way.

5529. But what remedies have been discovered by such experiments ?—We must first know the causes and then the remedies. We have learned that inflammatory action in a very large number of cases is due to disturbed nerve functions; and therefore, instead of treating inflammatory action by antiphlogistic methods, we have learned to support the nervous system, and our patients have recovered. That is therapeutics of the very highest kind, and that has been the great fact of modern therapeutics. Then with regard to tubercle—there are some very curious facts now coming to light about tubercle and the tuberculous condition through experiments on living animals. I do not say at present our therapeutics are much, but there are lines of experiment which seem to promise great help in therapeutics. Some wounds or scrofulous glands which have hitherto been considered rather as a relief to the system and even promoted, there seems reason to think may become seats from which the semina of disease may spread; and if it should so turn out, we may be able to stop many of the tuberculous affections in their bud. I do not say we shall, but still there is a line which seems to be opening in that way by experiments on living animals. Then again the whole theory of vaccination came from experiments on living animals. It is true that Jenner was taught vaccination through the people who took cow-pox from the cows, but we did not know all it meant. By Mr. Ceeley's experiments on animals, by re-vaccination from the human subject, we learnt the real bearing of vaccination, and vaccination is highly therapeutic. It has saved more lives I believe than any other known fact.

5530. (*Chairman.*) I think I remember hearing you once say at the London University that it had saved more lives than all the other discoveries put together ?—It has been put thus;—that the wars of Napoleon were very destructive, but that the discovery of vaccination, which was about that time, saved more lives than his wars destroyed.

5531. Was it by you that that was stated ?—No.

5532. But you adopt it ?—I adopt it entirely.

5533. (*Mr. Hutton.*) Have you ever suggested that certain surgical modifications of the living body might be made which might be transmitted to future generations, and might really improve the organization of the body permanently ?—No; I will explain what I did say, though it did not merit the name of a suggestion, and it was rather meant of lightness than of gravity.

5534. Was it not said to students ?—No. I was President of the Clinical Society, and it was said to a number of members of my profession, and not to students; but even if it had been said to students I should be quite prepared to maintain that science may hereafter show that in our bodies there may be superfluous parts, relicta of ancestral relations, which linger in us, and which (though I do not know them now) may hereafter be found to be useless, and that it is a conceivable object of science to find them out, and it might even be a conceivable object of practice to remove them.

5535. Would you justify experiments upon living animals for investigations of that description ?—I should not say that, because I do not think we are ripe enough for such inquiries; but we may become ripe enough hereafter.

5536. Do you point to the spleen, or what kind of redundant organ ?—As one example the prepuce is considered to be redundant, and its removal useful.

5537. (*Mr. Huxley.*) The teeth which are present in the jaws of the fœtal whale is a fact of the class to which you refer, is it not ?—Yes.

5538. (*Mr. Hutton.*) Is it borne out that modifications caused by these surgical operations are transmitted to future generations ?—I did not affirm they were; but what I hinted was this: Pointing to my friends the surgeons, I said, "Should advancing

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"knowledge show that we have superfluous parts or "organs, and especially if these are liable to disease, "what a land of promise for operations!" But that was merely hypothetical; it was no real suggestion that the surgeon was now to be called in.

5539. It was a joke, was it?—No, indeed it was not a joke. I should say it was an anticipation.

5540. (*Chairman.*) You would rather say it was leviore plectro?—Yes.

5541. (*Mr. Huxley.*) But if you were disposed to argue the case you would have such facts as those relating to the hereditary transmission of the effects of injury, recently brought forward by Brown-Séquard to back you; there is a basis of argumentation for you to go upon there?—Yes, but I did not think the thing was ripe for assertion.

5542. (*Mr. Hutton.*) You would not justify experiments on living animals for a hypothesis so crude as that?—Certainly not; I should not perform experiments at the moment for such a hypothesis, nor should I recommend others to do so.

5543. (*Mr. Huxley.*) But there really is no subject at the present time of greater importance in Biological science than hereditary transmission, is there?—It is most important.

5544. And I presume that if a competent person thought it desirable to perform experiments on hereditary transmission, you would not say that it was wanton conduct?—Certainly not.

5545. (*Mr. Hutton.*) As regards remedial drugs, are there many which you could enumerate which have been discovered by these processes?—I am sorry to say that I am not a great believer in drugs.

5546. But there are some drugs, such as quinine and others, which are useful, but which have not been so discovered?—Of course I do not think that all knowledge is obtained by vivisection.

The witness withdrew.

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MR. GEORGE RICHARD JESSE recalled and further examined.

5551. (*Chairman.*) When we adjourned the other day, there had not been time for you to finish your statement. Will you be so good as to proceed with it now?—With regard to the list which I was then requested to furnish of the subscribers to the Society for the Abolition of Vivisection, I may state that I am having it prepared. In regard to Mr. E. C. May's letter, which the Commission, I think, demurred to printing in the Appendix, on the score that he being alive, he ought to give evidence himself, I have communicated with him, and he tells me that he has written to the Commissioners to state respectfully that his health totally precludes him from coming here. Under those circumstances, I presume that you will receive the letter and print it.

5552. At all events the Commission cannot receive it from you. Will you please proceed with your statement?—Your decision has quite taken the Society by surprise. We thought, and doubtless correctly, judging from the copy of the Royal Commission on Vivisection, that you had the power to do that; as it says, "To call for, have access to, and "examine all such books, documents, and papers as "may afford the fullest information on the subject, "and to inquire of and concerning the premises by "all other lawful ways and means whatsoever." I merely mention that lest you should have forgotten it.

5553. Will you have the goodness to proceed with your statement?—Certainly, having done all that I can upon that point. Then I think the Commission also objected to allow me to read even a passage from the works of Chalmers; is that so?

5554. It is so?—I understood that that was on the ground that I could not state in what portion of his works it occurred; but one of our Society has been to the British Museum, and I have this morning received the particulars.

5555. What you were told the other day was that if you would have the goodness to put in the reference it was open to you to do so. As I understand, you were not able then to put in the reference, but you are now?—I am.

5556. If you will be so good as to put in the reference we will take notice of it?—The extract is from a sermon by Thomas Chalmers, D.D., published in Edinburgh, March 5th, 1826. I should have thought that a Scotch divine might have been heard against Scotch vivisectors. The sermon is published in a separate volume, second edition, published by Chalmers and Collins, Glasgow, 1826. Then the Society wishes to express most respectfully its regret that it should have been thought necessary to issue a Royal Commission on this subject, inasmuch as torturing animals is contrary to the existing law; and the Society most respectfully wishes to submit that what is required on this question at present, is to enforce the existing law of the land, which is this: the Act 12th and 13th Victoria, chapter 92, passed August 1st, 1849, entitled "An Act for the more "effectual prevention of cruelty to animals." Section 1 repeals 5 and 6 William the Fourth, chapter 59, and 7 William the Fourth and 1 Victoria, chapter 66. Section 2 is, "And be it enacted, that if any person "shall cruelly beat, ill-treat, over-drive, abuse, or "torture, or cause or procure to be cruelly beaten, "ill-treated, over-driven, abused, or tortured any "animal, every such offender shall for every such "offence forfeit and pay a penalty not exceeding 5*l*." There is more in the Act, but that I think is all that it is necessary for me to read. The Society has received very numerous letters from various parts of the kingdom, and from abroad, even I think as far as Peru, expressing great feeling upon the subject, and the very strongest indignation which language can express in regard to the practice of vivisectors. It would take up too much of the time of the Commission, I suppose, (though I do not know for certain about that, to read these documents; but I should like to read one that I received only the day before yesterday, and which may be taken as a sample of many more. Of course we think, and I believe the public will think, these letters very important on this question; because they express the sentiments of a great number of people of education and rank.

The witness withdrew, and the Commission deliberated.

After a short time the witness was again called in.

5557. (*Chairman.*) You have offered us in evidence a letter which we understand comes from Peru?—No; I said we had had letters even from as far as Peru.

5558. What is the letter that you wish to read to the Commission?—It is a letter on the subject of vivisection.

5559. From whom?—From a clergyman who was educated as a physician; in fact, he is a physician too.

5560. Where does he live?—In Devonshire.

5561. Then the same principle which prevents our receiving Mr. May's letter will prevent our receiving any other letter addressed to you by somebody who is not here to be examined by us.

(*Witness.*) That applies to all letters which have been received, doubtless, by the association on the subject of vivisection; very good, I am sorry to hear it.

(*Chairman.*) There are some limits to all things. When a decision is communicated to you by the Commission will you be good enough to accept it, and not to argue it?

(*Witness.*) There are limits to all things. I beg leave to state that I have come here at the request of the Commissioners, and that I represent a society. I

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beg leave to say also that when I was here on a previous occasion there were marks of disrespect from one end of the table which I did not think it worth my while then to notice, but as I have been spoken to in this manner, I beg leave to say that I am sorry that any person appointed by Her Majesty should behave in that way to a witness.

(*Chairman.*) Will you have the kindness to leave us to ourselves for a little while?

The witness then withdrew, and after a short time was again called in.

5562. (*Chairman.*) I am desired by every member of the Commission who is here present to disclaim all intention of having shown any mark of disrespect to you upon the former occasion, and I am sure that I may say the same with regard to any colleague who may have been then present, and may not be present now. Now I have to ask you whether you withdraw that imputation?—I stated my belief at the time. I felt confident of it. I thought it even attracted your own observation. May I ask if it is so? Did anything of the kind attract your observation on a previous occasion?

5563. I have told you on the part of all my colleagues here present that they individually disclaim any such intention, and I ask you, therefore, whether you withdraw the imputation?—Under those circumstances, I am quite ready to say that I am glad to find that I was mistaken.

5564. Now will you proceed with your statement?—"Human Physiology, by John Elliotson, M.D., " Cantab, F.R.S., Fellow of the Royal College of " Physicians, President of the Phrenological Society, " late Professor of Medicine in University College, " London, and Physician to University College " Hospital, formerly Physician to St. Thomas's " Hospital, and President of the Royal Medical " Society of Edinburgh, &c. &c. &c." "London, " Longman, and Company, 1840" (that is enough to enable you to identify the book). On page 448 I read:

"I will not presume . . . .
. . . . the French Institute in 1826."

Then at page 428 there is this:

"Dr. Magendie, who cut living animals here and there with no definite object, but just to see what would happen, informs us that," and so on.

Then on page 429:—"If ever he amused himself by sticking pins in the chorda oblongata of pigeons, the birds thus ornamented by him would walk and fly backwards for above a month!"

On page 465:—"Numerous as have been Dr. Magendie's physiological errors, humbly as I estimate his knowledge and reasoning powers, and much as I abhor his cruelty to brutes," and so on.

Then on page 423, "See Gall, l.c., 8vo., t. vi., p. 210. From page 178 to 288 are excellent remarks upon the unsatisfactory nature of such experiments as have been made by Fleurens, Rolando, &c., &c. See also 4to., vol. iii, p. 56, and 8vo., t. iii, p. 379 sqq. The first three-quarters of the 6th volume should be read by all who are acquainted with the writings of these experimenters, or of Tiedemann, Rudolphi, Serres, &c., upon the brain. They will find these writers less meritorious than they imagined."

Then, on page 424, "Where is the anatomist or physiologist who precisely knows all the origins, the whole extent, all the ramifications, all the connections of an organ? You remove the cerebellum, at the same moment you severely injure the medulla oblongata and spinalis, you injure the tuber annulare you injure the tubercula quadrigemina; consequently, your results relate not merely to all these parts, but to all those which communicate with them, either directly or indirectly. You think you have insulated the tubercles, but these tubercles have connections with the corpora olivaria, the medulla oblongata, the cerebellum, the sense of vision, and many convolutions; the thalami, optici, the corpora striata, are connected below with the crura cerebri, the tuber annulare, the medulla oblongata, the pyramids, and the spinal marrow; above, with all the cerebral membrane, all the convolutions, the non-fibrous grey substance of their surface, with the different commissures, as the anterior commissure, the great commissure or corpus callosum; with the fornix, the septum lucidum. Thus there does not exist a cerebral part which we do not know to have numerous connections with other parts. I do not except even the corpora mammilaria, the pineal gland, the infundibulum, &c. The connections yet unknown are unquestionably still more numerous. (Gall, l.c., p. 240, sqq.) Sir C. Bell has lately imitated Gall in objecting to vivisections as a means of discovery. Gall's nature was most tender. He had a horror of inflicting pain upon poor brutes, and would allow Dr. Magendie to be little more than a canicide. He always kept birds and dogs in his house at Paris, and I have seen him kiss his horses on alighting from his carriage at his country house, and then stand to receive the caresses of several immense bloodhounds which put their fore legs upon his shoulders. (See his glowing remarks on cruelty to brutes, l.c., 4to., vol. iv., p. 196, 8vo., t. v., p. 259, sq.)."

Then on page 426 it goes on to say: "Hence the contradictory and strange observations and inferences of most experimenters on the brain of living brutes." And on page 427: "In opposition to M. Fleurens, MM. Fouville and Pinel Grand-Champs ascribe to the cerebellum the function of sensation. M. Fleurens, after removing the cerebrum, declared all sensation and volition to be lost. M. Bouillaud found animals so deprived give signs of pain, and exert will in endeavouring to escape (Dr. Magendie's Journal, t. x. p. 36 sqq.)."

The society's object in bringing forward some of these works is not only to show the cruelties perpetrated upon our weaker fellow creatures, commonly termed animals, but to show the demoralising effects upon the minds of those who perpetrate them, and which, of course, must tend to spread through society, and particularly when, as now it appears, that these things are to be taught to boys and girls. Now I am going to quote here a man of the first eminence as a veterinary surgeon. The book from which I take the quotation is "The Obligation and Extent of Humanity to Brutes, principally considered with reference to the Domesticated Animals, by M. Youatt, author of 'The Horse,' 'Cattle,' and 'Sheep,' Editor of 'The Veterinarian,'" and so on, published by Longman and Co., 1839, London, and this is on the title page—

'The heart . . . .
. . . . his own.'—
Cowper.

At page 200 I find this:

"'It is,' says Dr. Crampton, ...
. . . . by the very roots."

Then on page 202, "In a northern journal, the Phrenological Magazine, No. 29, are recorded a series of abominable experiments in illustration of the science of phrenology, and to determine the supposed functions of the different parts of the brain. They are so outrageously cruel that they ought not to have been tolerated in any civilized country, much less in the university of modern Athens. 'One experiment consisted in the entire removal of the cerebral lobes from a pigeon; the paring away of the cortical substance from the convex surface of the brain of a second, and of the central hemispheres of a third.' These three pigeons were then coolly and deliberately watched till the moment of their deaths, in order that this brute, rather than a philosopher, might ascertain precisely how long an animal, under such circumstances, retains its power of intelligence, and its faculty of recognising external objects; and also how many acts it was capable of performing, which display any determined end or motive. A hen was deprived of the anterior part of the brain. What was the result of this experiment? The poor fowl not

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having the use of its faculties, as before the injury was inflicted, after being hunted from place to place, attacked by other fowls, thrown from various heights, exposed to violent rain, and to the heat of a kitchen fire, and burnt by hot irons, died in consequence of another experiment. A young dog was selected 'which possessed the reputation of being lively, docile, and intelligent.' And for what purpose? To have 'a thick gimlet forced into the anterior lobes of its brain;' and then to be watched, in order to discover how he will 'eat, drink, and walk;' to have burning irons forced into each anterior lobe; to be pinched occasionally, to ascertain how much feeling he has left, and to have other injuries inflicted upon him, until, on the sixth day he dies. Soon after this, another 'young, lively, docile, and intelligent dog' was selected. The anterior part of the brain of this animal was also transfixed, and his various agonies were closely and minutely watched and recorded. Those words 'watched and recorded' are in italics. 'Water was poured down his throat; camphor was administered. When menaced he crouched as if to implore mercy, and uttered cries which nothing could repress. When menaces were succeeded by blows, he lay down in a supplicating posture and whined. He burnt and scalded his muzzle, lips, and feet by attempting to eat food which was purposely placed before him just taken from the fire.' 'Some days afterwards,' those three words being in italics, 'I led it,' says the miscreant, 'to the river, and regardless of its terror, threw it in. On this occasion it quickly swam on shore and returned to the house.' The poor tortured animal 'still manifesting docility in coming, when after caressing it, we called upon it in a tone of kindness; or if we had menaced, beat, or called upon it in vain, in going away holding down its head and tail, and crouching down as if in the act of supplication. Its eyes became animated, its ears were erected on the slightest noise; but otherwise it had a look of imbecility.' The experiments lasted during 16 days; and, adds M. Bouilland in capital letters, 'It was sacrificed in the performance of a new experiment.' We do not believe that the records of any age contain atrocities more infamous than these. The new Act of the 5th and 6th William IV., surely extends to barbarities like these. We trust that similar exhibitions will never be attempted in any part of the United Kingdom; or if they are, and wherever the disgraceful scene may be enacted, we trust that some persons may be found humane and courageous enough to summon the offender to the tribunal not only of public opinion but of common laws."

This is what the Society at the present moment is endeavouring to do at Edinburgh in regard to Professor Rutherford. We have placed the matter in the hands of our solicitor with regard to experiments of his lately published in the British Medical Journal. On page 20 occurs this quotation; it is from Charles Bell, no doubt Sir Charles. "For my own part I cannot believe that Providence should intend that the secrets of nature are to be discovered by the means of cruelty; and I am sure that those who are guilty of protracted cruelties do not possess minds capable of appreciating the laws of nature."

The society thinks that it is important to show the nature of some of these creatures who are tortured in this manner. You hear some people say that animals have not minds; others deny them immortality, though the latter is certainly what no man can know; and Butler, in his Analogy, says that what applies to the immortality of man applies also to them; and men who have studied the subject know that men of the first calibre of mind (Locke for example) have held the opinion that they have minds, and some believe that they are immortal.

Then on page 45, "My own experience," so this is not only recorded by Youatt, a man of the first eminence in his occupation——

5565. **Are you going to show us that Mr. Youatt's opinions are in favour of the immortality of animals?**—No, not that; but I am going to show here that the intellect and moral qualities of these creatures are such that that consideration alone ought to prevent them from being treated in such a way. That is all. Of course I am not going into an abstract question of that sort.

"My own experience . . . .
. . . . admiration and gratitude."

The Quarterly Review, No. 170, September 1849, at page 390, contains the following: "It was a desire
. . . . in our own day."

I am now about to quote from "Dogs: Their Management; being a new plan of treating the animal, based upon a consideration of his natural temperament, illustrated by numerous woodcuts depicting the character and position of the dog when suffering disease, by Edward Mayhew, M.R.C.V.S. author of 'The Horse's Mouth, showing the Age by the Teeth,' editor of 'Blaine's Veterinary Art,' &c. &c., London, George Routledge and Co., 1858"—On page 163 he says:

"The French have . . . .
. . . . recoil with disgust."

Then on page 73, "The mouth of . . . .
. . . . destined to be its food."

I next quote from "The Life of Sir Astley Cooper, Bart., by Bransby Blake Cooper, Esq., F.R.S., London: John W. Parker, 1843. At page 200 of the first volume I find this:

"Dr. Rools of Kingston . . . .
. . . . the human structure."

Then at page 334 you will find this: "It was not . . . . this scientific receptacle."

We read that to show how this tends to demoralise society. Here were the servants of this surgeon; he made thieves of them; they went and stole dogs for him, taking other people's property. There is another extract in that same volume which I might read, but I think I should only take up your time by reading it; it backs up the other, but I do not think it requires backing up; it can stand by itself. On page 443 of the second volume there is this note:

"His investigations on . . . .
. . . . of the brain."

I conceive from that Sir Astley was afraid of the accusation, not of the cruelty.

I shall now quote from "Canine Pathology, or a Description of the Diseases of Dogs," &c., by Delabere Blaine, London, T. and T. Boosey, Old Broad Street, Royal Exchange, W. Simpkin and R. Marshall, Stationers' Hall Court, Ludgate Street, 1832, third edition. Perhaps I ought to mention that Blaine is, I believe, from what I have heard from veterinarians, also from what I have seen of his books myself, but judging principally from what other veterinarians say of him, the most distinguished veterinarian that this country or any other probably ever produced. He says in regard to mineral poisons, at page 182, "Those who wish for further information relative to the effects produced on dogs by various poisonous agents, may consult Abbé Fontana, Orfila, Mr. Brodie" (the late Sir Benjamin I believe), "&c. &c., who have sacrificed more dogs in experimental inquiry than humanity dares to think of, though science might demand it."

At page 35 there is this passage, "I have thus far
. . . . can be aware of."

I will now read from a "Memoir of George Wilson, M.D., F.R.S.E., Regius Professor of Technology in the University of Edinburgh, and Director of the Industrial Museum of Scotland, by his sister, Jessie Aitken Wilson. Edinburgh: Edmonston and Douglas, 88, Princes Street, and Macmillan and Co., London and Cambridge, 1860." On page 340 I read: "It was at . . . . do as he did."

Then on page 347, speaking of Dr. Wilson, it says: "It is seldom . . . . a human heart."

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In speaking of the life which he wrote of the late Dr. John Reid, of St. Andrews, on page 344, there is this passage: "The great matter . . . . Christianity upon them."

The next quotations are from "Physiological, Anatomical, and Pathological Researches, by John Reid, M.D., Fellow of the Royal College of Physicians of Edinburgh; Chandos Professor of Anatomy and Medicine in the University of St. Andrews, &c., &c., Edinburgh: Sutherland and Knox. London: Simpkin, Marshall, and Co., and Samuel Highley, 1848." The first passage will show how contradictory the opinions of vivisectors are. One seems to lay down a theory by experiments on animals, and then others disprove it. Nobody can wade through (really that is what it has been to me) the works of these men without being forcibly struck by that; and really the want of intellect displayed by these men strikes one, the obtuseness and want of perception.

At page 555 there is this passage: "Cause of death. The older experimenters attributed death from the inflation of air into the veins, to distention of the right side of the heart, arresting its contractions; and some of them compared its condition to that of the bladder when over distended with urine. Bichat maintained that death begins at the brain, and depends upon the circulation of frothy blood in the vessels of that organ. Nysten, Cormack, and Amussat have referred the death to the mechanical distention of the right side of the heart. Leroy, and Piédagnel attributed it chiefly to emphysema of the lungs. Sir Charles Bell believed that the air, by circulating in the vessels of the medulla oblongata, annihilated the functions of that important portion of the central organs of the nervous system, and thus killed by arresting the respiratory muscular movements. Marchel de Calvi supposes that death is due to the action of the carbonic acid contained in the air,—a supposition which could be readily disproved by a reference to some of Nysten's experiments. Bouillaud, in his report to the Academy, attributes the death partly to the distention of the right side of the heart, and partly to the difficulty of transmitting the frothy blood through the lungs; and Mr. Erichsen refers it entirely to the latter of these two causes. Wattmann (p. 70) gives at some considerable length his explanation of the cause of death, which is much too long to transcribe. He attributes it partly to the disturbance and enfeeblement of the contractibility of the heart, occasioned by the mechanical effects of the air in its cavities, partly to the derangement of the respiratory function; and partly to the circulation of blood mixed with air in the capillary vessels of the systemic circulation being unable to maintain the nutrition, and the vitality of the tissues, especially of such important organs as the brain, the spinal cord, the lungs, and the heart. We do not think it necessary to enter into any critical examination of the arguments advanced by the supporters of these different explanations of the cause of death, as the account which we have already given of the appearances observed after death, and the symptoms which precede it, but more especially the former enables us to select the true one. As in almost all cases in dogs, in the majority of rabbits, and in individuals of the human species, and in a considerable number of horses and sheep killed either by the forced or the spontaneous entrance of air into the veins, no air was found in the left side of the heart, or in the arterial system, it is perfectly obvious that in all these cases the circulation of air in the arteries of the brain, and in the medulla oblongata, could not be the cause of death; for a thing which did not exist could not act. The theories of Bichat and Sir Charles Bell cannot therefore explain the cause of death in all or even the greater number of cases, and there is no evidence in their favour, even in those cases where air was found in the left side of the heart, and in the arterial system, for in all these the right side of the heart was on an average not less distended with air or frothy blood, and the death was not more rapid than when the air was entirely confined to the right side of the heart and the venous system."

Then on page 44 he says: "Since the above memoir was published, Mr. Erichsen (Edinburgh Medical and Surgical Journal, January 1845, vol. 63,) has given us the results of an extensive and careful experimental enquiry into the pathology and treatment of asphyxia, and he confirms the accuracy of all my experiments and observations on this subject, as far as he has repeated them. The amount of increase in the force with which the heart drives the blood along the arteries for a short time after an animal becomes insensible in asphyxia, will, I believe, be found to correspond nearly in both our experiments, when the difference in the form of the hemadynamometers used is taken into account. But while Mr. Erichsen does not object to my data, he dissents from one part of the theory of asphyxia deduced from them. We agree entirely in our explanation of the cause of the suspension of the sensorial functions, in fact, he has been pleased to say that he does 'not feel called upon to make any remarks on this point, as it has already been fully and ably investigated' in the above memoir; but we differ in our explanation of the arrestment of the circulation of the blood through the lungs. I have adopted the opinion that this is due to the cessation of the chemical changes between the blood and atmospheric air in the lungs; while Mr. Erichsen supposes that it depends upon the venous blood acting as an excitant upon the contractility of the ultimate ramifications of the pulmonary veins, and thus causing an obstruction to its passage along these vessels. This view of the cause of the accumulation of the blood in the pulmonic heart and its vessels, adopted by Mr. Erichsen, appears to be incompatible with some well established facts."

Then on page 46 occurs this passage: "Notwithstanding, therefore, all that Mr. Erichsen has so ingeniously advanced against the part of the theory of asphyxia that refers the impediment to the passage of the blood through the lungs to the cessation of the chemical changes which occur there in natural respiration, my belief in its truth has not been shaken."

It is often said that, in these experiments the animals do not feel. I will read a few statements made by Dr. Reid himself in regard to his own experiments, on page 69: "When the glosso-pharyngeal was pricked with the forceps, the indications of suffering were distinctly but not strongly manifested, but the application of a tight ligature was evidently attended by intense pain, an effect which we were somewhat surprised to find also attended the application of a tight ligature to the hypoglossal."

Then on page 73; "With the exceptions mentioned, very severe indications of suffering, and in a few cases also distinct muscular twitching of the neck and face attended the pinching and cutting of this nerve." That is headed "*Experimental investigation into the functions of the eighth pair of nerves.*"

Then on page 92: "I have exposed the trunk of the *par vagum* in the neck, in at least 30 animals, and in almost all of these, the pinching, cutting, and even the stretching of the nerve were attended by *indications of severe suffering.*" Those words "indications of severe suffering" he has placed in italics himself. "It was frequently difficult to separate the nerve from the artery, on account of the violent struggles of the animal, though some of them had been pretty quiet during the previous part of the operation."

Then on page 169 occurs the following passage: "As statements such as those of Mr. Brachet are, however, more effectually met by facts than by arguments," (I should say, I believe Mr. Brachet was considered one of the most eminent medical practitioners and scientific men of France in his day), "I proceeded to put them to the test of experiment. These experiments were seven in number, and six of them were made in the following manner: The vagi and sympathetics, and in some cases the recurrents also, were cut in the middle of the neck, and a portion of each removed. At a longer or shorter period after

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the operation, the pulsations of the heart were reckoned when the animal was lying or standing on the ground, and after it had been caressed for some time to calm its fears. It was then lifted up on the table on which it had been previously tied and operated upon, and after having been spoken to harshly the pulsations were again reckoned. After being again caressed for some time, the pulsations were counted a third time, and when replaced upon the ground they were reckoned a fourth time. The following results were obtained: In the first dog the pulsations of the heart were about 140 before the commencement of the experiment. The animal at this time was apparently somewhat alarmed. Four hours and a half after division of the nerves, the pulsations of the heart were about 170 when the animal was standing on the ground, and rose to 200 at least when placed upon the table. After it was replaced on the ground they had again fallen to about 170. After 19 hours the pulsations were 160 on the ground; they rose again to about 200 when placed on the table, again fell to about 160 when still on the table, and were not increased by being replaced on the ground. In the second dog the pulsations were 156 on the floor, and about 190 on the table; and in the third dog they rose 20 beats in the minute when placed on the table. In both of these two last experiments the pulsations of the heart soon subsided to their former frequency, and were not increased by replacing the animals on the ground. In the fourth dog the pulsations 24 hours after division of the nerves were 140 on the floor, and instantly rose to 180 on the table. After waiting until they had again fallen to their former frequency, they were not increased by replacing it on the ground. In the sixth dog the pulsations were 140 the third day after the section of the nerves, when the animal was on the floor, and were raised to 160 by placing it on the table. In these experiments it was particularly observed, that the animals made no struggles in carrying them to and from the table, and, consequently, the increased excitation of the heart must have arisen from the mental emotion of terror. In the seventh dog this was conjoined with violent struggles. The pulsations, eight hours and a half after the operation, were 130; when placed on the table, and made to struggle, the pulsations as far as could be made out were about 220; when he had been subjected to pain, and had struggled more violently, they became so frequent that they could not be accurately reckoned, but were at least 260 in the minute. A large tube had been previously introduced into the trachea in this last animal. These experiments are, we conceive, sufficient to prove that, after section of the vagi, the pulsations of the heart may not only be quickened by muscular exertion, but also by mental emotions. Though in all probability the vagi are the usual channels through which mental emotions affect the heart, yet it appears from these experiments that this may also take place through the medium of the ganglionic system of nerves.

"*Pulmonary Branches of the Vagus.*—In my former paper I gave the results of several experiments from which, in opposition to the observations of Magendie, Wilson Philip, and Swan, I concluded 'that lesion of one of the pneumogastrics does not necessarily or even generally induce disease of the lung of that side.' Since that time I have carefully examined the lungs of two dogs and a cat killed some time after a portion of one vagus had been removed. One dog lived two months, the other nine days, and the cat three weeks. No morbid change could be detected in the lungs."

Then on page 193: "To show that others have also sometimes observed great insensibility of the mucous membrane of the trachea, even when the vagi were entire and uninjured, I have only to refer to the works of Haller. He there relates several experiments upon different species of quadrupeds, two cats, a she goat, a rabbit, a lamb, a he goat and a sheep, in which the trachea was opened, and various irritating substances, such as oil of vitrol, butter of antimony, and fumes of sulphur were introduced into the air passages without exciting cough. Some of the animals gave indications of suffering, and breathed forcibly."

Then on page 197 (this was his own experiment). "The animal the subject of the 14th experiment coughed so incessantly during the three last days of its life that I could not reckon the respirations until after many trials, and even then imperfectly."

Then on page 87: "It is unnecessary to state how much these experiments are at variance with the opinion of Sir C. Bell, that the function of this nerve is to associate the movements of the tongue and pharynx with the muscles of respiration in the instinctive movements of deglutition," and that is the way they go on.

Then on page 88: "We have lastly to inquire in what manner the section of the glosso-pharyngeal nerve affects the sense of taste. My observations on this head are in perfect accordance with those of Dr. Alcock. Dr. Alison had an opportunity of witnessing the persistence of the sense of taste in one of the dogs, after a portion of the trunk of the nerve on both sides had been removed, and Dr. Sharpey was perfectly satisfied that the animal, the subject of the sixth experiment, was sufficiently sensible of disagreeable impressions upon this sense; and though in the case witnessed by Dr. Alison a few pharyngeal filaments, and in that witnessed by Dr. Sharpey one pharyngeal twig on one side were found to have been left uncut, yet it was obvious that the rejected morsel sprinkled with coloquintida was fully recognised before it passed beyond the anterior part of the mouth. I need not add that the lingual portion of the nerve was fully divided in both of these cases. The remark, however, was repeatedly made (and it is of importance as explaining the error of Panizza on this point), that if animal food was offered and the dog very hungry he would eat the morsel containing the coloquintida rather than lose it; though he refused it if he saw any prospect of procuring another free from the bitter. The subject of the first experiment in which, as was stated, the glosso-pharyngeal was cut on one side only even ate readily several pieces of bread dipped in a strong solution of gentian root." (Othello, I think, says, "As bitter as coloquintida." I suppose they picked out the bitterest thing they could find). "Lest any doubt may arise that the presence of a few pharyngeal branches could have influenced the sense of taste, I may adduce the subject of the fourteenth experiment to prove that when the nerve is divided before it has given off a single filament, still the animal retains a sufficiently acute perception of disagreeable savours. I have fed that dog with morsels of animal food from my hand, and after he had taken several morsels in this way, which he readily swallowed, I then presented a morsel similar in size to the others, and with the coloquintida concealed in a way that he could not see it, but no sooner was it taken into the mouth than it was rejected with evident symptoms of disgust. This was repeated more than once."

Then on page 90 this passage occurs: "I endeavoured to ascertain the state of the sensibility, and of the sense of taste in that portion of the tongue where this nerve is ramified, after the trunk had been divided on both sides; but from the restlessness and struggles of the animals I was unable to arrive at any satisfactory results." All that torture was thrown away even according to his own account. He had knocked down Bell I suppose, and some one will knock him down if they have not done it already.

Then I find this at page 163: "*Pneumogastric Nerves.*—I have again had ample opportunities of confirming the statement made in my former communication drawn from experiments upon dogs, rabbits, cats, and calves, that the pinching, cutting, and even the stretching of the vagi nerves when exposed in the neck, are in by far the greater

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majority of cases attended by indications of severe suffering. In opposition to the opinion expressed by Dr. M. Hall and Mr. Broughton, that the nervus vagus is not a nerve of sensation, I adduced the authority of Haller, Brunn, Dumas, and Dupuy. If additional evidence be thought necessary I may also add to those the names of Molinelli, Mayo, Magendie, and Brachet. In the first and fifth experiments upon the vagus, related by Molinelli, it is expressly mentioned that the animals (dogs) gave indications of suffering in tying these nerves with a ligature. Mr. Mayo says that 'asses, cats, and dogs almost invariably express great pain when this nerve, yet 'entire, is pinched with the forceps, and after its 'division equal suffering appears to result from 'pinching the part connected with the brain.' Magendie, in pointing out to his pupils an experiment where the nerve was stretched and cut without exciting pain, remarked 'In certain cases, on the 'contrary, the nervus vagus appears to possess the 'most exquisite sensibility; for it is scarcely touched 'without exciting immediately cries and convulsive 'motions.' (That does not look as if they felt no more than a piano-forte, as Dr. Crichton Browne stated of Dr. Ferrier's experiments.) "Brachet in one experiment irritated the upper end of the cut vagus with the view of subjecting the animal to suffering, and with success. I attempted to give an explanation of the source of fallacy which had misled Dr. M. Hall and Mr. Broughton in their very limited number of experiments; but I am now convinced that there is another circumstance more likely to lead to such errors than the one I mentioned, and that is the very different degrees of sensibility possessed by different animals, even of the same species."

Then at page 64 there is this: "*Part First, Glosso-Pharyngeal Nerve.*—The experiments on this nerve were all performed upon dogs, and were twenty-seven in number. Seventeen of these were for the purpose of ascertaining if it should be considered a nerve both of sensation and motion; and what are the effects of its section upon the associated movement of deglutition and on the sense of taste. The other ten were performed upon animals immediately after they had been deprived of sensation."

Now if you will allow me to make the remark, I will read the two next short sentences out of justice to this Dr. Reid. So far as my experience has gone it is almost, if not quite, the only instance in which I have found a thorough physiologist (when I say "physiologist," I use the word in the sense of an experimenter upon animals, putting them to torture for so called scientific purposes) with any shade at all of mercy; and perhaps that bore fruit in regard to his last hours. On page 112, I find this "I was anxious to ascertain whether irritation of these nerves would produce closure of the glottis by a reflex action. As the experiment is one in which it is difficult to arrive at accurate conclusions without inflicting much pain, I did not persevere in the attempt." Then on page 221, "The muscular movements of the stomach were, however, in all of these animals so indistinctly marked that I could obtain no satisfactory results; and as the experiment was a cruel one I did not persevere in it."

I will now read from Lockhart's Life of Scott, Volume IX., page 297, chapter 77, Letter to Miss Edgeworth, 4th of February 1829. I must hark back in regard to this. I am going to read this to strengthen what I stated in regard to the demoralizing effect which torturing animals produces upon those who inflict that torture; and I am now going to bring forward the evidence of no less a man than Sir Walter Scott, not only a man, as everybody knows, of high intellectual calibre, but nobody could call him unduly sensitive or enthusiastic, for a more daring and intrepid man has seldom lived. "I am no great . . . . learned carcase-butcher.'

I shall next quote from "Personal Recollections from Early Life to Old Age of Mary Somerville," &c. "London, John Murray, Albemarle Street, 1874." I do not think I can bring forward any authority more deserving of respect. She was so appreciated by scientific men that her bust is placed in the rooms of the Royal Society, an honour which I think was never before conferred upon any lady, at least I believe so; it is many years since I saw it. At page 192 you will find this: "The Marquise de La Place . . . . kindly nature abhorred."

At page 306 there is this passage: "A German professor . . . . not a little."

On page 348, this is towards the close of her life, she says, "The short time . . . . the lower animals."

The book from which I am now about to quote is, "Researches into the History of the British Dog," written by myself, and published in 1866. This is a quotation from Pope: "I cannot think . . . . use to us?' See Spence's Anecdotes, Singer's edition, page 203, and Pope's paper in the Guardian, on Animals, 21st May 1713."

5565*a*. (*Mr. Huxley.*) What is the title of the book from which you read that?—"Researches into the "history of the British dog." Published in 1866.

5565*b*. By whom is it written?—By myself.

I shall now quote from "Proceedings of the Royal Society, volume 22, number 151." This is in reference to the substance of what I read when I attended here on the previous occasion, in regard to Dr. Ferrier's experiments. "March 5th, 1874. Joseph Dalton Hooker, C.B., President, in the chair." The following paper was read:—"The Localization of Function in the Brain. By David Ferrier, M.A., M.D., M.R.C.P., &c. 1874. (Abstract.) The chief contents of this paper are the results of an experimental investigation tending to prove that there is a localization of function in special regions of the cerebral hemispheres. In a former paper published by the author in the 'West Riding Lunatic Asylum Medical Reports,' Vol. III. 1873, the results were given of experiments on rabbits, cats, and dogs, made specially for the purpose of testing the theory of Hughlings Jackson, that localized and unilateral epilepsies are caused by irritation or 'discharging lesions' of grey matter of the hemispheres in the region of the corpus striatum. Besides confirming Hughlins Jackson's views, the author's researches indicated an exact localization in the hemispheres of centres or regions, for the carrying out of simple and complex muscular movements of a definite character, and described by him as of a purposive or expressional nature. Facts were also recorded tending to show that other regions of the brain were connected with sensory perception, but no localization was definitely arrived at. Among the experiments now related are some in further confirmation and extension of those already made on cats, dogs, and rabbits, as well as a new series of experiments on other vertebrates. In particular, numerous experiments on monkeys are described, for the purpose of which the author received a grant of money from the Council of the Royal Society. In addition, the results of experiments on jackals, guinea-pigs, rats, pigeons, frogs, toads, and fishes are narrated. The method of investigation" (I wish to draw particular attention to this passage, as a good deal has been said about the animals being under anæsthetics,) "consists in the application of the stimulus of an induced current of electricity directly to the surface of the brain in animals rendered only partially insensible during the process of exploration, complete anæsthesia annihilating all reaction. It is supplemented by the method of localized destructive lesions of the hemispheres." That was the point that we wished to make about that.

I shall now quote from page 378, of "Proceedings of the Royal Society, volume 23, number 161, April 29th, 1875. The Duke of Devonshire, K.G., Vice-President, in the chair. The Right Hon. W. E. Forster and the Right Hon. Russell Gurney, were admitted into the Society." And at page 409, I find "Experiments on the Brain of Monkeys, No. 1, by David Ferrier, M.A., M.D., M.R.C.P., Professor of Forensic Medicine, King's College, London. Com-

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municated by Dr. J. B. Sanderson, F.R.S." The facts recorded in this paper are the results obtained by electrical stimulation of the brain of monkeys, after the method described by the author in the West Riding Lunatic Asylum Medical Reports, volume III., 1873. They formed part of a paper "On the localization of function in the brain," read before the Royal Society on March 5th, 1874. (See Proceedings, vol 22, p. 229.) This memoir also contained the results of other experiments on the brain of monkeys chiefly relating to the effects of localized lesions of several parts of the hemispheres, with a view to determine the significance, as regards sensation and motion, of the phenomena caused by electrical irritation. These experiments are not here recorded, but are reserved for comparison with the results of a more extended reinvestigation of a similar nature, on which the author has been for some time engaged, and which will shortly be laid before the Society. In order to avoid unnecessary detail, and in order to place the results together for the purposes of comparison, the animals experimented on are described, the dates of experiment given, and numbers assigned to them, so that they may all be brought into relation with each other:—"Experiments on monkeys (Macaques), No. 1, left hemisphere, June 14, 1873. No. 2, right hemisphere, June 18, 1873," and so on at different dates, 13 of them.

Then at pages 419 and 420, I read: "X. A similar result. In this case, after several other parts had been under exploration, excitation of this region gave rise to a species of epileptic fit, beginning in the left angle of the mouth, next proceeding to the left arm and hand, and lastly affecting the left leg and tail. The spasms next attacked the right angle of the mouth, the right arm, and the right leg in succession. The fit lasted several minutes. The pupils were not dilated, nor did the animal apparently lose consciousness completely."

Then on page 423, speaking of experiment 13: "The result in this case was also negative. To test this matter more fully, another monkey, not among those already numbered, was experimented on on December 10."

On page 424, there is this paragraph: "In a later experiment (December 2), on another monkey it was found that stimulation of the frontal part of the brain caused the eyes to move to the opposite side. This was found to be the case with irritation of both right and left hemispheres. The eyelids were not always opened, however, nor was dilatation of the pupils observed. Sometimes also the eyes moved upwards, instead of to the opposite side, &c."

On page 425, there is this passage. "IX. Both eyes directed upwards and to the left. Pupils contracted. In this animal," (I beg particular attention to this,) "which was allowed to remain quite conscious during stimulation, an experiment was made as to vision by holding before it a teaspoonful of milk, which it was eager to seize. In its attempt this point was stimulated, with the effect of causing confusion of vision and some difficulty in reaching the milk."

At the bottom of page 426, this paragraph occurs: "Nothing very definite was arrived at. In some the results were altogether negative; in others the following phenomena were noted, perhaps not altogether satisfactory as to their nature."

Then I find on page 428 this: "Occipital lobes (superior and middle convolutions). These were experimented on in 1, 3, 5, 7, 8, 9, 10, 11, and 12; also in another not numbered, on November 21." "In the case of 10, it was observed that stimulation of the inferior occipital convolution towards its inner aspect caused uneasy movements in the hind legs and tail, the head being turned to the left (opposite side) and backwards. Occasionally also a plaintive cry, as if from annoyance, was uttered. On cessation of the irritation the animal subsided into its dozing state."

On page 429, there is this passage: "*Corpora quadrigemina.* The ganglia were subjected to experimentation in the following seven cases, viz., V, VI, VIII, IX, X, XII, XIII, with the results:—V. In this case the exploration was not sufficiently definite, as the exact position of the electrodes was not observed, and death occurred before a more careful exploration could be made. The application of the electrodes to the ganglia on the left side (position as to the testes or nates not ascertained) caused the animal to utter various barking, howling, or screaming sounds of an incongruous character. The head was drawn back and to the right, and the right angle of the mouth was strongly retracted, while the stimulation was kept up. Tho tail was raised and the limbs were thrown into contortions, but nothing further was ascertained, as the animal died from hæmorrhage. VI. In this case irritation of the right anterior tubercle (nates) caused intense dilation of both pupils (especially beginning in the left), elevation of the eyebrows, and turning of the eyeballs upwards and to the left, at the same time that the head was turned in the same direction, with an intensely pathetic expression. Momentary application of the electrodes to the posterior tubercles (testes) caused the animal to bark loudly, the sound passing with longer stimulation into every conceivable variation of howling and screaming. Continuous application of the electrodes for several seconds caused ultimately firm clenching of the jaws, retraction of the angles of the mouth (particularly the left), elevation of the eyebrows, and retraction of the ears. The pupils were dilated, eyes widely open, and the head thrown back. The tail became elevated, the limbs, after contortions of various kinds, became rigidly drawn back, the arms drawn back and flexed at the elbows, and closely approximated to the sides. A complete state of opisthotonus was induced. The dilatation of the pupils occurred on irritation of both nates and testes; the screaming, &c., only on irritation of the testes. VIII. The results in this case were essentially the same in VI. as regards the dilatation of the pupils, howling, and rigidity of the limbs, &c. IX. As before, stimulation of the anterior tubercle on the right side caused elevation of the eyebrows, dilatation of the pupils, and turning up of the eyes to the left. Irritation of the ganglia for some time caused a condition of opisthotonus, and the phenomena described under VI. Irritation of the testes caused utterance of every variety of barking and howling, ultimately trismus and general opisthotonus. X. Exactly as in IX. XII. As before, irritation of the testes caused barking and howling. When the animal was nearly dead irritation of the testes caused only powerful retraction of the angles of the mouth, so as to show the firmly clenched teeth. XIII. In this case the results as to the nates and testes were in every respect similar to those already detailed in the former cases." Then he goes on to say at the bottom "On this point, however, further experiments are necessary."

Now I will quote from "Proceedings of the Royal Society, volume 23, No. 162, May 13th, 1875. Dr. J. Burdon-Sanderson, Vice-President, in the chair. The Croonian Lecture, 'Experiments on the Brain of Monkeys' (second series), was delivered by David Ferrier, M.A., M.D., Professor of Forensic Medicine, King's College. Communicated by Dr. Sanderson, V.P.R.S. Received April 27, 1875. The following is an abstract." And it says on page 432, "No. 5. Destruction of the hippocampus major and hippocampal convolution abolishes the sense of touch on the opposite side of the body." No. 9. "Ablation of the occipital lobes produces no effect on the special senses or on the powers of voluntary motion, but is followed by a state of depression and refusal of food, not to be accounted for by mere constitutional disturbance consequent on the operation. The function of these lobes is regarded as still obscure, but considered to be in some measure related to the systematic sensations. Their destruction does not abolish the sexual appetite." I wonder how he arrived at that! He made some experiments no doubt in that way after the style I suppose of Dr. Brachet described by Dr. Elliotson; at least it seems probable.

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"10. After removal both of the frontal and occipital lobes, an animal still retains its faculties of special sense and the powers of voluntary motion." Then I would refer you to "Proceedings of the Royal Society, volume 23, No. 159."

Then I will next quote from the Saint Bartholomew's Hospital Reports, volume 9, London, Longmans, Green, and Co., 1873. Article 12, page 161, "On the changes of the liver which follow ligature of the bile ducts, by J. Wickham Legg, M.D." On page 162 he says this: "During the past winter I have made several observations upon the changes which follow ligature of the bile ducts in animals. The animals used were cats; these seemed to survive the operation better than dogs. Most observers find that dogs live only five to ten days after," long enough in that misery.

Then on page 163 he goes on to say: "All the operations recorded in this paper were done in the pharmacological laboratory of my friend and colleague Dr. Brunton, I am therefore glad of this opportunity to express my most sincere thanks to him for his courtesy on this, as on many other occasions. Had I to repeat these experiments I should choose only young, not fully grown animals, and a warm time of year. Though the cats were kept in a warm place, and the January of this year was mild, yet three of them were found dead one morning after a slight frost in the night."

Then on page 175, Experiment XVI, that is on 16 cats I believe. (I do not read the others because they would not strengthen our cause and would only take up time, and one sack of wheat out of a hundred of course is sufficient to show the character of the other sacks.) "June 27. Black and white cat, well "nourished, full grown. Bile duct tied double and "piece cut out. July 3," (that will be six days afterwards,) as the cat was now very weak and seemed about to die, it was determined to make the diabetic puncture. The cat was therefore laid prone, a cut made through the skin over the occipital protuberance, and the chisel applied immediately underneath this. After dividing the occipital bone the chisel was passed in a direction downwards and forwards so as to cut the line made by joining the two auditory meatus. The chisel was pushed on until it met with the basilar bone, and was then withdrawn. Operation was over at 12.30. Before the operation the cat had languidly taken a little milk; urine passed during the operation; though highly jaundiced, gave no reaction with Trommer's test. At 2.15 urine pressed out of the bladder gave no reaction with Trommer's test. July 4, cat still alive; urine gave no reaction with Trommer's or Moore's test. July 7, the cat died in the night between July 5 and 6. Examined today at two o'clock. Much more peritonitis than in any other of the experiments. The upper and under surfaces of the liver covered with a layer of exudation; the fluid in belly turbid. Complete obstruction of the gall ducts. No microscopical examination of the liver. *Analysis of the foregoing cases.* As regards time of death: 16 cats had their bile ducts tied; three of these died from prolapse of the bowels, two on the fifth, and one on the seventh day after the operation. Another cat had the diabetic puncture done on the sixth day after the operation, and died probably on the ninth day. So in twelve only was the natural progress of events interfered with. Out of these twelve, two died on the third day, two on the fourth, and one on the eighth, tenth, fourteenth, sixteenth, eighteenth (but in this case only the left hepatic duct was tied), and twentieth days respectively. Two cats were killed on the twenty-seventh and twenty-ninth days; the bile had found again a natural passage to the intestines. This curious result was noticed by Brodie, one of the earliest to make the experiment of ligature of the bile ducts. (Brodie, 'Quarterly Journal of Science, &c.' London, 1823, vol. xiv., p. 344.)"

Then on page 177: "The cause of death in these creatures is obscure. Blondlot, and many other observers, attribute it to peritonitis. Blondlot gives a distinct cause. He says that the ligature eats through the bile duct; the bile is thus poured into the peritoneum (Blondlot, '*Traité analyt. de la Digestion.*' Paris, 1843, p. 174.) In my own cases, I did not notice this in one instance, although in all cases the bile ducts were carefully dissected out. The ligature was around the duct in every case. Peritonitis can scarcely be set down as a valid cause. In all, the marks of peritonitis were so slight and so limited to the part between the under surface of the liver and the duodenum, that it was impossible to attribute the fatal result to this local disturbance. In none of my cases too was the suppuration from the wound at all considerable. It had usually ceased at the end of the first week."

Then in the next paragraph he says: "Leyden seems to think that it is the addition of the jaundice to the peritonitis which kills the animals. Experiment XV. would seem to discredit this explanation. Here only one branch of the hepatic duct was tied; yet the cat died on the eighteenth day, although no jaundice had been set up at the time of death. Again, dogs, in whom biliary fistulæ have been set up and again closed, live for months, notwithstanding an intense jaundice. I should be far more inclined to attribute the cause of death to the changes which take place in the liver." I do not think I need go on with that; there is anough to show that he disagrees with him on that point.

I will next quote from the British Medical Journal, October 23, 1875. Experiments on the Biliary Secretion of the Dog, by William Rutherford, M.D., F.R.S.E., Professor of the Institutes of Medicine in the University of Edinburgh, and M. Vignol ———. I do not wish to read much out of this; I bring it forward partly to show that these painful experiments upon animals are still carried on; at all events this appears to be incomplete, and it is of the date I have just read. So that it shows that these men do not care to wait for any opinion of the Royal Commission; but they are going on on their part, and in a way which we esteem contrary to the Act of Victoria.

"Two years ago, Röhrig," and so on, "performed a number of experiments on the effect of various substances on the biliary secretion," and so on. "Method of experiment. All our experiments were performed on dogs that had in nearly every instance fasted about eighteen hours. After paralysing the animal with curara, and establishing artificial respiration, we opened the abdomen in the linea alba, and tied a glass cannula in the common bile duct, near its junction with the duodenum. To the end of the cannula which projected from the abdomen, we attached a short india-rubber tube, and to the end of this again a short tube of glass, drawn to a narrow aperture, so that the bile might drop from it. The gall-bladder was then compressed, in order to fill the whole tubing with bile and the cystic duct was clamped to prevent the return of the bile to the gall-bladder, and so compel all the bile secreted by the liver to flow through the cannula. The wound in the abdominal wall was then carefully closed, and in all our later experiments the animal was thoroughly covered with cotton wool, in order to quickly restore it to its normal temperature."

Then page 2: "As is well known, curara is of great value in such experiments, for by paralysing voluntary movement, it prevents the irregular outflow of the bile, which certainly follows irregular contraction of the abdominal muscles; and if care be taken to give doses just sufficient to produce this paralysis, the biliary secretion is not apparently affected; but if too much be given, the heart is rendered weak and irregular, and the secretion of the bile diminished."

Then I go on to page 3: "It therefore appears that in the progress of the experiment the composition of the bile remained almost precisely the same. This is remarkable, seeing that the animal had been deprived of water for so long a time" (there he tortured the animal, or something like it) "and, moreover, seeing that the entrance of the bile into the

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intestine had been cut off. It should be mentioned that in taking the bile secreted near the beginning of such experiments for analysis, we were always careful to eliminate that which had been expressed from the gall-bladder into the cannula."

Then there is additional matter on the same subject in the British Medical Journal, October 30, 1875. I have a pencil memorandum from the gentleman who sent me this. He says, "Among the allotments of the Scientific Grants Committee of the British Medical Association is Professor Rutherford's Researches on Biliary Secretion, 25*l*." Then at page 7, "Experiment 15. Dog weighing 9·5 kilogrammes. In this experiment it was proposed to test the effect of aloes on the liver when well nigh exhausted; accordingly at the sixth hour of an experiment on a dog that had fasted the usual period of 18 hours, twenty grains extract of Socotrine aloes in 5 c.c. of water were injected into the duodenum (*a*, figure 15), and this dose was repeated in half an hour. The secretion of bile was increased, but the effect was not very marked; nevertheless, the result is noteworthy, seeing that in this case there was a great secretion of bile during the first four hours of the experiment (fig 15)."

Then the last experiment which is recorded here, but the paper does not seem to be finished, is experiment 16, on injecting rhubarb.

5566. If there are any further passages in published works to which you wish to refer us, will you, if you please, simply give us the references to them now, and then proceed to say anything that you have to say?—I would refer to the Hand Book for the Physiological Laboratory, edited by Dr. Burdon-Sanderson, 1873, and the pages to which I would call attention are pages 210, 212 (rabbit, &c.), 238 (spinal cord), 245, 308, 403 (dogs and cats recovered from chloroform), 404 (frogs), 409 (frogs), and 417 (pigeons). Then I would refer to "An Inquiry into the Process of Nature in Repairing Injuries of the Intestines, illustrating the treatment of penetrating wounds and strangulated hernia, by Benjamin Travers, Demonstrator of Anatomy at Guy's Hospital, &c. London, 1812." We put in that book as containing cruel experiments to animals generally. And then, "The Wrongs of the Animal World; to which is subjoined the speech of Lord Erskine on the same subject; by David Mushet, Esq., London, 1839." The pages to which I would refer are pages 191, 228, 229, and 201 and 202. In regard to the fact that these so-called experiments are perpetrated at the present day, and to a very considerable extent, though some people deny it, we wish to refer to what was done at Norwich, in the case of that Frenchman Dr. Magnan. It was done publicly, and there was a prosecution, or rather an attempted prosecution; he somehow or other managed to escape. There was a miscarriage of justice there, as we should say, and he was not apprehended. I do not mean that it failed through anything wrong in regard to the magistracy, but simply that he was not brought before them. Then I would refer to "The Physiological Pathology and Treatment of Asphyxia, by James Phillips Kay, M.D., formerly President of the Royal Medical Society, Edinburgh; London, Longman, 1834." That book we put in generally in the same manner. Then the next book is "Physiological Researches, by Sir Benjamin C. Brodie, Bart., London, Longman, 1851." We put that in generally. There is something which I should now like to refer to, the opinion of Alexander Chalmers in regard to the Abbe Spallanzani; I think it is in the Biographical Dictionary.

5567. You must not rely on our having printed in the Blue Book everything which you have read to us or to which you have referred us in published books; the Commissioners will exercise their discretion with regard to that. I will now ask you, have you said all that you wish to say to the Commission?—No.

5568. What is the nature of that which you have further to address to it?—It is to show the cruelty and inutility of these practices.

5569. You have put in the references to the various books that you wish us to consider?—As I have gone on I have done so.

5570. You have exhausted that portion of the subject?—I do not know that I have quite.

5571. Do you know that you have not?—I could not answer decidedly at the present moment; I should like to refer.

5572. Then you mean that you are not able in the course of this present examination to conclude your evidence in that respect?—I think not quite.

5573. Have you any observations that you are desirous of offering before to-day's examination closes?—No. I would reserve them till another day. I am tired now; my health is not strong, and I am not accustomed to read like this.

5574. At any rate to-day you are not prepared to address any further observations to the Commission?—If it was a matter of necessity, now or never, I should endeavour to do something.

5575. Then we will ask you to proceed now?—Could you not take me on Monday? I make that request.

5676. The arrangements of the Commission do not enable us to meet on Monday?—Or on any future day?

5577. If you have any observations that you wish to address to us now, we wish you to proceed wth them?—I have given my reasons for not doing so. I could hardly do justice I think to the Society and my clients if I went on now.

The witness withdrew.

Adjourned.

Saturday, 11th December 1875.

PRESENT:

THE RIGHT HON. VISCOUNT CARDWELL, IN THE CHAIR.

The Right Hon. Lord WINMARLEIGH.
The Right Hon. W. E. FORSTER, M.P.
Sir J. B. KARSLAKE, M.P.

THOMAS HENRY HUXLEY, Esq.
JOHN ERIC ERICHSEN, Esq.
RICHARD HOLT HUTTON, Esq.

N. BAKER, Esq., Secretary.

Mr. W. B. Carpenter, C.B., M.D.

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MR. WILLIAM BENJAMIN CARPENTER, C.B., M.D., called in and examined.

5578. (*Chairman.*) You are Registrar of the London University?—Yes.

5579. I think you were formerly Lecturer on Physiology at the Bristol Medical School, and afterwards at the medical school of the London Hospital?—Yes.

5580. And I need not ask you if you are the author of the well-known book upon the Principles of Human Physiology, and of a book upon the Principles of General and Comparative Physiology?—Yes.

5581. And I believe of several articles in the Cyclopædia of Anatomy and Physiology?—Yes.

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5582. I doubt not therefore your attention has been directed to the subject into which we are instructed to inquire?—Yes. I have taken from a very early period a great interest in the progress of experimental physiology.

5583. Are you of opinion that as it is conducted in this country, to which we limit our inquiry, it is, generally speaking, conducted by persons of a sentiment of humanity towards animals?—I know nothing to the contrary.

5584. You believe that to be so?—I believe it to be so.

5585. If it should be at the present moment rapidly extending, would that in your opinion be a reason why some general regulation should be arrived at for its government?—If any such regulations could be efficiently applied. I have a very strong objection to any legislation that cannot be thoroughly carried out,—that can be evaded.

5586. You think that any law which was adopted for the purpose should be one which it was possible to carry into effect, and which it was intended really to carry into effect?—Yes.

5587. And if such a law as that could be adopted without interfering with the progress of physiological science, are we to understand that it would have your approval?—Quite so.

5588. Have you considered at all the details of the various measures that have been proposed?—I cannot say that I have. I merely know their general idea of licensing places and persons, or places or persons.

5589. You are aware that some of the greatest names in science are associated with a Bill which was introduced into the House of Commons in the last session by Dr. Playfair?—Yes.

5590. If you have not yourself considered the details of that Bill I will not examine you upon the details, but will only generally ask you whether in your opinion the names to which I have referred are names that would carry generally the confidence of persons in medical science?—I am not sure that they have considered all the points and difficulties which would naturally arise.

5591. But I ask you whether the names are the names of those persons who are most likely to carry the confidence of the scientific world in this country?—So far as I recollect them they were, certainly.

5592. I understand that you have not yourself considered the details of that Bill?—No.

5593. Therefore we must only ask you hypothetically. If they have considered the details of it you would attach great weight, I presume, to their opinion?—Not altogether to the exclusion of my forming an independent opinion.

5594. But I presume that you would form that independent opinion rather after having considered the details of the measure than before, would you not?—Certainly; but as far as I understood the details of that measure (I cannot say that I carefully studied them, because I saw very soon that the Bill was withdrawn) there would be to my apprehension very grave difficulties in working it effectually. For instance, I could not see how it could exclude or take cognisance of the performance of physiological experiments in private houses; or how it was possible to draw any definite line between the performance of an experiment that no sensible man would object to, and the performance of a cruel and painful experiment. There is a gradation from one to the other; and my great difficulty in the consideration of this subject has been, to find how any definition can possibly be framed to apply to the class of experiments which it is desired to restrict, without interfering with the class of experiments which I think are perfectly legitimate without restriction,—experiments which any man ought to be able to perform in his own house without interference from legislation.

5595. Then, viewing it rather in a general way, you rather despair of it, than come down to the particulars of these special measures and say that they fail in doing it?—My position is this, that in view of the gradation of which I have just spoken,—between, for instance, putting a frog under the microscope to look at its circulation, which I should say no one ought to interfere with any man showing to his children in his own house, and what are properly called physiological experiments,—I do not see, and I never could see, where and how to draw the line.

5596. Then, as far as it has yet occurred to you, it is impossible that a law should be passed which could accomplish the object which we have in view?—A law that should be really effectual without unduly interfering with the liberty of the subject.

5597. (*Lord Winmarleigh.*) When you state that there would be a difficulty in reaching the practice in private houses, does not that observation apply to a number of other things that are under legislation at the present moment?—I have contrasted it in my own mind with the Anatomy Act. The Anatomy Act does not prevent anyone from dissecting a dog or a cat in his own house; it simply applies to the dissection of the Human body as a whole; and no person may legally perform dissection of a human body in any other place than in a licensed building and under authorities who have a license. But every medical man does perform dissections of parts of human bodies in his own house; every medical man who goes to a post mortem examination, and finds something of interest to him, brings it home to study more carefully. That is a case in which it seems to me that a broad general line may be drawn; because there is a pretty clear common sense distinction between the two things, the regular practice of dissection for the education of students in a medical school, and the dissection of small portions of a body which a medical man may bring home to examine more carefully. The Inspector of Anatomy, who looks strictly after the one, would never dream of interfering with the other. But I cannot see how such a line can be drawn with regard to physiological experimentation.

5598. I think you misapprehend the object of my question. You stated that one of your objections to legislation was that you could not reach this practice in private houses. Now I will take a subject in strict connection with the one which we are investigating; I will take the subject of cock-fighting, for instance. It is contrary to law; Parliament is not prevented from legislating very usefully against cock-fighting, by the idea that you could not reach it in private houses; it is illegal in private houses, and we trust to the feelings of persons that it will not be practised in private houses when it is illegal. Does not that remove the difficulty that you have expressed with regard to legislation reaching private houses?—I think not, for I do not see how it could be possible for legislation to reach it in a private house. Cock-fighting is a thing which is to a certain degree notorious; the cocks cannot be brought there without the knowledge of others, and their noise is heard. Allow me to give a nearer parallel, namely, the action of the Maine Law, which I have studied and thought of a great deal, in the countries where it prevails. I know perfectly well that it is impossible by any Maine Law to prevent drinking in private houses.

5599. But does it not check it?—No, I believe not; I believe rather the reverse.

5600. I will return to cock-fighting. It is a known fact that there are instances still where cock-fighting is carried on by stealth in private houses; but it has been very much checked through the country, has it not, by the legislation which has taken place on the matter?—I presume that it has, but I trust very much more to a healthy public sentiment on the subject.

5601. (*Mr. Erichsen.*) You have as yet expressed no opinion as to the importance of vivisectional experiments upon animals as a means of scientific investigation?—I have no hesitation in saying that it is of the most fundamental and essential importance, and that it is impossible that physiology can be studied without it; and that if anyone will take the trouble to go over the history of physiology for the last half

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century, for example, to go no further back than the researches of Sir Charles Bell, he will see that the whole fabric of our accurate knowledge of physiology depends upon experiment. And having been asked that question, I would take leave to make a statement in regard to an assertion which has been made, that Sir Charles Bell himself regretted that he had made physiological experiments, and that he declared that he had done so merely to satisfy others, and that he could have arrived at all at which he did arrive by anatomy only. Whether Sir Charles Bell made that statement, or not, I do not know; but it is perfectly notorious to all those who have studied the history of his discoveries, that he was led into his early mistakes, which had to be corrected by other physiologists, simply by trusting too much to anatomy; he formed certain ideas on the basis of anatomical research, which ideas had to be corrected by experiments made by himself and by others. It is an entirely erroneous notion to suppose that Anatomy can lead by itself to any certain Physiological deduction, without putting that deduction to the test of experiment.

5602. And with regard to the importance of physiological experiments as a means of demonstration to students, what is your opinion?—I think that a very clear line may be drawn in regard to demonstrations. I think that no experiments involving animal suffering need be made for the purpose of demonstration of physiological facts and doctrines already perfectly well ascertained. My own conviction has long been that that is not only unnecessary, but that it is morally injurious. But on the other hand there are a very large number of physiological experiments which may be made, and ought to be made, in my opinion, in the presence and for the instruction of medical students, that can be made without animal suffering. I think we have as much right to destroy the life of an animal, —to cut off the head of a turtle, for example, and to exhibit the great phenomena of reflex action upon its dead body, as we have to kill it for food.

5603. (*Mr. Hutton.*) Would you put any limit on the painful character of the experiments to be made for scientific purposes, or would you justify the infliction of any amount of pain for a sufficient scientific purpose?—I should certainly justify it for a well considered purpose, if that pain could not be avoided. There are certain cases in which the use of anæsthetics would neutralise the result; and I think that the history of science shows that some of those experiments have been of very great importance. The whole series of experiments made up to the time of the use of anæsthetics, by Sir Charles Bell, Dr. Marshall Hall, Dr. Waller, and others, were excessively painful experiments; but I should hold them fully justified in making those experiments. I witnessed a large series of experiments by my friend Dr. John Reid, in which a considerable number of animals were operated on at once; and the very valuable results which he worked out, were worked out entirely through his having used several animals, and not being satisfied with experiments on one animal. I may give this as an illustration:—At the time when Dr. Reid began his experiments, the doctrine was that the division of the *par vagum*, the eighth pair of nerves, destroyed digestive power. This was a very important physiological conclusion, but Dr. Reid had reason to suspect its correctness; and he found that it depended upon this, that the immediate effect was always to suspend the digestive power, that is, to destroy it for the time; but by making the experiment upon several animals, he found that while some of them sank under it, others survived; and in those that survived it, the digestive power returned without any reunion of the nerve. Now the establishment of that fact was one of fundamental importance in physiology, because it showed what is the degree of control which a nerve has over a secretion.

5604. My question was rather whether, quite admitting the scientific value of the experiment, you would scruple to inflict any amount of protracted agony, supposing that were necessary for the purpose of a good scientific result; whether in fact on moral considerations you would put any limit on the amount of agony to be inflicted?—I do not see where you are to draw the line. A humane man will shrink, no doubt, as Bell did in the first instance, from inflicting animal suffering; and it will be a question in his mind between the probable benefit to be derived and the value of the result. Now I may give this illustration. Dr. Brown-Séquard, who is one of the most humane men I know, has inflicted more animal suffering, probably, than any man in his time, unless it be some of the recent German experimenters; but his labours have extended over thirty or thirty-five years, and he has worked out, as every physiologist knows, results of the very greatest importance, by means of experiments which were often protracted through long periods. One of the most valuable of these, in its practical bearing, was the section of the spinal cord. At the time when he took up his inquiry, it was the received doctrine in physiology that the division of the spinal cord in the back would necessarily destroy life. Now this had a very important bearing upon the treatment of patients who suffer from accidental injuries to the spine. Cases of that kind are very common in the London hospitals; cases where men fall from masts of ships and strike the back, and have complete paralysis of the lower limbs; and every surgeon knows that it is excessively difficult to keep such patients alive for any considerable length of time, and that nurses suppose that they must die, and will neglect them. Brown-Séquard desired to ascertain if there was any necessarily fatal result from that experiment. He performed this section of the spinal cord in a great number of animals of different kinds—frogs, rabbits, guinea-pigs, and birds; and the immediate result of course was to produce complete paralysis of the hinder limbs; but by the most careful and tender nursing of these animals, he was able to keep them alive, warding off from them the injurious influences which would have otherwise caused gangrene of the limbs and death; and in that manner, carrying on this nursing through months which must have been to a certain degree months of suffering to the animals, he established the position that the spinal cord may be completely restored and the power of the limbs completely regained. Now this complete restoration of the spinal cord is not only a physiological fact of the very greatest interest, but of the greatest practical value to Man; because it encourages all the attendants of a sufferer under these circumstances in doing exactly what Brown-Séquard did with his animals, namely, giving the most careful attention to all the minutiæ which should prevent, remove, and antagonise so far as possible the consequences of the injury, in the hope, which has been now realized over and over again, of complete recovery.

5605. (*Sir John Karslake.*) May I ask whether in those cases the section of the spine was done under anæsthetics?—These experiments were performed before the introduction of anæsthetics; but the section itself is a matter of comparatively small moment; it is the after suffering of the animals which is the most painful to a humane mind; to see these poor creatures going on week after week, and month after month, in an artificially induced paralysis.

5606. (*Mr. Forster.*) Are those experiments still being made?—I should consider it quite unnecessary to make them now.

5607. You do not know whether they are being made by Dr. Brown-Séquard?—I doubt whether they are, because they require so very much trouble. The constant watchful care of the animals which is necessary is a thing which no man, not a thorough enthusiast, as Brown-Séquard is, would have carried on.

5608. Do you know on how many animals he tried the experiment?—I should think 40 or 50 at least.

5609. Now was it at all necessary to try it on so many?—I think so quite, for the same reason that John Reid found it necessary. In order to establish the fact that survival may take place at all under such cir-

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cumstances, it is important to experiment upon several; otherwise, as one will die and another will die, you may come to the conclusion that death is inevitable.

5610. But do you at all know out of those 40 or 50 how many survived?—I should think about half from his description of the experiment.

5611. Then why should it be necessary to try it on so many after having ascertained that several did survive; after having ascertained, for instance, that five did survive, why should it be necessary to try it on more?—It was important to experiment on several different kinds of animals; for instance, it might be said that a frog would survive, but that a warm-blooded animal would not.

5612. (*Chairman.*) What was the use of proving that a frog could survive such a painful experiment, if no reasonable deduction could be drawn from that fact with reference to man?—In such an experiment as that, there would be no reasonable deduction from the frog; but the deduction may be drawn much more reasonably from a rabbit or guinea-pig or bird.

5613. (*Mr. Forster.*) Then that would seem to make the sufferings of the frog useless, would it not? —To establish the fact of regeneration in the frog would be the first step. I think a frog would suffer very little; my belief is that frogs have extremely little perception of pain.

5614. (*Mr. Huxley.*) Is there any evidence that a frog would suffer at all after the spinal cord was divided, because the consequences that take place in the higher animals which give rise to pain do not take place in a frog, and after the first operation I presume there is no evidence of pain?—I do not think there is any evidence of pain.

5615. (*Mr. Hutton.*) I gather that you would deny that there is any limit on the suffering that ought to be inflicted for a well-conceived experiment?—If that experiment is one which, after careful consideration of the results which may be expected from it, is likely to lead in any decided degree to the discovery of physiological truth, whether or not the immediate application to the alleviation of human suffering is apparent. All science shows us that the results of scientific truths in benefit to mankind are very often long postponed. The discovery of chloroform, for example, is a very characteristic instance of that. Chloroform for many years was a mere scientific curiosity; it was a compound not supposed to have any relation to human welfare.

5616. I see an experiment narrated in your own work on physiology, as to which I should like to know whether you think it really was a desirable one to make. I find this stated: "The introduction of a "little boiling water threw the animal at once into a "kind of adynamic state, which was followed by death "in three or four hours; the mucous membrane of "the stomach was found red and swollen, whilst an "abundant exudation of blackish fluid had taken "place into the cavity of the organ." It is not one of your own experiments, but one of which you are there narrating the results. Now do you not think that that might have been argued as one of the most certain inferences from the well-known facts of human experience, and that it was quite an unnecessary experiment to make?—That which you have just read is probably taken from a late edition of my book.

5617. It is the seventh edition by Mr. Power, page 129?—It is not an experiment that I am acquainted with. I have so far given up the study of Human Physiology, that I have really not kept pace with the inquiries to which that experiment relates.

5618. (*Mr. Forster.*) Then this experiment was not published by you?—No. I would not give an opinion upon it without knowing the purpose of it.

5619. (*Mr. Hutton.*) It is published in your book, but not by you?—Not by me.

5620. It is not a thing on which you would wish to offer an opinion?—I should certainly not wish to offer an opinion unless I knew the object of the experiment, and the ideas under which the experiment was made. But I might take another illustration, the study of the effects of poisons upon animals, the corrosive poisons, for example. It is essential to the study of Toxicology, as relating to Man, that most painful experiments should be made upon animals.

5621. I was not in the least denying that, but simply asking this. You have expressed the opinion that experiments are not usually made which are needless, and I was putting the case of an experiment that I thought was needless. Now, I see in a paper which has been presented to the Commission various experiments on gluing animals together, that is to say, removing the skin from two different animals, and binding them closely together, so that a new membrane forms which is common to both, and they are then by careful treatment enabled to live until they grow together, in fact. Now should you regard that as an experiment which has the slightest scientific value, or which is justifiable morally? Clearly it must involve the greatest possible misery to the animals so artificially united?—I should be sorry to answer offhand in regard to such an experiment, unless I knew the objects which the experimenter had in view. It might be that he had conceived a plan which might be of considerable importance in surgery, bearing on the question of how far a large and obstinate ulcer, for example, might be cured by endeavouring to induce the union of its surface with the surface of another animal. Every surgeon knows that the treatment of these large ulcers is one of extreme difficulty, and that the plan has been employed in recent times of taking small bits of skin from another part of the same individual, and planting them, as it were, upon the ulcer. I think it possible that these experiments were intended to test the question, whether the skin of another animal might not be so employed, and whether a considerable amount of human suffering might not be saved by transplanting the skin of another animal. That is what occurs to me as the possible object of the experiment.

5622. Surely that is a totally different experiment from artificially uniting the animals, so that they have to live together. To remove a piece of skin from one animal to another is a totally different experiment from the one which I referred to?—The first question would be, whether under the most favourable circumstances union would take place in such transplantation? A piece of skin completely removed might very probably slough away, and not continue to live.

5623. (*Mr. Huxley.*) Is it not the case that such experiments as those which Mr. Hutton has just been describing have recently been undertaken in this country for the purpose of putting that well-known doctrine of Mr. Darwin of pangenesis to the test; is that within your knowledge?—It has not come under my knowledge. As I said, I have not kept up my knowledge of experimental physiology of late years, excepting in one or two departments.

5624. (*Mr. Forster.*) Do you not think that there may be some danger that a physiologist would be inclined to try experiments, I mean painful experiments, simply with a sort of discovering idea, to find out what will happen, without having any definite notion of producing a result which would bear upon some question affecting life or pain?—I am quite sure that that has been the case, and is the case.

5625. And would you not consider that that is a thing which is open to very great objection?—Certainly.

5626. (*Mr. Huxley.*) Is that the case within your knowledge, because physiological experimentation nowa-days is so troublesome and complicated a thing that, so far as I have known physiologists, they do not generally undertake an experiment without some definite purpose. Of course if it is within your knowledge, I have nothing further to say?—My impression is that a very large number of Magendie's experiments certainly were made simply in this way, performing a certain section and then looking out for results, not with any distinct conception of what the object of that experiment was.

5627. Then it is the case that Magendie's experi-

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ments were made 40 years ago, and that since that time experimental physiology has completely changed front, if I may so express it. Men now have far more definite objects and much clearer views in respect to living operations than they had then?—My impression is that there will always be a tendency of the kind referred to, arising, I may say, out of an imperfect scientific conception, on the part of a class of students having no particular objection to the performance of such experiments. I have myself seen in certain instances, a perfect callousness to animal suffering, before the introduction of anæsthetics. I will not mention names, but I have seen a callousness which very strongly repelled me; and this when important experiments were being performed. But that, I think, does not constitute any adequate reason against the performance of well-considered experiments with a definite object.

5628. From your acquaintance with physiological science in this country, may I ask whether it is your opinion or not that physiologists as a rule, if they had the alternative before them of performing experiments with pain, or removing it, would choose the alternative of removing it?—Most certainly, as far as I am acquainted with the present state of things, they would choose the alternative of removing it.

5629. That taking the English physiologists with whom you are acquainted, they would look upon it as a matter of duty to do so?—Certainly.

5630. You mentioned just now Dr. John Reid. I think you were perfectly acquainted with him?—Very intimately.

5631. Have you had brought under your notice statements that have been recently circulated respecting Dr. John Reid, holding up some expressions of his during his last illness, when, as I need not tell you, he was suffering from terrible agony, with the intention of showing that he considered those sufferings a judgment upon him for his experimentation upon animals?—I would not say but that he might have used some such expressions in the agony he experienced during the latter part of his life; he suffered under one of the most painful diseases which can affect a man, cancer of the tongue, and in the most severe form; but I have letters from him, written within a few weeks of his death, in which he refers to his previous work with the greatest gratitude at having been permitted so far to complete his physiological researches, and to establish by their means truths of great scientific importance.

5632. (*Mr. Hutton.*) May I ask this question? We have had experiments described to us in which dogs have been subjected for nine hours to an amount of suffering that we could not quite determine, because they were under the influence of curari, and we do not know what the influence of curari is, simply for the sake of testing the action of different drugs, rhubarb and so on, on the liver, on the secretion of bile. Now is that, do you think, a sufficient end for the infliction of severe suffering, supposing the suffering inflicted was severe, for that length of time?—I should say so certainly, because the question of the action of the liver, and the function of bile in digestion, and the influence of different remedies upon the secretion of bile, is one of the most important inquiries in its relation to the whole of practical medicine.

5633. And do you suppose that if the drug was introduced into the intestine, and did not go through the ordinary process of digestion, you could rightly infer from the effects so produced what would be the case if it were introduced into the stomach, and did go through the ordinary process of digestion?—Digestion does not take place in the stomach alone, but in the whole of the intestinal canal; and it is very important indeed to determine the place at which the different portions of that digestive process take place, and the effect of the secretion of bile upon its successive stages, in the stomach, in the small intestine, and in the large intestine, and the effect of various medicinal agents upon each of those stages. Therefore for certain purposes I should say that it was quite right that these drugs should be introduced into the intestine, and should not have gone through the stomach. I am quite aware that this is a subject upon which some very able experimenters have been engaged, one particularly with whom I have conversed many times on the subject; and I believe that these experiments, of which I think an account was given at the last meeting of the Medical Association at Edinburgh, are felt in the profession at large to be of the greatest value in leading towards (I do not say that they have yet entirely succeeded) a definite guidance in the administration of remedies affecting the secretion of bile and its operation in digestion.

5634. Are you prepared to express any opinion as to whether those experiments might not have been made, with due allowance for the effect of anæsthetics, under anæsthetics?—I think that they would probably have had to be made under both; because the state of the nervous system is one that has a most important bearing on the secretive action; and it would be impossible to say, from the result of experiments under anæsthetics, what would be the result when the nervous system was in full activity.

5635. (*Mr. Huxley.*) You mentioned just now, in terms which I was glad to hear, Dr. Brown-Séquard. You were acquainted with Dr. Brown-Séquard when he was in London I think?—I have been acquainted with him for the last 30 years.

5636. But you knew him in London, did you not?—Long before he came to London.

5637. You are aware that during his short stay in London he had a very large practice?—Yes.

5638. He acquired a great reputation among persons suffering under nervous disorders?—Yes.

5639. And his practice in your opinion, I doubt not, justified that reputation?—Yes.

5640. His special power of treating nervous disorders arose entirely from his intimate acquaintance with the physiology of the nerves, did it not?—Yes.

5641. That intimate acquaintance being derived from experiments?—Yes. I may mention that Dr. Brown-Séquard first came to me very soon after Dr. John Reid's death; he was in London at that time, certainly 30 years ago; and I then said to him, "Your "mode of experimentation seems to me more that of my "late friend Dr. John Reid than that of any other "physiologist that I am acquainted with," and he said, "You could not pay me a greater compliment."

5642. In any legislation upon this question, I presume you would think it very desirable to draw a clear distinction between experimentation upon living animals so far as they are sensitive, and experimentation upon living animals generally?—Certainly.

5643. Because suppose it were enacted that in certain cases experimentation upon living animals should not take place, a strict interpretation of that phraseology would of course prevent experimentation upon animals in which general life still remains, but without any life of the nervous system?—Certainly.

5644. And I presume you do not think it desirable to prevent experimentation of that kind?—Certainly not. It is there that the difficulty of drawing the line seems to me to become the greatest obstacle to legislation.

5645. (*Chairman.*) As Registrar of the London University, will you tell us generally what is the relation of the London University to the Brown Institute?—The Senate of the University of London are Trustees, under the Will of Mr. Brown, of the Institution for the establishment of which he left a large sum of money to be administered by the Senate.

5646. What were Mr. Brown's general objects?—The provisions of his Will were to the effect that this sum, the residue of his fortune, was left for the establishment of an Animal Sanatory Institution; the special objects of which were to be the investigation and study and, as far as may be, the cure, of the maladies and injuries to which any quadrupeds or birds useful to man may be subject.

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5647. Have questions arisen in the Senate of the University with regard to the present administration of that Trust?—Yes.

5648. Are those questions finally disposed of?—The first question is, I believe, finally disposed of. It was this: Mr. Brown provided by his Will that animals shall be received as patients into the institution; he also made special provision for the purchase of diseased or injured animals or their carcases for pathological study, and he left the direction of that study to a Committee to be appointed by the Senate. It came to the knowledge of a member of the Senate that the artificial induction of disease was being practised,—the disease called pyæmia, for example. It was urged that this practice was inconsistent with the provision of Mr. Brown's Will, that kindness to the animals committed to the charge of the Professor-Superintendent should be a general principle of the institution. It was replied by other members of the Senate, that this principle had reference to the animals received as patients, and that the procedure objected to was one which in the present state of animal pathology it was necessary to take, for the complete investigation of disease, although not provided for specially in Mr. Brown's Will; so that these inquiries stood upon a different footing from the treatment of animals received as patients.

5649. Now upon that were legal opinions taken?—Upon that a legal opinion was taken. Two cases were drawn out, one under the direction of the member of the Senate to whom I have referred, and the other by myself chiefly as representing the Committee of the Brown Institution. These were placed before Mr. Charles Hall, who is now Vice-Chancellor. It was agreed on both sides that his opinion should be considered as decisive; and Mr. Charles Hall gave it as his opinion that the distinction was a valid one, between the animals received as patients, and the animals upon which this pathological inquiry was being prosecuted; and that the concurrent testimony of pathologists, that such artificial induction of disease has now become an essential part of the investigation and study of the diseases of animals, fully justified the carrying on of this research as a part of the proper business of the Brown Institution.

5650. And upon that opinion the Senate acted?—Upon that opinion the Senate have acted.

5651. Will you tell us the other matter that is in suspense?—A member of the Senate brought under its notice certain statements, which statements I should say had been published, in part at least, in a medical periodical.

5652. Upon that was an inquiry instituted by the University?—The Senate appointed a committee.

5653. Has the report of that committee yet been dealt with by the University?—It has been placed before the Senate, but the Senate has postponed the consideration of it.

5654. Until after the report of this Commission?—Until after the report of this Commission.

The witness withdrew.

Adjourned to Monday next at 2 o'clock.

Monday, 13th December 1875.

PRESENT:

THE RIGHT HON. VISCOUNT CARDWELL, IN THE CHAIR.

The Right Hon. Lord WINMARLEIGH.
The Right Hon. W. E. FORSTER, M.P.
Sir J. B. KARSLAKE, M.P.
THOMAS HENRY HUXLEY, Esq.
RICHARD HOLT HUTTON, Esq.
N. BAKER, Esq., Secretary.

THOMAS LAUDER BRUNTON, M.D., D.Sc., called in and examined.

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5655. (*Chairman.*) You are Lecturer on Materia Medica in Saint Bartholomew's Hospital, are you not?—Yes.

5656. And you have paid particular attention to the subject which is referred to this Commission?—Yes.

5657. Generally your attention has been directed to physiology, and under the head of physiology to the department of pharmacology?—Yes, more especially.

5658. In pursuing your researches do you make any considerable use of experiments upon living animals? Yes, very considerable use.

5659. In the way of administering drugs to living animals?—Yes.

5660. I suppose, therefore, that painful operations upon living animals do not fall very much within your particular experience?—Operations do fall within my experience, because in trying to make out exactly the action of the drug, one is obliged not merely to give it to the animal and watch the general effect, but also to perform certain experiments, certain operations, in order to watch the effect of the drug before and after the performance of such operations, and thus to ascertain the action of the drug upon particular parts of the body. In this way we make out precisely where and how the drug acts.

5661. Then operations are performed by you upon living animals for the purpose of exposing to view the particular portion of the interior of the animal on which you wish to witness the effect of the drug?—Sometimes.

5662. Are those operations performed under an anæsthetic?—Yes, almost always.

5663. And if there are any exceptions is that for some special reason?—Always.

5664. Then do I rightly understand you to say that regard is had to the probable suffering of the animal, with a view to prevent it altogether or at least as much as possible?—To prevent it as much as is at all possible.

5665. Then as a matter of fact is much pain in the way of operations inflicted upon living animals in the course of your researches?—No; the chief pain is simply due to the action of the drugs themselves.

5666. The object being to test the operation of the drug upon the lower animals instead of testing it upon man?—Yes.

5667. Has that particular branch of research been very much developed of late?—I will not say precisely the number of years, but lately it has developed very rapidly and made very rapid progress.

5668. Can you give us any instances in which the making of those operations upon living animals has been very conducive to the welfare of the human race?—We have had several new remedies within the last few years which have proved very serviceable, and I think I may say have been introduced into our pharmacopæia solely on account of their action having been previously ascertained upon the lower animals.

5669. Sir James Paget told us that in consequence of some observations of yours upon nitrite of amyl that fatal and distressing malady *angina pectoris* has been reduced more within the domain of medical control than it had been previously; that is so, is it?—Yes, that is so.

5670. Can you mention any other instances?—The

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introduction of chloral, which has now become very useful, and is a very commonly employed hypnotic, is an instance. Then we have the introduction of atropia as a remedy for salivation. We have the introduction of apomorphia as an emetic. We have also the introduction of the Calabar bean into medicine. Those are the chief instances that occur to me at present. But there is also one other instance in which such experiments have proved very useful, namely, the discovery by Claude Bernard of the method by which carbonic oxide produces poisoning. When a charcoal brazier is lighted you get two sorts of gas produced, carbonic acid and carbonic oxide. Now if a person is suffocated with carbonic acid, as is the case if a man falls into a brewer's vat, for instance, it is quite sufficient in order to resuscitate him, simply to keep up artificial respiration for some time. With carbonic oxide, on the contrary, this is not sufficient, because Bernard discovered that carbonic oxide forms a compound with the blood which is incapable of taking up the oxygen of the air in the lungs, so that the blood is for the time rendered useless. This discovery was applied by Hüter, of Berlin, in a case of charcoal poisoning. A man was poisoned by a charcoal brazier one night; Hüter was brought in before the man was quite dead; he kept up artificial respiration, but saw that this was going to be of no service, because the blood was so saturated with this carbonic oxide that artificial respiration was doing little or no good; he, however, knew that if he withdrew some of this poisoned blood and replaced it by fresh blood then the artificial respiration would have its proper effect; he accordingly did so and saved the man's life. That man would certainly have died if Claude Bernard had not previously discovered that carbonic oxide acted in such and such a way upon the blood.

5671. (*Lord Winmarleigh.*) I presume from what you say that that discovery was made by an experiment upon some animal?—Yes.

5672. What animal?—I believe Bernard used various sorts. So far as I remember it was chiefly upon dogs that he experimented. This carbonic oxide has the same general action upon all animals, but if I remember rightly it was chiefly dogs that Bernard used in his experiments.

5673. Could you describe to the Commission what the particular experiments were by which that result was brought out?—The way in which it was discovered was this. Bernard found that when animals are poisoned by carbonic acid the blood is of a dark colour, and that when they are poisoned by carbonic oxide the blood is of a cherry-red colour, and remains of a cherry-red colour. Being struck by this curious appearance of the blood, he set to work to investigate the amount of gases contained in the blood, and in this way he discovered that carbonic oxide formed a compound with the blood and prevented the blood from taking up oxygen as it usually does.

5674. That could not have been discovered without great danger by an experiment on the human being, I presume?—I should say it was quite impossible.

5675. You stated just now that the chief pain inflicted upon animals in your department is by the operation of the drug itself upon the animal, not by the incision or anything else that is done to expose the experiment?—No; because the way in which I perform my experiments is this: I give the animal chloroform, and I do my operations whilst it is under its influence; but then I am obliged to allow the animal to come out of the chloroform in many instances, because otherwise the action of the chloroform would so interfere with the action of the drug that I should not be able to separate them. In some cases I am able to keep the animal thoroughly under the influence of the anæsthetic the whole time, by giving it a large dose of chloral and giving it the drug afterwards. Then I get the two actions combined, that of the chloral and that of the drug; but in those cases I am able to separate the two. I know what the chloral alone would do, or rather I do not know precisely, but I have a pretty fair idea, and then I know what the two together do, and in this way I ascertain the action of the drug; but it is sometimes impossible to do so. One must sometimes give the drug after the animal has come out of the chloroform.

5676. But for the purpose of that particular experiment you have to perform an operation which would be of a painful nature but for the chloroform which you give?—Yes.

5677. You say that you are obliged to keep the animal alive after it comes out of the anæsthesia for the sake of seeing the operation of the drug?—Yes.

5678. Is the incision that you have made for that purpose allowed to remain open?—Sometimes.

5679. And does that cause no pain?—It certainly will cause a certain amount of pain, but, as we all know, if we get a cut on the finger it is the cut itself that a person objects to. For example, in taking off a finger a person would say, "Well, I must have chloroform; I cannot stand having my finger cut off without chloroform." He does not mind the pain afterwards; it is the cutting that he dislikes.

5680. You think then that the animal does not suffer any excruciating pain, at any rate, when recovered from the anæsthesia while you are examining the operation of the drug?—No. I simply judge from myself, or from men who have had operations done. It is the performance of the operation that is the exceedingly painful part, and after the operation is over they do suffer a certain amount of pain, but still it is quite bearable; and I judge that in animals it is the same.

5681. What drug is it in your opinion that creates the greatest amount of pain by its operation upon the particular part on which you wish to make the experiment?—I should think that probably the most powerful thing with which I have experimented is the poison of snakes.

5682. Would you describe to the Commission what is the usual result of your experiment?—The animal first seems to get heavy and depressed, and then vomits; sometimes the vomiting is very violent, and then by-and-bye after a little while you get twitchings in some animals.

5683. Then do you use anæsthetics in that case?—No. I should use an anæsthetic if I were obliged to do any painful operation, any cutting operation, but then I could not use an anæsthetic when experimenting on snake poison, because the anæsthetic would completely mask the operation of the poison itself.

5684. Is that an experiment of very long duration?—Well, the animals generally die in about half an hour.

5685. It does not exceed half an hour altogether?—I think not. I am not quite certain that that is quite as painful as poisoning by strychnia.

5686. What kind of pain is it in that case; is it from convulsions?—In strychnia there are convulsions. It is somewhat difficult to say which will cause the most pain. It is possible that although in cases of poisoning by strychnia during the operation of the strychnia there may be pain, the convulsions on the other hand may not be painful, because it is difficult to separate the appearances due simply to the action of the drug and the appearances which might be produced in consequence of an indirect effect, that is to say, in consequence of pain occasioned by the convulsions.

5687. But you undertake to say that as regards the general run of your experiments they are quite the exceptions where there is any pain necessarily attached to them from the operation?—Yes; quite exceptional.

5688. Do you think that you have given us the principal instances, or are there any others?—I think I have given you the principal ones.

5689. Have you any experiments on hand now with any special object of more than common importance?—Not at present. Some time ago Dr. Fayrer and I were engaged in a research on the action of snake

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poison, with the object of ascertaining whether we could get an antidote to that or not.

5690. Have you ever made any experiments with regard to hydrophobia?—No.

5691. Do you know whether any experiments have been made with a view to the prevention or to the cure of hydrophobia?—There may have been but I am not aware of any. It would be exceedingly difficult to experiment upon that.

5692. (*Mr. Forster.*) Are you a lecturer, may I ask, at St. Bartholomew's Hospital?—I am a lecturer at St. Bartholomew's Hospital.

5693. I have got before me the return that was sent from that hospital to the questions which have been asked by the Commission, and to this question, "State whether, and in what proportion, the animals "are in the state of anæsthesia; distinguish the "case of frogs; and also what anæsthetics are em"ployed?" the answer sent in is, "In all cases of "class demonstration the animals are in a state of "anæsthesia. The animals are killed at the end of "the demonstrations. The anæsthetics employed are "wourali, chloral, chloroform, and ether." Are you in the habit of employing wourali in those cases?—Sometimes.

5694. Do you think it is an anæsthetic?—To a certain extent I think it is. I do not think it is an anæsthetic in the same degree as chloral, opium, or chloroform.

5695. We have had a great deal of evidence before us showing difference of scientific opinion as to whether it is an anæsthetic, but on the whole the balance of evidence in the view of Claude Bernard seems to show that while it stops muscular motion it does not banish pain?—I think Claude Bernard is mistaken.

5696. You observe that the answer which I have just read to you, as it stands, seems to show that for the purpose of preventing pain a drug is given which may not prevent pain at all?—The reasons for believing that Bernard has been mistaken are that if you give a frog wourali and stop the circulation in one part of the body so that the wourali only gets to one part and leaves the other part unpoisoned, pinching the skin of the poisoned part does not cause the animal to kick as pinching it on the unpoisoned part would do. Or if you take two frogs and treat them in the same way, but poison the upper part of one frog and not of the other, you will find that the nerve-centres seem to be to a certain extent paralysed; the wouralised frog does not kick on the application of a painful irritant, although it might perfectly well do so. These facts were discovered by Von Bezold so long ago as 1860, and by Schiff and Lange in 1870.

5697. Without entering into the scientific evidence on one side or the other, I will ask you this question; do you think it is a matter of certainty that wourali is an anæsthetic?—No, it is not a matter of certainty.

5698. In what cases do you employ it in demonstrations?—I very rarely employ wourali in demonstrations.

5699. Will you be kind enough just to look at that answer (*handing the return to the witness*) and tell me to what cases that answer would seem to refer?—To frogs. I do not think that I ever employed any wourali in class demonstrations except in the case of frogs; and in the case of frogs I can demonstrate, and did demonstrate this last spring to my class, that wourali certainly paralyses the sensory nerves as well as the motor.

5700. The answers that are given to the inquiries in that return would seem to give the impression that anæsthetics are not employed at St. Bartholomew's Hospital for the purposes of investigation; because to the question "When not in that state, the reason why not?" the answer is "See above." Do you imagine that ought to be the impression given?—The reason of that is simply this, that I have been absent on the continent, and Dr. Moore I should say probably did not know what to say; I and Dr. Legg are the only persons, I believe, at St. Bartholomew's who do any experimental work for original investigation, and Dr. Moore did not know exactly to what extent I used anæsthetics in my laboratory in original investigation, so that he was not able to give a satisfactory answer to that question.

5701. Then the correct answer would have been that anæsthetics are used whenever they can be used with safety to the experiment?—That would have been the correct answer. I got a notice to be present at a meeting at St. Bartholomew's, but unluckily I did not get the notice until after I came back on Saturday, and the meeting was held a week or two ago.

5702. How often do you have these class demonstrations with living animals, do you think?—I give about eight lectures in the course of the year. I lecture once a week after Christmas; and I give from 8 to 10 lectures.

5703. And have you lectured upon your particular branch, the pharmacological branch, of physiology?—Yes.

5704. I suppose you give drugs in the presence of the students sometimes?—Yes.

5705. In those cases I suppose you are unable to give anæsthetics?—The same rule applies to that as to what I said before. If there is any operation to be done I use the anæsthetic. If there is no operation to be done and I have simply to show the action of the drug, I do not give the anæsthetic.

5706. Do you wish to give any explanation of this answer, made in the return sent from your hospital, that "in all cases of class demonstration the animals are in a state of anæsthesia"?—It is so very nearly true that it hardly needs correction; and the reason of it is this, that in the demonstrations that I give in winter I use anæsthetics in all cases. In my summer lectures, when I am giving a general course and when I show the action of drugs or would show it, the only one that I really do show without an anæsthetic is the action of prussic acid; because I lecture only on the inorganic materia medica, and so I am precluded from showing the action of strychnine and picrotoxine and other poisons which I should show to the class if I lectured on organic materia medica as well; therefore as the case at present stands, the only instance in which I exhibit the general action of a drug to the class without the use of an anæsthetic is in the case of prussic acid.

5707. I suppose you mean that there is immediate death in that case?—It is immediate death.

5708. (*Mr. Hutton.*) With regard to the lecturer who does show the action of strychnia and picrotoxine would he exhibit the action of those drugs without anæsthetics or not?—He does not show the action.

5709. (*Mr. Forster.*) Then in fact there is not before the students of St. Bartholomew's Hospital any demonstration of the effects of strychnine on living animals?—So far as I know there is not. On second thoughts, I believe that they are shown by the Lecturer on Forensic Medicine.

5710. (*Sir J. B. Karslake.*) You referred to some cases in which animals have been subjected to the gases from a brazier of charcoal; that I presume could not be done when they were under anæsthetics?—Yes it might have been done, but there is this difficulty about it, that chloroform itself has been discovered to have an action upon the blood; and then there is this further reason against doing it so, that it is well known that death by charcoal poisoning is almost painless.

5711. I was just going to ask you the question what the amount of suffering of the animal was when he was subject to poisoning by charcoal?—I believe there is no suffering. The notion that there is no suffering prevails so widely that that is a very common method of suicide in France.

5712. It is always supposed in those cases, is it not, that there is no pain?—It is supposed that there is no pain.

5713. (*Lord Winmarleigh.*) The pain is in the recovery, I suppose?—Yes; so far as I remember, in that case in which Dr. Hüter saved a man's life he

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did suffer some pain, but I will not be positive as to that.

5714. (*Sir J. B. Karslake.*) In the case of those animals operated upon by Claude Bernard the only pain they suffered, I suppose, was the pain involved in drawing a certain amount of blood from the animal and transfusing healthy blood?—Bernard did not transfuse healthy blood; that was the application of Bernard's discovery which was made by Hüter.

5715. No great pain was involved, I suppose?—No.

5716. In the cases in which you do demonstrate to your class, do you think it is essential that they should see the operation and not only obtain a knowledge of it by reading?—I think it is. It makes a very great difference to the student whether he sees it or simply hears it. If he simply hears it he will pick it up for the time being, and will be able to pass a very good examination; but what we want is that he shall learn it so as to remember it for his whole life.

5717. In the cases to which you confine your demonstration then, you think it is essential that you should make the experiment before the class?—Yes. If I may give the example of prussic acid which I do show, the only one in which I do not use an anæsthetic, it is very well to tell the student "If you get a case "of prussic acid you have to do this, that, and the "other;" but what I do is this: I take the animal, give it the prussic acid, and go through the whole process of trying to recover it. I proceed with the animal, in fact, exactly as I should do with a human being, so that I think the students in that case will remember that if they are called in to a case of poisoning by prussic acid, they have to do this, that, and the other, and they will remember it much more vividly than if I simply told them.

5718. What are the animals which you chiefly use for the purpose of these experiments?—Rabbits and cats.

5719. (*Mr. Hutton.*) Your experiments for original research are almost entirely private experiments, are they not?—Almost entirely.

5720. Can you give us the least idea on what scale they have been conducted?—The number of animals employed varies enormously according to the research. In some researches one will use only a few animals, but in others you will use a great quantity.

5721. What is the largest number used by you in scientific research for a single experiment?—I think the largest number is 90; that was one series of experiments. I was trying to discover the pathology of cholera.

5722. (*Mr. Forster.*) What sort of animals did you use for that purpose?—Cats.

5723. (*Mr. Hutton.*) Do you mean that you poisoned 90 cats with cholera fluid?—No, I set about the investigation in a different way. It was discovered by Moreau that by performing a certain operation upon the intestine you could get a discharge into the intestine. This discharge was discovered by Kühne to be exactly the same as was found in the intestine after cholera; so I thought, if we can find out the exact part of the nervous system that is concerned in causing this discharge, we shall probably be able to find out the part of the nervous system concerned in cholera, and having once found that out we may be able to get a drug that will act upon it and thus cure cholera.

5724. You performed this operation on the 90 cats under chloroform, I suppose?—Yes, under chloroform. I must say, however, that the animals were allowed to live for four or five hours after they recovered from the operation.

7525. And to live, I suppose, in a certain amount of pain and discomfort?—They suffered a certain amount of discomfort and possibly pain, indeed, probably pain, though I do not think very great pain; I think probably not much more pain than a man would suffer who had perhaps a bad attack of diarrhœa.

5726. They did not die then in the same sort of agony that a cholera patient dies in?—No, because only a certain portion of the intestine was operated upon.

5727. Did they die as the result of the experiment?—They were killed at the end of the four or five hours.

5728. Now have those operations resulted in any beneficial discovery?—Not yet; they are not finished.

5729. They are still going on, are they?—They are still going on; and it is, perhaps, not to be very much wondered at, because the subject is an exceedingly difficult one, and it would take a number of experiments to make it out.

5730. And those experiments are performed in your own laboratory at St. Bartholomew's?—Yes.

5731. (*Mr. Forster.*) What makes you choose cats for those experiments?—They are very good animals to experiment upon, and they are cheaper than rabbits.

5732. Why are they good animals?—They are a convenient size, and you get the results very definitely. If you do your operations rightly you are sure of getting the results.

5733. Do you think that the cat is on the whole better for it than the dog?—You cannot get dogs.

5734. (*Mr. Hutton.*) How do you procure your cats?—They are supplied to me by a man.

5735. Who steals them for the purpose, I suppose?—I make no inquiries.

5736. And have you occasion ever to sew up the lips of cats, or anything of that kind?—No.

5737. How do you put them under chloroform?—It is as easy as possible; cats are the easiest animals in the world to manage. All you have to do is to put the animal quietly into a milk pan, stroke it, put a towel over it, and sprinkle the chloroform quickly over the towel, and the vapour of the chloroform being heavier than air descends, and the animal goes quietly under the chloroform. The only care one has to take is that one does not keep it under it too long.

5738. How do you apply it to a dog?—If it is a small dog I generally put it under a bell jar with a sponge or a large piece of blotting paper dipped in chloroform. If it is a large dog I use a packing case with cracks in the bottom, which are stopped with blotting paper which is saturated with chloroform.

5739. (*Mr. Huxley.*) Have you ever found any difficulty in applying chloroform to frogs, because we have been told that there is a difficulty in it?—No; with a bell jar you can do it without difficulty.

5740. (*Mr. Forster.*) Why do you use wourali then in their case and not chloroform?—If I use chloroform I do not use wourali; I use one but not both.

5741. As far as regards the success of the experiment, you might just as well use chloroform for a frog as wourali, I suppose?—No, not always; it depends on what you are going to do. For some experiments you must use wourali when you want to get the nerves paralysed. You do not get that with chloroform. The action of the chloroform is upon the brain; the action of the wourali is upon the nerves. If you simply want to do an experiment which in another animal might cause pain, and which you are afraid might cause the frog pain, you may use chloroform.

5742. Then the purpose for which wourali is used is in order to keep the animal quiet, to make the experiment an easier one to conduct?—Yes, in frogs and in the higher animals it is to get rid of some of the effects which might be due to irritation of the nerve centres. For example, this is the case in some physiological experiments that have been made in Germany, by irritation of various parts of the nervous system, of the upper part of the spinal cord. You want to ascertain the influence of that part upon the vascular system generally, the system of blood vessels, and you want to ascertain that alone. If you irritate this upper part of the cord after you have given wourali you only get the action upon the blood vessels, but if you were to irritate this part without giving wourali previously, you would get the irritation conducted all down the ordinary motor nerves, and get all the muscles set into violent action; the action of the muscles would react upon the vessels, and you would get the whole experiment disturbed.

5743. Is there anything to prevent your giving both drugs, or giving them mixed together, so as to stop the pain by the chloroform and the nervous movement by wourali?—Yes, there is, and it is this:—in very many of those experiments you want to ascertain what is termed the reflex action; that is to say, that an impression is made upon a nerve and goes up to the cord and is transmitted down. Now chloroform acts upon the reflex centres and abolishes their influence completely; so that if you give the wourali which paralyses the ends of the motor nerves, and give the chloroform which paralyses the reflex centres, you deprive yourself of the possibility in many instances of making satisfactory experiments.

5744. But are there not many instances in which you give wourali simply for the purpose of getting the animal perfectly quiet?—Yes, those instances which I gave.

5745. But if it is done for the purpose of getting the animal perfectly quiet, could not chloroform be given also?—No, for that very reason; if you were to give chloroform the experiment would be at an end, you would have abolished the action of the reflex centres, and thus you might as well not do the experiment at all.

5746. (*Mr. Hutton.*) What would you say was the next largest series of experiments that you made after that on cats with reference to cholera, which you say is still going on?—The next is on the action of the snake poison.

5747. And how many and what kind of creatures have you used for that purpose?—All sorts,—rabbits, guinea-pigs, frogs, dogs, pigeons, fowls. The number I do not exactly remember. When I said just now that I used 90 cats, I should have said that was in one series, but I am now at the third series. The number 90 is not the whole that is included in the investigation; I have used a much larger number for investigating the subject of cholera than 90.

5748. What would be the total number of animals then used for these cholera experiments, and have they been all cats in all the series?—Nearly all. I could not exactly say the total number without looking up the reports of the British Association. I think I can fill in that afterwards. For the snake poison experiments I should think I have used about 150.

5749. Of different kinds of creatures?—Of different kinds of creatures.

5750. (*Mr. Huxley.*) In experimenting on snake poison you do not require that the animal should be bitten by the snake; you simply inoculate the poison?—Inoculate the poison simply.

5751. (*Mr. Hutton.*) After the snake poison what would be the next most important series of your experiments?—I think possibly those on nitrite of amyl and nitrites generally.

5752. Had you to use a great many animals for those investigations?—No, not so many, a very much smaller number; because you get very decided results. It is when you get slight differences between your different experiments that you have to use such a large number.

5753. Were you able to use anæsthetics with the nitrite of amyl experiments?—Sometimes, but generally I think I did not, because a number of these experiments were made in a laboratory in Germany, and the professor considered that it would not be at all advisable, as one would get the result complicated. By using chloroform along with nitrite of amyl one would have got the result very much complicated.

5754. For the purposes of the nitrite of amyl experiments you have to expose the arteries, have you not?—Yes.

5755. And is not that a very painful process?—I think it is somewhat painful.

5756. And the effect of the nitrite of amyl was in dilating the smaller arteries?—Yes.

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5757. (*Chairman.*) Could you not have performed the operation of exposing the arteries under an anæsthetic?—Yes, and the only objection to that is this: that after you have given the animal chloroform it does not come quite out of it for a very long time, there is always a residual action, and where you are experimenting with such a substance as nitrite of amyl, which belongs to a series of bodies nearly allied to chloroform, you would be very apt in such a case to get the residual action of the chloroform interfering with that of the amyl.

5758. Supposing you had used ether?—There again you get the same difficulty, indeed rather worse.

5759. Do I rightly understand you to say that if you were to do it over again you would not use an anæsthetic?—I should certainly feel myself obliged to do several experiments without any anæsthetic, on account of the residual action of chloroform.

5760. (*Mr. Hutton.*) Do you consider that residual action to be so considerable that the use of chloroform would have vitiated Dr. Rutherford's experiments on drugs now going on, or recently going on in Edinburgh, on the action of drugs on the liver?—No.

5761. They were all under wourali, and not under chloroform?—I do not think that chloroform would interfere much with those experiments.

5762. Do you think that, allowing for the action of chloroform, those same experiments might have been made under chloroform instead of under wourali?—I should be chary of giving an answer to that question for this reason, that I have not given sufficient attention to it to be quite certain; just thinking over it I would say possibly they might.

5763. I understood that all your experiments on the action of drugs, even those on the action of drugs on the liver, have been made under chloroform?—Yes, but I have made very few on the liver. As to the experiments that I have done, I have only made I think about five or six original experiments on the liver, and those were done under the use of chloroform.

5764. And in those cases you did not think the action of chloroform injurious?—I thought that it might interfere a little with it, but then it was a different sort of research from that of Dr. Rutherford. It could not interfere so much with mine as it might have done with his.

5765. Is it not true that wourali itself excites the secretions?—Somewhat.

5766. So that there would be a residual action to allow for in its use?—Yes.

5767. (*Mr. Huxley.*) Would there not be some danger of disturbing the condition of the circulation in the liver if you kept the animal under chloroform for the number of hours which would be necessary in Dr. Rutherford's experiments?—Yes, I was forgetting that; there would be the difficulty that you would very probably get more or less an amount of venous congestion; my experiments did not last so long.

5768. (*Mr. Hutton.*) Still you would not think that the first operation might not have been performed under chloroform; I mean the exposure of the biliary duct, even in the case of Dr. Rutherford's experiments?—To answer that question I should have to run over the whole literature on the subject to know exactly what had been made out about the action of chloroform. I do not think I would give a definite answer to that.

5769. I observed that you distinguished chloral as a hypnotic from an anæsthetic; would you tell me precisely what you meant?—Chloral in small doses produces sleep, apparently a natural sleep, and in such doses it is a hypnotic; you can wake a person readily from such a sleep. If you give an animal a large dose you produce a greater effect; the sleep is then so profound that the animal will not awake. You may cut it into pieces, or do what you please with it, it will lie as if dead. It is an anæsthetic for animals, but you cannot use it safely as an anæsthetic for human beings, because these animals that have got so large a dose of it do not recover.

5770. But you give it in doses sufficient to make

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it an anæsthetic?—I give it to animals as an anæsthetic; but the doses I give to animals are such as I would not dare to give to human beings.

5771. Do you regard opium as an anæsthetic or only as a hypnotic?—As an anæsthetic assuredly.

5772. Is it in moderate doses an anæsthetic?—Yes, I should say decidedly.

5773. Why is it then that chloroform is so very much more used than opium for the purposes of operations?—Because, to begin with, if you give a dose of opium sufficient to render a person anæsthetic you keep him soundly asleep for many hours, and then he will awake with a deranged digestion and a bad headache, and those are not the conditions in which you want to have any patient whom you wish to recover from the effects of an operation.

5774. But chloroform disorders the stomach, does it not?—But its effects are much more transient than those of opium.

5775. And it is much less dangerous, I gather from you?—Much less dangerous.

5776. I notice in this Handbook of Physiology, of which you are part editor, that you put two asterisks to a great many of your experiments which you describe as "best suited for demonstration"?—Yes.

5777. I want to ask you a question or two about some of those; for instance, the experiment on page 470, "demonstration of the functions of the "*chorda tympani* and sympathetic fibres of the "submaxillary gland of the dog", where you first subject the dog to chloroform; is not that a very long experiment?—A very long experiment.

5778. Can you keep the dog under chloroform during the whole of that experiment?—Perfectly well.

5779. Have you to have a special assistant for that purpose?—Yes; you do not want a trained assistant, but just the ordinary laboratory servant.

5780. And in the other experiments of that kind, some of them I think very long, that are also marked as demonstrative experiments, the same is true, is it, that you employ an assistant to keep the animal under chloroform for the whole period?—Yes.

5781. Now when you speak of them as demonstrative experiments, are those experiments which you would show to an ordinary class?—Yes; all those marked with a double asterisk are such as I would show.

5782. And you would think it so easy to keep the animal under chloroform the whole time, that you would have no fear of its suffering during those experiments?—None whatever.

5783. Now I see one of your experiments on page 505, I think of the same kind, in which the animal is to recover from the chloroform; it is marked with a double asterisk, and is headed "Mode of producing "biliary fistula in guinea-pigs;" there the animal is to recover, and you say that the animals would sometimes live for a week?—In that experiment you keep them under chloroform the whole time. The sentence, "when the bile duct is tied the guinea-pigs die "in less than 24 hours, but when it is not tied they "will live for a week," is simply a statement of the results obtained by Dr. Heidenhain.

5784. You mean that it is not one which you would have repeated?—That sentence is merely an interpolation, the same as this one,—"The bile in guinea-pigs "is secreted in very large quantities, being as much "as 7·3 grammes in an hour per kilogramme of body "weight." Both are merely statements of the results obtained, and not the experiment.

5785. You meant the animals to be destroyed before, did you?—Yes, this is shown at page 507, where you come to the end of the directions for performing the experiment. What you do is this, "sew up the "wound, leaving the free end of the canula outside." That is the end of the operation, and next comes what you have to demonstrate. I should have put the two former sentences as a footnote really. "The bile is "secreted under a very low pressure." You have now got to proceed to demonstrate this.

5786. The result is that the animal lives a long time, is it not?—No, you find at the end of the page, "if you carry the experiment far enough the animal dies."

5787. And is it the intention of the experiment that you should carry the experiment far enough for that?—Yes, if you do not pour sufficient water into the tube you can kill it.

5788. Then there are none of these experiments merely for demonstration that you take to involve suffering?—I think hardly any. There are two that do involve slight suffering. The first of those is at page 515, "Production of glycosuria by puncture of "the floor of the fourth venticle," and the other is "Moreau's experiment" at page 525; I think those are the only two.

5789. There is one in which you allow the effect of the anæsthetic to pass off, and then draw down the animal's intestines?—But if you look at that you will see that you only allow the effect of the anæsthetic to pass off so far that you get only a slight movement; that is all you want, you do not allow it to pass off so far as to cause the animal pain. All you want is to allow it to pass off so as to get a slight indication.

5790. To cause it uneasiness?—Yes, but I am not sure that it feels it; I am not sure that it is not reflex action.

5791. I think you said that the supposed anæsthetic effect of wourali was discovered as early as 1860?—It was discovered by Von Bezold, that when you gave wourali the animals did not react as they would under the ordinary supposition, under Bernard's supposition, that the motor nerves only were paralysed. He attributed this effect to the action of wourali on the spinal cord. But in 1870 Schiff and Lange made some experiments in which they showed that in frogs, at least under certain conditions, that is to say in spring frogs (we do not know why, but the effects of wourali seem more marked on the sensory nerves of frogs in spring,) you got from wourali very distinctly a paralysis of the sensory nerves as well as the motor nerves.

5792. And that is not equally clear in autumn or at other periods of the year?—Not so obvious.

5793. How long has it been your opinion that wourali is to a certain extent an anæsthetic?—Since I first became acquainted with Schiff's experiments.

5794. That was about a year and a half ago?—It is two years and a half ago.

5795. And since that time you have been in the habit of using it partially as an anæsthetic?—No; because I have had no occasion to use it. My experiments have not required it. I have used in preference the other anæsthetics, because I know that they were more thorough anæsthetics than wourali. I do not mean to say at all that wourali is such a thorough anæsthetic as the others. All I can say is that I believe it is a partial anæsthetic.

5796. And that you limit to the case of frogs, and frogs in spring, I understand?—I would limit it to the case of frogs. I do not know; I think it is probable that it also acts so in the higher animals, but we cannot tell. There is just this fact, that when animals have been operated upon under the influence of wourali they do not show so much pain after they come out as you would imagine they would do if they had been suffering all the time.

5797. (*Lord Winmarleigh.*) You are acquainted, I suppose, with a great number of gentlemen who are pursuing the same investigations as yourself?—Yes.

5798. Have you any means of knowing whether any of them agree with you in what you have just stated with regard to wourali?—I do not know. I could not state positively.

5799. May I ask whom do you allow to be present with you when you are making your experiments for investigation?—The room is so small that I have no one present excepting just the man who is helping me.

5800. You do not allow students or other persons to be present?—There is no room for them, my laboratory is about half the size of this table.

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5801. What is your rule with regard to destroying the animal after the experiments ?—I always do it.

5802. Invariably; you never allow an animal to recover itself for the purpose of another experiment ?—Never.

5803. (*Mr. Hutton.*) Dr. Pavy told us, I think, that the effects of poison by strychnia could be shown under chloroform, and that he did show them in his class under chloroform. Would that in your opinion show it efficiently ?—I do not think it would, because chloroform has been used as an antidote to strychnia.

5804. (*Mr. Huxley.*) Having experimented so much on poisons, I presume you have paid particular attention to the effects of opium ?—Yes.

5805. I think I am right in saying that that experiment upon the chorda tympani is occasionally performed under the influence of opium, is it not ?—Yes.

5806. Would you, having paid this attention to the influence of opium, and having been familiar with what is the opinion of those who have inquired most carefully into its effect, be of opinion that "in the "deepest narcotism produced by morphia or opium "there is no real insensibility to pain ?"—No ; I believe there is thorough insensibility to pain.

5807. If anyone stated, physiologically speaking, that as his opinion, I presume you would have no hesitation in contradicting that statement ?—No.

5808. Emphatically ?—Emphatically, however high an authority it might be.

5809. If anyone stated that persons under morphia or opium feel the pain, but they soon forget it, what would you say to that statement ?—I should ask him for the proof.

5810. If a person should say that when an animal in a physiological experiment is narcotised it is still sensible to pain, would you say that that is an accurate statement or the contrary ?—The contrary.

5811. It is a point upon which you would have no hesitation whatever ?—None whatever. With regard to that experiment on the chorda tympani, I must say that you cannot do the whole experiment under opium, you cannot show it as you could under wourali. In the description that I have given of the experiment in the handbook, I have given instructions to do it under chloroform ; but by doing it under chloroform there are several points that you cannot show. The first time that I tried to demonstrate the action of the chorda tympani under opium I failed completely in the first part. I wished to show that the irritation of the lingual nerve caused reflexly a secretion of saliva ; I utterly failed, it did not occur to me that by giving the opium I paralysed the reflex.

5812. But the second part of it, the irritation of the chorda tympani, succeeds perfectly ?—Yes.

5813. (*Mr. Hutton.*) I find that the passage in Dr. Pavy's evidence to which I was referring just now is this; he was asked at question 2143 "Have "you ever made experiments on the effect of strychnine on animals ? And he says "Yes." (Q.) And "you have proved, have you not, that death under "strychnine generally takes place from the ceasing of "respiration ?—It does. (Q.) In that case were you "able to use chloroform ?—Yes. (Q.) The chloroform did not in that case interfere with the success "of the experiment ?—No. There is an experiment "which I perform at lecture in which the animal is "placed under the influence of chloroform, and some "strychnine is introduced into the circulation whilst "the animal is under the influence of the chloroform ; "and whilst the animal is still under the influence of "the chloroform it dies immediately." What I wanted to ask you was whether you thought that that demonstration could be efficiently performed under the influence of chloroform ?—It may be so performed, but I should not think it so efficient or advisable for a class as the other course ; because in showing these actions to a class my object is, first, to impress upon the student the mode in which the poisons act, and secondly, to impress upon him what he is to do in case he gets a case of poisoning brought to him. If I were showing that to a class, I should first give the animal the strychnia, and then give it the chloroform, and show how the action of the strychnia was modified by it, instead of giving the chloroform first.

5814. But I suppose in the case of some poisons the experiment is an extremely painful one, in the case of picrotoxine, for instance ?—It is just the same as in strychnine.

5815. (*Chairman.*) You have told us that you have tried a great many experiments in the hope of discovering some antidote for snake bite ?—Yes.

5816. May I take it then as your deliberate opinion that it is by the way of experiment that we are to find an antidote, if we do find one ?—Assuredly.

The witness withdrew.

Adjourned to to-morrow at 12 o'clock.

Tuesday, 14th December 1875.

PRESENT :

THE RIGHT HON. VISCOUNT CARDWELL, IN THE CHAIR.

The Right Hon. Lord WINMARLEIGH.
The Right Hon. W. E. FORSTER, M.P.
Sir J. B. KARSLAKE, M.P.
THOMAS HENRY HUXLEY, Esq.
RICHARD HOLT HUTTON, Esq.
N. BAKER, Esq., Secretary.

ALEXANDER JAMES SINCLAIR, M.D., called in and examined.

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5817. (*Chairman.*) You are a physician in Edinburgh ?—I am a physician, and a Fellow and Examiner at the College of Physicians in Edinburgh.

5818. And you were a demonstrator of anatomy at the School of Medicine in Edinburgh ?—Yes, during the years 1868, 1869, and 1870.

5819. Your name has been mentioned in the course of our inquiry by Doctor Hoggan, who says that you gave him some advice on the subject of operating upon animals. I think you have had an opportunity of seeing the passage in which your name is mentioned ?—I have.

5820. Have you any observation to offer to us upon it ?—Simply this, that he made a statement which was inaccurate as to the fact.

5821. Will you be so good as to give us anything which you wish to give in the way of correction ?—He implied that I was in the habit of advising him to operate, and also of operating myself without the use of anæsthetics. Now such was not the case ; he must have mistaken my meaning when I gave him the advice that he mentions in the report that was sent to me.

5822. You altogether negative the statement that you advised him "not to have cats, because they "made such a squalling and scratching when you "began to cut them" ?—I negative the statement as far as the latter observation is concerned, I advised him not to use cats for vivisection, because I thought that the handling of them would be troublesome ;

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that they would be troublesome animals to use for the purpose.

5823. Have you anything to say about any advice given to him respecting anæsthetics ?—I gave him no advice respecting anæsthetics.

5824. Either one way or the other ?—Neither one way nor the other, the subject was not mentioned.

5825. Then will you state what your own practice is with regard to anæsthetics ?—I have performed vivisection with and without anæsthetics; but more frequently with anæsthetics, as finding it more convenient.

5826. But is it the practice, so far as you are acquainted with it, to regard the sufferings of the animal as of importance, and to get rid of those sufferings by the use of anæsthetics wherever it is possible to do ?—I think that the use of anæsthetics offers a double advantage, which is not solely to the operator; it offers an advantage to the operator as keeping the animal much quieter, and also saves suffering to the animal.

5827. But I wished rather to limit you to this point, whether anæsthetics are, as far as your knowledge goes, used for the purpose of taking away pain as far as they can be ?—I believe they are.

5828. You know nothing to the contrary ?—I know nothing to the contrary.

5829. (*Lord Winmarleigh.*) Would you state to the Commission what are the experiments that you have chiefly been connected with which you perform without anæsthetics ?—I have performed experiments for two purposes. The first purpose was for a paper I was writing on the inhibitory action of the nerves on the secretions.

5830. Were those experiments in which anæsthetics could not have been used ?—The experiments I made for the elucidation of that subject were done both with anæsthetics and without anæsthetics. I found afterwards that it made no difference if I used anæsthetics, and I continued the experiments with anæsthetics.

5831. But when you first made the experiments you believed that they could not be made under anæsthetics ?—I used both means to ascertain first whether there was any difference.

5832. But you did that with the object of relieving pain to the animal ?—I did.

5833. Do you believe that in Edinburgh in the experiments with which you are acquainted the same feeling pervades the heads of departments there; that is to say, the persons who are most experienced in these matters ?—I think so.

5834. Do you know anything of the Veterinary College at Edinburgh ?—I do not.

5835. You do not know anything of the experiments made by the head of the veterinary department ?—I do not.

5836. Do you believe that there would be any objection on the part of the professors of physiology or medicine in Edinburgh to control being given by legislation over them ?—I am not aware.

5837. Would you yourself object ? Would you, for instance, object to an inspector ?—Yes, I would.

5838. On what ground ?—That the inspection is needless, unnecessary.

5839. You do not believe that there are any experiments made without anæsthetics except under absolute necessity ?—I do not.

5840. You speak with considerable experience on that point, do you ?—I do not speak with considerable experience, but as far as my knowledge goes of the practice of vivisection in Edinburgh that is the case.

5841. Could you state to the Commission about what number of animals you have operated upon yourself ?—Somewhere about twelve.

5842. Twelve in all ?—Twelve in all, not more.

5843. In the course of your practice ?—In the course of my practice.

5844. What have they been ?—Dogs, rabbits, and guinea pigs. I have only vivisected two dogs, and they were done for two purposes; one purpose I have already mentioned, and there was another object also.

5845. Not frogs ?—I have vivisected frogs. I beg your pardon for forgetting to mention them. I cannot tell the number of frogs which I have vivisected.

5846. Is it a great number ?—I cannot say.

5847. (*Sir J. B. Karslake.*) In the case of frogs were any means taken to prevent pain by pithing or removing the brain or using chloroform ?—In such cases where the experiments could be so done, I pithed the animal; but that is not vivisection.

5848. However, in the case of frogs, you have pithed the animal where it was possible to do so consistently with the experiment ?—Where it was possible to do so consistently with the experiment I have.

5849. Did you use experiments on living animals for the purpose of demonstration to your classes ?—No, I never had occasion to do so.

5850. Then your experiments have all been for private purposes ?—For private purposes, my own experiments.

5851. (*Lord Winmarleigh.*) Were any other parties present ?—Yes.

5852. What class of persons were present ?—Some of my pupils.

5853. In your investigations ?—In my investigations.

5854. (*Sir J. B. Karslake.*) Do you remember the fact of a conversation with Dr. Hoggan upon the subject of vivisection ?—I do.

5855. About how many years ago was that ?—In 1869.

5856. Had you had much practice in experiments on living animals yourself at that time ?—I had not.

5857. Had you made any experiments on living animals ?—I had.

5858. On dogs ?—On dogs.

5859. And had you made these series of experiments on twelve animals which you have spoken of at that time ?—I had not at that time.

5860. How many experiments had you made on living animals at that time ?—I cannot remember.

5861. At all events not so many as twelve ?—Not so many as twelve.

5862. Do you remember the circumstances under which the conversation took place between you and Dr. Hoggan ?—I do.

5863. Was it a casual conversation, or did he come to you for advice ?—It was a casual conversation.

5864. Can you give it to us as far as you remember it ? What led to it ?—He was elucidating some theory of respiration at that time as a student, and we had many arguments together regarding it, and it was at that time that I advised him to have more proof by using vivisection.

5865. He had not used vivisection, as far as you understood, up to that time; is that so ?—I am not aware of his having used vivisection up to that time.

5866. Now will you go on with the conversation as far as you remember it. Do you remember what led to your mentioning cats as animals that were not proper to be employed for the purpose ?—No, I do not; it was in the course of casual conversation. I mentioned that merely as an advice to him to use the most convenient animal to give him the least trouble.

5867. Were the experiments which he proposed to make, and which you advised him to carry out upon living animals, experiments which could have been made under anæsthetics without injuring the effect of the experiment ?—I cannot say.

5868. Was your conversation with reference to a particular class of experiments to be made for a particular purpose, or with reference to experiments generally on living animals ?—Experiments to be made for a particular purpose.

5869. And that is the only conversation which you recollect having had with him ?—That is the only conversation that I recollect having had with him.

5870. (*Mr. Huxley.*) Could you tell us in a few words what was the nature of the theory of respiration which was to be tested ?—The theory was that the forcing of the blood through the lungs by the heart had the power of expanding the lung itself.

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5871. Independently of the action of the respiratory muscles?—Yes.

5872. Do you mean that the forcing of the blood into the lungs distended them?—I do.

5873. That is a purely mechanical problem?—A mechanical theory of respiration.

5874. And one which could be subjected to no test except that of actual experiment by vivisection?—I believe not.

5875. On the face of it you will agree with me, that it does not look very probable?—Not very probable.

5876. But still it might be possible, by appropriate experiments, to determine the matter of fact whether the lungs are enlarged in accordance with the systole of the heart or not. I say it might be possible, without, of course, admitting that it is likely?—It might be possible.

5877. And seeing that this was the only way of bringing the thing to a practical test, you recommended Dr. Hoggan to apply that practical test?—I did.

5878. You are very familiar, I presume, with the relations of professors and students in the University of Edinburgh?—Yes.

5879. Is it not the custom in Edinburgh for a professor, if he finds a young man who shows a disposition towards scientific inquiries, to encourage him to pursue those inquiries?—It is.

5880. In fact, a professor would think he was failing in his duty if he did not so encourage a young man who showed ability?—He would.

5881. And it often happens, does it not, that a young man who shows such capacity is encouraged by his teachers to embody the results of such research in his thesis?—He is.

5882. That is very commonly the case, is it not?—Very commonly.

5883. And when such theses are meritorious, especially when they contain the results of good original research, it is very common that medals are awarded for them, is it not?—It is so.

5884. It is a matter of common notoriety that that is the practice of the university?—That is the practice.

5885. Were you actually a teacher at the time that Dr. Hoggan applied to you?—I was a demonstrator of anatomy.

5886. And therefore a teacher?—Therefore a teacher.

5887. Were you demonstrator in the university?—I was demonstrator in the School of Medicine.

5888. Was Mr. Hoggan then a student?—He was a student at that time.

5889. And you stood to him in the relation of teacher to student?—I did.

5890. And in giving him that advice you were pursuing the course, I presume, that you would have pursued with regard to anybody who showed an aptitude for research?—Yes.

5891. You considered that you were simply doing your duty in that?—Yes.

5892. It was not from any general love of vivisection that you advised him to make these operations?—Not at all.

5893. (*Mr. Hutton.*) Is it the fact that cats are particularly difficult to handle under chloroform?—I cannot speak from experience as to that.

5894. We were told yesterday that they were particularly liable to the influence of chloroform; very easy to chloroform?—I cannot speak from experience on the subject.

5895. Might not Dr. Hoggan have fairly inferred from your suggestion that you were not thinking of the use of chloroform, but simply of vivisection without chloroform?—I cannot say so.

5896. As you made no reference to anæsthetics, and simply advised him not to use cats because they were difficult creatures to handle, did not that imply that they were not to be used under chloroform, or might it not so have implied to his apprehension?—I cannot answer how Dr. Hoggan would take it up. I certainly did not imply it myself.

5897. (*Mr. Huxley.*) As a matter of fact the preliminary operation of handling a strange cat before it is chloroformed is a difficult one, unless you know how to set about it, is it not?—It is. That is the reason why I advised him not to use cats.

5898. (*Mr. Hutton.*) Now would you at that time have thought him doing a wrong act if he had made these experiments on cats without using chloroform?—I would not.

5899. But the chloroform would not have interfered, I take it, with the particular action of the lungs that was to be inquired into, would it?—I do not think so, but I cannot say.

5900. When you spoke of your operations on frogs, you said that you were quite unable to give the number, which I can easily understand, but was it a large number or a small number of frogs that you had vivisected? Would you say, for instance, that they counted by dozens, or by scores, or by units?—Perhaps by units.

5901. You mean to say, of course, under dozens; you mean to say that certainly, if you have vivisected frogs, they have not been much above a dozen?—They have been more than a dozen.

5902. But not many dozens?—I cannot remember the number. I must have vivisected about 30 or 40 at least, perhaps more.

5903. Have you ever used anæsthetics with them?—No.

5904. Do you regard them as animals that do not suffer sufficient pain to need anæsthetics?—I do.

5905. And in regard to the experiments that you made with respect to the inhibitory action of the nerves, I think I understood you to say, did I not, that you did not use anæsthetics in some of those cases, simply because you thought that they might interfere with the results of the experiment?—I did think so.

5906. But that otherwise you certainly would have used them?—Yes, certainly.

5907. Why, then, do you say that you would not have condemned Dr. Hoggan if he had made these experiments on cats without attempting to anæsthetize them?—His desire was to elucidate a theory, and if anything had gone wrong in the experiment, he might have ascribed it to the action of the chloroform.

5908. You think that it might fairly have been ascribed to that? That it might really have interfered with the experiment?—He might have thought so.

5909. Do you think that the opinion in the University of Edinburgh generally at that time or now would be unfavourable to the making of experiments without anæsthetics, without any very special regard to whether or not the anæsthetic would interfere with the object of the experiment; I am speaking of painful experiments, of course?—I think anæsthetics would be used in every case that they could be used in.

5910. Do you think that the opinion of the university would strongly condemn a man in any case where he did not use anæsthetics, if it was clear that the anæsthetics would not interfere with the course of the experiment?—I do.

5911. (*Chairman.*) I understood you to object to inspection on the ground that you were not aware of any abuses which rendered inspection necessary; is that so?—Yes, that is the reason of my objection.

5912. But supposing that it were the object to prevent experiments being conducted, not by the persons whose humanity you have spoken of, but by a different sort of person altogether, of whose humanity no favourable account could be given, would you object on that supposition to the institution of inspection?—I would not; I would desire it.

5913. (*Lord Winmarleigh.*) Do you think that that is the general opinion of those who are engaged in the same practice as yourself?—I think so.

The witness withdrew.

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PETER DAVID HANDYSIDE, M.D., F.R.S.E., called in and examined.

5914. (*Chairman.*) You are teacher of anatomy in the Edinburgh School of Medicine ?—Yes, and examiner in anatomy, physiology, and surgery to the two Royal Colleges of Edinburgh, and during this year to the University of St. Andrews.

5915. And a consulting practitioner in medicine and surgery ?—Yes.

5916. And you have been acting and consulting surgeon to the Royal Infirmary of Edinburgh ?—Yes.

5917. And you have been for a great number of years, I think, a teacher ?—For 26 years. I was for 12 years teacher of anatomy and physiology, until the disjunction of physiology from anatomy in the Edinburgh School of Medicine. I then taught systematic, clinical, and operative surgery during two years of my service as acting surgeon. I have subsequently taught anatomy, human and comparative, for 12 years, including the current teaching year.

5918. That is your position as teacher at the present moment ?—Yes ; teaching anatomy regularly during six winter and three summer months.

5919. Have you had the opportunity of seeing some remarks made upon your name by Dr. Hoggan at question 3482, and so forth ?—I have. If I may be allowed to do so, I will make a remark on his answer to question 3471. Mr. Hoggan says, "I was strongly urged to try experiments by my teacher and by others, on a subject that had already been published. I refused to do so, and tried quite by myself" (although he was my demonstrator at the time this was unknown to me) "whether I could administer the anæsthetic and go through it. I found I could not do it, and therefore I refused to go further into the question until I had learned how vivisection was done." He never made any inquiry of me on these points. Then, at question 3472, he was asked "Am I to understand that you were advised by the lecturer whose lectures you attended to try experiments yourself ?" And his answer was, "I was advised by nearly every one of my friends to do so, and the lecturer, my own teacher, offered to associate himself with me, and put his own rooms at my disposal for the purpose." That I associated myself with him is not correct. I said to him that as he was my demonstrator of anatomy my rooms were at his disposal for any observations and experiments which, under my approval, he might make. That is quite the case ; but as for associating myself with him, I did not admit his theory. The word "associate" indicates that I took up his view, which I shall show afterwards not to be the case ; but as he was my demonstrator, he was associated with me in that sense.

5920. In popular language, you do not object to the term, but you object to it if it implies that you were identified with that particular view ?—Yes. At the end of question 3478, there is a quotation which evidently is meant to apply to me, and I probably did make use of those words, "You have laid it out beautifully in theory, but you must show by experiments how it is done ; you must try experiments on animals, and publish your theory along with those experiments." Now, I have no doubt that I said that, because I had confidence in Mr. Hoggan, as an elderly and intelligent student, and I told him, "It is impossible to establish your theory without experiments on the lower animals." Then, question 3480 is, "When this advice was given to you to try experiments, was it accompanied with any advice to subject the animals to as much anæsthesia as possible ?" He says, "There was no question of anæsthesia at all." My commentary upon that is that this was taken for granted. It was not spoken of, but it was taken for granted that anæsthetics should be used. With the exception of the experiments showing the circulation in the web of the frog's foot, he had never seen experiments by me. It is customary for me to put a dead dog upon the table for my introductory lecture ; but that dog I have either given prussic acid to, or pithed it, or given chloroform to it ; or I get from the Veterinary College a dead hound (often they have such animals which die under their care from disease). It seems the best introduction to a course of anatomy to lay a dissected animal on the table.

5921. The reason, as I understand you, why nothing was said to Dr. Hoggan about anæsthetics was, that it was taken for granted by you that no pain would be inflicted upon the animal in question that could be avoided ?—Clearly.

5922. And that he as your demonstrator knew that from the practice which he saw followed in your experiments ?—Yes.

5923. May I take this opportunity of asking you if you have been secretary of the Society for Prevention of Cruelty to Animals in Edinburgh ?—Yes, I had my share in originating that society in 1839, was the secretary for the first three years, and see nothing in vivisection inconsistent with the objects of that very humane society. The reasons I will give afterwards. Then, at question 3483, Mr. Hoggan is asked, "Is he a lecturer now ?" And his answer is, "Yes ; and he offered to associate himself with me in the whole theory." I reply to that that it is not so, I told him that I had no grounds from observation or experiment to think favourably of his theory. Then his answer proceeds, "and place his rooms at my disposal in order to elucidate the theory, or rather to illustrate it by means of experiments." This I did, but not the former ; that is to say, I did not offer to associate myself with him as favouring the theory.

5924. (*Mr. Huxley.*) I presume that you contemplated that Mr. Hoggan, then being a promising student, would probably make a good piece of original research, and for that purpose you let him have access to your rooms and the conveniences thereto attached ?—Yes.

5925. But it would be an error to suppose that you meant in any way to connect yourself with his work further than that ?—I had no intention of doing so.

5926. I am almost ashamed to ask you the question, but supposing that he had published a memoir on the subject, you had no intention of associating your name with it ?—No.

5927. Or in any way to take the credit of it ?—Not the slightest. I rather opposed it, and showed him that *à priori* it was unphilosophical, and that it was not based upon fact, and could not be established, as I conceived, by experiment ; but I gave him full liberty to throw light upon it by experiment ; it was my duty as a man of science to do so.

5928. (*Chairman.*) I think I have gathered from you that, as far as your long experience of teaching in Edinburgh goes, humane consideration for animals is always kept in view ?—Always.

5929. That in instituting experiments on animals, you, as far as possible, avoid experimenting upon them while they are living, or, that if you experiment upon them while they are living, then, if possible, it is under anæsthesia ?—Quite so.

3930. Now will you state what is the practice when there may be an experiment which must, if it is performed at all, be performed upon the living animal, and where anæsthetics would interfere with the experiment, is it the practice then to shorten the duration of any suffering as much as possible ?—As an illustration of that, I may say that when we show the action of the heart in a frog we always kill it beforehand, just instantly cut off its head, because the heart's action in a frog is as well shown after death as before. M. Onimus recently states that two hours after death he found the right auricle of the heart of a decapitated criminal beat spontaneously. After the lapse of five hours constant currents applied to the skin caused the contraction of the subjacent muscles. I may add that when a healthy dog's tail is cut off it will move for 20 minutes ; showing that irritability and vitality to that extent is not gone ; mobility remains but not sensibility. First, therefore, I say that the heart's action in a frog

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can be shown after death. And, secondly, if we want to show the action of the muscles, or to count the rapidity with which the nerve current moves in the frog, that also can be done after death.

5931. What you have mentioned you mention in confirmation of the statement that the sufferings of animals is an object always present to the mind of operators, as far as you know it, in Edinburgh ?—Yes.

5932. And that every effort is made to avoid suffering, and to mitigate and diminish suffering ?—Consistently with my knowledge that is always an object in view, and held stedfastly in view in performing experiments on the lower animals. I have an apparatus with me for the purpose of showing the circulation in the web of a frog's foot; very simple, being a slip of wood, a damp calico bag open at two parts, and a soft tape, and the animal suffers nothing. (*The witness produced it, and described it to the Commission.*) In order to show under the microscope the circulation in the web of a frog's foot, I place this instrument against the upright bar of the microscope. I then insert the frog in the bag, previously soaked in cold water, and its head projects here (*pointing to it*), and one limb at this point (*pointing to it*). I then bring this tape gently in this way (*describing it*), measuring the amount of pressure exerted upon it cautiously, because if I put too much pressure it will disturb the circulation; and so far, therefore, as these two stages are concerned there is no pain. Then the web is spread over this opening (*pointing to it*), and there are inserted four minikin pins, each $\frac{1}{8}$ of an inch in length, and so slender, yet conical, as not to draw blood. These transfix the web, which is very little sensitive as we believe; because the animal shows no repugnance in the shape of retraction or contraction of the part; after it is once in its whole body is at rest, and the web stretched out remains for about an hour under the microscope. The circulation otherwise is not interfered with, and its respiration is quite free during the period that the students are one by one looking through the microscope.

5933. (*Lord Winmarleigh*). You do not consider that a cruel experiment at all ?—No; and it is essential.

5934. And not attended with cruelty you think ?—Not attended with pain to the animal. I do believe that.

5935. Do I rightly collect from what you said that you can experiment on the nerves of a dog without inflicting pain ?—My remark applied to a frog at that time. We cut off its head. We always kill the frog before examining the heart, and then we may lay the heart out upon a bit of cold marble.

5936. But you alluded to cutting off the dog's tail, did you not, and said that you could operate upon the nerves without inflicting pain ?—My remark had reference to a large watch dog, the tail of which my elder brother wished shortened. After having sewed up the wound at the stump, and after the animal had run about, I watched the state of its detached tail, and found that till the expiry of 20 minutes, lying in the stone trough of a pump well, it was contracting and contracting, showing that the irritability and mobility remained under the contact of the irritation of cold for that period of time. It was the custom to dock ears and tails too, which I do consider unjustifiable and very cruel, as well as injurious to the animal afterwards. Still I make the remark as a physiological observation, and one which shows that there is no need for us to operate upon the nerves of a live frog when we wish to show the current through the nerves, because that is sufficiently shown from the irritability remaining after the animal's head, or the dog's tail, as the case may be, is cut off.

5937. You say that you have been secretary to the Royal Society for the Prevention of Cruelty to Animals ?—To the Scottish Society for the Prevention of Cruelty to Animals.

5938. Then I presume that you are not at all opposed to legislation upon the subject ?—Not at all opposed to legislation.

5939. You yourselves have recommended legislation on the subject, have you not ?—Though a director at present, I have not usually attended the meetings. However, if they have done so it is quite with my approval of certain legislation which I am prepared to suggest.

5940. What legislation would you suggest ?—I would suggest that a system of licensing would afford the general public "ample security that the practice of "vivisection is nowhere being carried on to any objectionable extent." And the operation of that might be, firstly, that certain students and registered practitioners wishing to conduct *original* experiments *only*, should be licensed; secondly, that teachers of anatomy, surgery, and physiology may be free to repeat such an essential experiment as that upon a frog's foot, showing it once to each of their classes; and also be free to conduct, *in private*, any original experiments under anæsthesia, where anæsthetics can properly be used. Then the mode of licensing should be twofold. Firstly, two laymen, such as justices of the peace, I think, should be required to say "We consider this party and these premises" (I would register both the individual and his premises) "a proper person to be intrusted with the performance of vivisection for original experiments, *or* "for teaching purposes, and a proper place for that "purpose." Secondly, to ensure that the experiments shall be original, there should be a certificate from a public teacher of anatomy, physiology, or surgery in a school of medicine, or in the medical faculty of a University, to testify that, "To my certain knowledge "A.B. is a fit and proper person to practise vivisection under the statutory regulations."

5941. You would have two justices of the peace in the first instance you say ?—Yes. I think laymen should be conjoined with a professional expert in order to allay public disquietude.

5942. But could two lay magistrates know the qualifications of the person ?—I think it well that they should represent the general public. Every teacher of anatomy must possess the Home Secretary's license, countersigned by two justices of the peace; and not only is the teacher licensed, but the place also where he means to practise anatomy.

5943. Does your plan include an inspector ?—Yes, three inspectors; one for Scotland, one for England, and one for Ireland.

5944. What class of men would you take the inspectors from ?—I think you require to have for inspector an experienced anatomist, practical surgeon, or physiologist. I think these are the only three who usually perform such experiments.

5945. What power would you give to the inspector ?—I would give him power to see that the Act is properly carried out. This power would run parallel to that of the inspector of anatomy. His duties are to see that there are no bodies dissected except those that are obtained according to the provisions of the Anatomy Act of 1832; that no one shall offend against the provisions of that Act, and that the Home Secretary be furnished quarterly (say, in this case, half-yearly,) with an official return.

5946. How many inspectors are there for the Anatomy Act ?—Three, one for each division of the kingdom.

5947. Is that found to be sufficient ?—Yes.

5948. Do you think that one inspector for each division of the kingdom would be sufficient for this purpose of licensing the practice of vivisection ?—Perfectly sufficient. Few indeed would apply for the license. In a great measure the excitement on this matter has no proper basis to rest upon. To supply vivisectionists adequately the number of animals required is comparatively few.

5949. But we have been told that the study of physiology is very much on the increase ?—Undoubtedly it is; and it is essential further to the advancement of medical science that physiological and toxological experiments should be performed. In the notes of my evidence before me I have stated

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that medical science is but in its infancy; and the past and future of medicine as a science in its infancy necessitates the licensed practice of vivisection.

5950. Do you know whether your views are in accordance with the views of the majority of the profession ?—I believe they are.

5951. Have you had much communication with the profession in London or Dublin ?—Not much, because we are so thoroughly at one on the subject that we have taken no share in these newspaper agitations, excepting that we concur in thinking that there is no good basis on which the objections taken to moderate vivisection rest. The ways in which we think that the consideration of the past and future of medicine as a science in its infancy points to the necessity of a licensed practice of vivisection are, first, by the discovery through experiments on animals of the properties of agents for relieving pain in man, such as chloroform ; secondly, the discovery of curative agents for the removal of disease; and, thirdly, the immediate rescue of human life from vitiated air, poisoning, and drowning. Experiments alone enable us to ascertain methods to be employed for this last-named juncture. For instance, Marshall Hall's methods and others' for resuscitation of man could only be substantiated through experiments on animals. Sir Astley Cooper's and Sir David Barry's experiments upon the circulation of the blood also call for mention. I say, in short, that experiment is the basis of medicine. The experiments of Mr. Syme on the periosteum of bone in living animals ; those of Sir Benjamin Brodie upon vegetable poisons applied to wounded surfaces of domestic animals ; and those of myself on the reproduction of cartilage, ligament, and nerve substance after excision of these ; on the portal circulation also ; and on the share that the lacteals, the lymphatics, and the veins respectively have in the absorption of poisons from the surfaces and interstices of the animal body;—all these have been admitted to be essential in their application to different phases of disease and accident in man. Further, with regard to colotomy, nephrotomy, and excision of the spleen, I may observe that we should not have been emboldened to perform these operations on man for the saving of his life without previous performance of them on the lower animals. I said that medical science is in its infancy, and I believe fully in vivisection; otherwise medicine, which is an inexact empirical, or tentative science, not like chemistry or mathematics, could not advance any further. The longer we live in the profession the more we see that we know very little; but we are getting exact knowledge, and I believe that is very much dependent on experiments on the lower animals done humanely and mercifully.

5952. I think I understood you to say that your opinion that it is desirable to put these experiments under some statutory regulation is an opinion which is shared in by the profession at Edinburgh ?—I have heard opinions in Edinburgh to that effect, but I have not spoken generally upon it. I believe it would be acceptable.

5953. (*Mr. Forster.*) Referring to the evidence of Dr. Hoggan, will you look at questions 3481 and 3484, in which he states, that in consequence of the kind of advice which, he says, you gave to try experiments, there was much private experimenting by the students ?—That is perfectly unknown to me. I am not aware of any case among my pupils of any such private experimenting by them as pupils; but I may give the name of Mr. Gilruth, who, after having passed physician and surgeon in 1865, has experimented. He became my pupil only in 1867–8, my assistant in 1868–9, and he performed his experiments between 1868 and 1873.

5954. If you will observe the exact form of the question at 3484. I asked Dr. Hoggan this question, " Then you give this as a fact, that you desire us to " take, upon your own personal knowledge, that at " the time that you were studying medicine at Edin- " burgh, it was common for the medical students to " try private experiments, and that with the know- " ledge and concurrence and approval of the lec- " turer ? " You in fact being the lecturer. And his answer is, " I do " ?—To that I answer, that it is not so, so far as I am concerned or have reason to know. Therefore I give a distinct denial to that statement included in the words, " it was common for the " medical students to try private experiments, and " that with the knowledge and concurrence and " approval of the lecturer." I do not believe any other teacher did such a thing, and I know that I myself never did.

5955. In the years 1871 and 1872 you were a lecturer, were you ?—Yes, as now on anatomy.

5956. Was there any other lecturer at that time ? —None connected solely with the Royal colleges. Mr. Turner was then as now in that capacity in the medical faculty of the University.

5957. And at that time, whenever anæsthetics could be used in any experiments, you yourself used to use them, did you not ?—I always did.

5958. (*Sir J. B. Karslake.*) For how many years have you been a lecturer having classes under you ?— This is my 26th year in teaching, 12 of which number were employed in lecturing on anatomy and physiology, 12 on anatomy, and two more (making the whole number of 26) on systematic, clinical, and operative surgery, in connexion with part of my term of office as acting surgeon to the Royal Infirmary.

5959. In the course of that number of years, have you ever recommended to students who were members of your class to practise these experiments on living animals in private ?—No.

5960. Or have you ever suggested it to them ?—No.

5961. And as far as your experience goes, have they done so ?—No. I am not aware of any, except what Dr. Sinclair has admitted in his evidence here to-day, that he had experimented, but when, is unknown to me. I did not recommend or suggest it to him.

5962. With regard to the question of the licence, as far as you yourself are concerned, and as far as you know the opinions of others in Edinburgh, would it be felt to be the least slur on the profession or on physiologists that they should act under the authority of a licence ?—I believe that some might not like it, but I think there is no ground for that feeling. Mr. Turner and I, for instance, are licensed as teachers of anatomy, and we are threatened with imprisonment for three months or a fine of 50*l.* if we depart from the regulations. Since we brook that, we could quite well brook having a licence to operate upon the lower animals, with a view to quiet the general public.

5963. Although you are under those regulations, they do not inspire any great terror in you, I gather ? —Not the slightest; and I should be very happy to see vivisection put under the provisions I have already suggested, to license the place and the man, just as under the Anatomy Act. There would be a parallelism between the two Acts, and a great affinity between them, to protect the practice of anatomy, and to regulate the practice of vivisection.

5964. (*Mr. Huxley.*) I asked Mr. Hoggan, on the occasion on which he appeared here, whether, when you gave him the advice which has been referred to, he had not been a medical student for three years, and he told me that he had been, and he stated that he had gone through two courses of anatomy; that was so also, was it not ?—It was.

5965. He had attained such an amount of proficiency in anatomy, that you had made him your demonstrator ?—Yes.

5966. I presume I may take it that you would not make an unskilful person, a clumsy person, a demonstrator ?—Never. I required a man of good talent as an anatomist. Dr. Sinclair, who has been examined here to-day, was my demonstrator for the two previous years, but he went away to a practice in England; Mr. Hoggan had been my assistant in my museum and anatomical researches, and I said to him, " You " can undertake this," and I found that he could do it uncommonly well, and that he satisfied all my pupils.

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5967. I may take it from you that the office of a demonstrator is one which requires considerable knowledge and skill in anatomy?—Yes.

5968. So that in giving this advice to Mr. Hoggan, you were not advising a raw student to begin vivisectional experiments?—Quite the contrary; I was advising a man in whom I had confidence.

5969. And as a matter of fact Mr. Hoggan would have had and would have needed no further instruction in anatomy?—And I believe got none; I am not aware of it.

5970. In reply to an inquiry of mine Mr. Hoggan said that he made medicine a sort of pastime. Under those circumstances you will understand that it did not occur to me to put to him a question which would elicit the statement which you have just made; so that we had not any information before us which led us to understand his exact position as an anatomist. But I now take from you that so far from its merely being a pastime, he was actually so accomplished in anatomy that you made him your demonstrator?—Yes; but he is so far accurate in this statement which he makes in answer to question 3496. "I had retired "from the service and made medicine a sort of "pastime;" he told me himself that he was a Royal Navy engineer on board ship for distillation of seawater at Annesley Bay during the Abyssinian war, as I understood him to say to me; and we frequently talked of it, for I asked him the process, and I found him a most intelligent man. He visited me in my house, and I had a good deal of communing with him at the museum. With regard to Mr. Hoggan's statement at question 3491, that he had been three years a student when I gave him the advice, I would remark that Mr. Hoggan was an elderly and distinguished student in whom I had more confidence as a demonstrator of anatomy than in many a young M.D. He had in fact raised himself from the position, as I understood him to say, of a working engineer on the Clyde, and thence went to India. He, as a navy engineer, showed great acuteness, conjoined with habits of steady and close application.

5971. I presume in your long experience as a teacher, it has often happened to you to have intelligent students coming for your advice in respect of views which they might entertain; and I suppose I may take it that you would not only give them freely such advice as you thought most appropriate in the particular case, but that you would regard it as your duty to give it them?—Unquestionably.

5972. I put to Mr. Hoggan this question at No. 3519. "Is it not within your knowledge that "the professors and teachers in the University of "Edinburgh think it their duty to encourage any "young man who shows a certain amount of scientific "aptitude, to carry out original research, and to "embody the original scientific research in the thesis "which he presents?" To that inquiry I got no direct answer. I will put the same question to you?—Clearly it is so.

5973. Is it not a matter of perfect notoriety, familiar to everybody who has ever been in the University?—And the School of Medicine in Edinburgh generally, comprising both the original school founded by the two Royal Colleges and the more recently formed Medical faculty of the University. It is uniformly the case. It is the duty of the teachers, and they do it.

5974. And I presume that if a student, of whose aptitude as an anatomist you had no great opinion, or whose knowledge you had reason to doubt, came to you and suggested that he should make a research involving difficult experiments, you would tell him that he had better learn anatomy first?—Clearly. I would not allow anybody not acquainted with the foundations of anatomy and physiology to venture upon such a course as experimenting on a living animal.

5975. May I ask further, whether it is not within your experience and knowledge true of persons engaged in physiological pursuits, that when the alternative arises in experimentation of performing experiments with anæsthetics or without them, the choice of anæsthetics is sure to be made, unless they should be in some way injurious to the experimentation?—That is always the case, and I am including myself in the answer.

5976. Did you ever meet with anybody who would deliberately say that he did not care to administer anæsthetics for the benefit of the animal, or for the prevention of pain in the animal experimented upon, but that he simply used them to save himself trouble?—I never met with such a monster.

5977. You know Edinburgh well, and you have known the life of medical students there for 25 years, if not more. Do you think that any man would dare say that in Edinburgh in medical society?—Certainly not. There is a remark which I should like to make on Mr. Hoggan's answer to question 3523, bearing on that point; he says, "I know that the question of "putting in a thesis is often spoken about with "a sneer." Now, as a public teacher of 26 years standing, I can state that I have never heard a thesis spoken about with a sneer. Then at No. 4022 Mr. Hoggan is asked this question, "I think you stated that "Dr. Handyside advised you to conduct this experi"mental inquiry, because you had stated to him that "you had new views upon respiration?", and he answers, "I had done so for a period of about two "medical years, and every other day I am sure I "had the same advice from Dr. Handyside." The true way to state that would have been that as occasion offered he had the same advice from me. I quite concur in his answer to question 4023, and in the quoted portion of his answer to 4026. The last question that has been sent to me is this, No. 4032, "I "only want to get out the facts clearly; and it results, "from what you have stated, that the recommenda"tion made to you, although there may have been "no explicit statement about anæsthetics one way "or the other, to perform these experiments, did not "necessarily imply that you were to perform experi"ments of a painful character?" His answer is, "There was the implication that the experiments "were of a painful character, or I would not have "given the answer that I did." The statement made in the question is true.

5978. (*Mr. Hutton.*) Did I rightly understand you that you had never advised any other student *in statu pupilari* to make these kinds of experiments except Dr. Hoggan?—Not to my knowledge. I do not believe that I ever advised students to make experiments, for these reasons, that I had not found them qualified, or they had not been engaged in investigations necessitating experiments.

5979. In regard to question 3479, "Do you think "that a similar advice was given to other students?" Dr. Hoggan's answer was, "I have no doubt of it "whatever, because several students have told me. "One told me, who is now dead, that he had per"formed 10 or 15 experiments on cats shortly before "then, and recommended me strongly, as what he "had seen was so very clear, to get cats and dogs and "examine the action of the various muscles in those "animals, that I had contended in my dissertation "had not the use that was assigned to them." With regard to that answer, you state that you would, as far as your experience goes, entirely deny the drift of it?—I never read question 3479 as having any application to me; if it has I quite deny it.

5980. You will observe that the question was, "Do "you think that a similar advice was given to other "students?" And the reply was, "I have no doubt "of it whatever"?—I did not read that as applying to me; it is too wild a thing to suppose it; but if meant to apply to me I give it a positive denial.

5981. I understood Dr. Hoggan to say that he was for two years your pupil, between 1869 and 1871, and that during the whole of that time he had the same kind of advice from you. At question 4022, Mr. Huxley asked him this question: "I think you stated that "Dr. Handyside advised you to conduct this experi"mental inquiry, because you had stated to him that

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" you had new views upon respiration?" And his answer is, "I had done so during two medical years, " and I constantly had the same advice from Dr. Han- " dyside." There I think you confirm his statement? —To Mr. Huxley's previous question 3491 "How " long had you been a student," Mr. Hoggan answers, "I may say three years." He had been my regular pupil during 1868–9, the summer 1869, also during 1869–70, and summer 1870. From the time that he had these views upon the respiratory muscles, I certainly, as occasion offered, encouraged him to perform experiments.

5982. Dr. Sinclair to-day dates that as 1869?—It is quite possible, because he was an ingenious man, and he was disallowing my explanation of the functions of the *serratus magnus* in conjunction with inspiration. I said to him at the very commencement, " *A priori* I think the argument is against you. It " is possible that you may be right, but you cannot " bring it out without experiments on living animals;" and he had liberty from me to perform certain experiments of a mechanical nature to prove on a dead human body how insignificant the heart's suction power is. He left off speaking to me because he found that I was so opposed to his theory; and it was discussed in the Medical Society, in the whole house, who, I understand, were not favourably disposed to it.

5983. In 1869 would not Dr. Hoggan have been a young student rather than an old one?—I was working at the time at the gorilla and chimpanzee, porpoise, potto, &c., and I found that, being neat-handed, he could assist me. He assisted me in my microscopic adjustments, was a good deal about me in the museum, and I gained confidence in his mechanical dexterity and his intelligence. He became, I think, an assistant in conducting a tutorial class in the first instance, then my assistant in the dissecting rooms during the summer and autumn of 1870, and from thence I promoted him to be my demonstrator at a nominal salary for winter session 1870–71, when his engagement came to a close and he left me for good; so that his answer to question 3485 can have no application to me. I found, when I could not get another diplomated demonstrator at the time that Dr. Sinclair left me in 1870, that I could trust Dr. Hoggan. I had at that time eight women attending my public lectures and dissecting room, and he gave most entire satisfaction to all the pupils in his demonstrations and dissections.

5984. Still, in 1869, he would have been young in his course?—Yes; perhaps so. At the time he was my demonstrator he had just completed his full course of anatomy. He got his certificates from me as a pupil afterwards. On his reminding me that he had not got his certificates of class attendance, I gave him these. I had employed him as a demonstrator for six months, and as such had reason to be decidedly satisfied with him.

5985. If, in 1869, he was advised to make these experiments on living animals, he was rather young for that purpose, was he not, as a medical student?—If you have confidence in a man, you can employ him very early if he shows intelligence, and if he shows that he is up in the knowledge of anatomy. But I am not aware at this moment that he ever performed a single experiment on an animal. I gave him the advice, but he never told me that he had ever tried the thing. I said, "To do anything, Mr. Hoggan, you must prove it by experiments."

5986. You have given very strong evidence as to the humane feeling towards animals in Edinburgh generally; do you mean that to apply to frogs also?—I do indeed.

5987. That frogs would not be operated upon, except under anæsthetics, where anæsthetics could properly be used?—I would rather employ anæsthetics upon frogs. I must admit I have not done so, because I have not put them to any pain in stretching the web, or for the purpose of showing the circulation.

5988. But in more severe experiments I mean?—I would always use anæsthetics in them.

5989. Do you think that that is the prevailing feeling in Edinburgh, because we have just had the contrary evidence?—To my knowledge anæsthetics are always given, and I have never seen experiments on frogs without the equivalent being employed, of cutting off their heads.

5990. (*Mr. Forster.*) And you have seen the experiments performed with anæsthetics on frogs, have you?—No, I have not. What I have seen at the Royal Society performed by the late Dr. Bennett and Dr. Rutherford together, when he was his assistant, were experiments on frogs when the head was previously cut off, and then currents of galvanism passed through and other experiments performed on them; but I have not seen the experiments referred to by Sir Alexander Grant in the return which is before you; I have myself not used anæsthetics with frogs, to show the circulation of the blood; but if I thought that these experiments gave pain I would give chloroform.

5991. (*Mr. Hutton.*) You have not performed any more severe experiments with it?—No; I have limited myself to anatomy chiefly, with the exception of 12 of the previous years that I taught physiology also.

5992. (*Chairman.*) What I understand you to say is that, in your opinion, a cold-blooded animal like the frog ought to be included among the animals which are to be anæsthetised if experiments in their nature painful are performed?—Clearly so; I certainly hold that.

5993. And I see in the return which has been sent to us by Sir Alexander Grant it is stated that 133 frogs got curari. Do you consider curari an anæsthetic?—I have no experience with curari.

5994. Is there anything else which you wish to say?—Firstly, I would simply refer to the results of my own experience as a student from 1825 to 1831 in operations on horses and dogs, upon the injection of air into the veins; ligature of the aorta and other vessels; upon the effects produced on domestic animals, by lactucarium, with the view to ascertain its curative properties on man; and upon the share that the different absorbent vessels respectively have in taking up noxious foreign matters. Secondly, as a teacher of anatomy, physiology, and surgery, I followed up my experimental researches on absorption. I also assisted the late Sir James Simpson in experiments on swine, and Mr. Spence in experiments, I believe, upon dogs. Thirdly, I had experience from 1837 till about the year 1854, as secretary of the Harveian Society, instituted in 1782 by Dr. Duncan, and one of its chief objects being to promote experimental research, and composed of such men as the late Drs. Alison, Rutherford, Huie, Sir James Simpson, and others, all tender-hearted and humane men, who were opposed to the practice of inhumanity towards animals. This society is instrumental in encouraging the production of such prize experimental essays as that upon lactucarium just alluded to, that of Dr. Madden of Torquay upon cutaneous absorption, and the essay of Dr. Mortimer Glover of Newcastle on compounds of bromine, and chlorine, including chloroform (to which last has been ascribed Sir James Simpson's employment of chloroform). These and other subsequent essays have elicited information of the greatest use for the relief of pain and suffering in man; for the prolongation of man's life in disease and old age; and for his extrication from the peril of sudden death. Fourthly, I have had experience as having been requested to undertake the office of honorary secretary to the Scottish Society for the Prevention of Cruelty to Animals in 1839; and having much sympathy with these animals, I felt that there was nothing inconsistent with this in my practising vivisection, but that it was justifiable in me to do so, seeing that there was no deliberate pain inflicted by me upon these animals. It was not willingly done; it was not uncompelled by duty; but it was my duty as a professional man to vivisect these animals, with which I had much sympathy, and quite justifiable; seeing it is only in that way that an empyrical inexact science like medicine can be advanced. Upon man himself antimony, aconite, and strychnine have been used experi-

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mentally with most dangerous results; and trials with the Calabar bean and other articles of the materia medica made on man, that properly ought to have been previously made on animals, have nearly cost us the lives of more than one eminent medical man. "How much is a man better than a sheep!" therefore we are at liberty to perform these needful experiments on the lower animals; *fiat experimentum in corpore vili:* and upon that principle "Ye are of more value "than many sparrows," I think it is quite justifiable for us so to operate. This we can do consistently with the most humane and merciful feelings towards the lower creation.

The witness withdrew.

Mr. William Williams called in and examined.

Mr. W. Williams.

5995. (*Chairman.*) You are the principal of the New Veterinary College at Edinburgh?—I am.

5996. You have had the opportunity, I think, of seeing some mention that has been made of your name before us by Mr. Mills?—I have.

5997. Have you any observation to make upon that evidence?—Simply that I know nothing about the operations referred to at Question 4949 in the statement "The principal, I believe, knew of it."

5998. Then in answer to what is said under head 4949, you say that you did not know of anything of the sort?—No.

5999. Do you believe that anything of the sort occurred?—I think so; I have heard something.

6000. Is it then, as we understand you now, an abuse that took place?—Certainly.

6001. And do you suppose that it was an isolated case of abuse, or are such abuses common in the Veterinary College at Edinburgh?—It is the only one that I have ever heard of.

6002. Have you made inquiry since you heard of this, to know whether any other such abuses have taken place?—I have.

6003. And what is the result of that inquiry?—This is the only instance that I can find.

6004. Are the regulations of the Veterinary College such that you might have expected that such an abuse would be brought to your knowledge?—It is a difficult matter to get students to tell upon one another.

6005. Then are we to suppose that in the Veterinary College at Edinburgh abuses of this kind might frequently take place?—Not frequently, certainly.

6006. Do you think that you might be able to make your regulations such as to prevent the possibility, or at any rate the likelihood, of anything of the kind occurring again?—I think so.

6007. Now in the answer to question 4955 it is said that the practice must have been "lasting at least for the last five years"?—That is untrue; because five years ago I was at the old college, and the space of the yard there was much more limited.

6008. Now is the whole story at all conformable to what is the practice at the Veterinary College at Edinburgh?—Certainly not.

6009. Do you purchase a horse for the purpose of dissection?—Yes.

6010. And then, before you kill him, permit experiments upon his living body to be performed?—Certainly not, with the exception of the experiment for the stone, which I perform myself.

6011. You perform an operation for the stone, not for the cure of the horse, as I understand it?—No; the horse is bought for dissection.

6012. And while he is still living you perform experimentally upon him an operation for the stone?—Yes; I do.

6013. And do you use any anæsthetic then?—Always.

6014. What anæsthetic?—Chloroform.

6015. Have you any other observation to make upon the statements of Mr. Mills which have been communicated to you?—He says here that the animals bought were dogs and cats; that is at 4927. I can safely say that I never performed an experiment upon a cat or a dog in my life myself, nor am I aware of any student having done so. I think I am in a position to state that, at least within the college, and within the knowledge of the college authorities, that no students ever perform on cats and dogs.

6016. (*Lord Winmarleigh.*) What is the nature of the building?—There is a large mansion house at the new college for the lectures and so on, and at one side I have some loose boxes and a dissecting room.

6017. Is there a yard?—There is a large space, and there is a paddock and a garden of about five acres of ground behind.

6018. Supposing that a horse was to be kept for experiments of this kind, do you think it could be done without your knowledge?—Certainly not; if I were at home I should know of it.

6019. Are you much from home?—I am a great deal from home. I have a large consulting practice in Scotland.

6020. You cannot state therefore whether this horse was brought into the college?—I have made inquiry, and the professor of anatomy tells me that he knows nothing about it.

6021. Is he always present?—Yes.

6022. How do you account for our being told that the horse had been there three days?—He might have been during the Christmas holidays. I cannot fix upon the time; I cannot make it out. But I understand that the horse had his tendons divided, and had had the nerve operation performed upon him in the paddock behind the building.

6023. And from your inquiries is it a fact that he was allowed to be there three days during the operation?—I cannot speak to that. I think it is hearsay evidence that Mr. Mills adduces.

6024. May I ask you what the rules of your college are? Is it ever allowed for students, or any other parties connected with the college, to bring in animals and to operate upon them without your knowledge?—No; except to bleed. They are allowed to bleed them.

6025. Then, if this has taken place it is contrary to the rules of your college?—Certainly.

6026. And how do you account for its never having come to your knowledge?—I never heard a word of it till I heard Mr. Mills' evidence, and I made inquiry, and it was stated to me by an old groom that he believes it was done.

6027. The result of your inquiry is that you do believe the thing was done?—Yes.

6028. And that Mr. Mills' evidence is correct as to the fact?—In so far. It is exaggerated, I think, as to the cruelty.

6029. What you do deny is that you knew anything about the matter?—Yes.

6030. The Commission may believe that the thing did take place?—Yes.

6031. And that Mr. Mills stated it with a little exaggeration?—Yes.

6032. (*Chairman.*) Do you confirm this (I am reading from Question 4957): "The subject that was "operated upon was a horse, and it was bought for "the purpose of dissecting. This animal was sub-"jected during a whole week to operations, such "as tenotomy and neurotomy, and various minor "operations"?—I believe that the operations were performed, but I do not think that the animal was allowed to live such a time as Mr. Mills states.

6033. "(*Q.*) Were those operations performed under "chloroform?—(*A.*) No; no chloroform whatever was "given. (*Q.*) Were no anæsthetics given?—(*A.*) "No; none whatever. (*Q.*) Were they painful "operations?—(*A.*) Very. The animal was cast by "means of hobbles. (*Q.*) And were they operations "chiefly on the nerves?—(*A.*) Nerves and tendons.

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"And the animal was bled in different parts of the "body, for the purpose of the students simply de- "monstrating to each other things that could be "learnt in every-day practice." Now do you confirm the whole of that?—I believe that the nerves of the fore legs were divided, and I think the tendon of one leg; but I do not think anything further. The horse might have been bled. Students have liberty to bleed horses, to perform the operation, because I am of opinion that it entails no suffering on the animal, and that it is essential in every-day practice that they should be able to bleed expeditiously.

6034. Do you mean that if a horse is purchased by you and put into your paddock, or one of your boxes, the students can of their own motion, and without special sanction from you, go and perform tenotomy or neurotomy, or bleeding, whenever they please?—They can bleed with a phleme.

6035. As much as they like?—They can bleed once or twice. They generally bleed from the jugular.

6036. Do I rightly understand that if an animal is brought into your paddock and is intended to be killed, perhaps, a week hence—— ?—That is an exception to the rule. They are bought and killed the same day, as a rule.

6037. Supposing that the horse is to be killed the same day or the next day, may the students go and occupy themselves with bleeding him?—They can bleed him in the jugular vein.

6038. As many of them as like?—It generally belongs to one or two of the students. We never buy the subjects; the students themselves buy it.

6039. And is there no control at all on your part or the part of any of the professors as to the use made of an animal when he gets there?—Certainly; he is killed for dissection.

6040. Supposing a student buys a horse, and operates upon that horse, is there any control whatever exercised, in the sense of taking care that that horse is not improperly operated upon, cruelly operated upon?—Certainly, every care is taken of it; but they are allowed to bleed with a phleme. That is the only operation permitted.

6041. As often as they please?—They would not bleed him above once or twice. They may open both of the jugulars.

6042. (*Lord Winmarleigh.*) Supposing you had found out that this had taken place, what course should you have taken?—In all probability I should have expelled the student.

6043. Have you ever had occasion to expel a student?—Never.

6044. Can you of your own authority expel a student?—I can.

6045. (*Chairman.*) In your absence on journies in other parts of Scotland, is there any representative left by you to exercise discipline?—Yes; Mr Vaughan, Professor of Anatomy, Dr. Young, who lectures on anatomy, my clinical assistant, Mr. Johnston.

6046. And is any one of these charged especially with responsibility in regard to the humane treatment of any animals that may be there?—The clinica assistant, Mr. Johnston.

6047. Then if a student buys an animal for the purpose of dissection, he can, as I understand you, bleed him, if he likes, before, but he cannot do anything else until the animal has been killed?—No.

6048. If then tenotomy, or neurotomy, or any of these operations were performed, it was in direct violation of the rules?—Yes.

6049. And the person who in your absence was responsible for discipline, is the person to call upon for an explanation as to how it happened?—Yes.

6050. Have you called upon him for that explanation?—Yes; I asked Mr. Vaughan.

6051. Was Mr. Vaughan the person responsible at that time?—Yes.

6052. He had not heard at all of the case?—He had not.

6053. And do you suppose it is likely that such cases can happen without you, if you are there, or Mr. Vaughan, if he is the person responsible, knowing something about it?—This is the only case that I ever heard of.

6054. And the only case you believe to have happened?—Yes.

6055. Now, with regard to the treatment of subjects in the Veterinary College, am to I take it from you that their humane treatment is an object with you and with those who act with you?—Certainly.

6056. That when an experiment like that of lithotomy has to be performed upon them, anæsthetics are used first?—Certainly.

6057. And that the animal would be put out of his pain at the end of the experiment and before the anæsthetic had ceased to operate?—Yes, with the exception mentioned by Mr. Mills here (he seems in choosing the cases to have suited his own purpose), that this horse was allowed to live some hours after the operation was performed.

6058. Now was that done?—Yes, it was.

6059. And was there any good reason for that?—Yes.

6060. What was that reason?—It was stated that no horse could recover after having had chloroform, and it was done for the purpose of demonstrating to the students that the horse did regain its consciousness.

6061. Then it was a demonstrative experiment, not an experiment to ascertain any new truth, but to demonstrate what you knew before to the students?—It was to disprove a fallacy.

6062. It was not to satisfy you?—No; but there was a strong feeling amongst the students that session, in consequence of something which had been said by an old veterinary surgeon, that chloroform was always very injurious, and that no horse ever regained consciousness after its administration.

6063. Was the state of the horse during this 12 or 14 hours such as to make it a state of great agony?—Certainly not. The horse was eating.

6064. (*Mr. Forster.*) When was it that you discovered that this horse had been treated in the way that Mr. Mills says; I refer to the first horse mentioned, which was so treated in your absence?—By inquiry after I saw Mr. Mills's evidence.

6065. And what did you hear did really happen to it?—I heard that they had divided its tendons, and that they performed the nerve operation upon him.

6066. Did you hear how long he had been subjected to the operations?—No; it was only a kind of vague report from an old groom whom I had.

6067. You did not hear it from one of the students?—No.

6068. And could not he tell you the time that the horse was so treated?—No, he could not, but he was of an opinion it was during the Christmas vacation.

6069. Had he seen it so treated?—I do not know whether he had seen the operations, but he said that he believed that these operations had been performed.

6070. Will you be good enough to look at the answer to question 4997, in which Mr. Mills speaks of a second operation upon a living animal performed by yourself. The first we have already heard about, and that was a case of lithotomy. What was the fact with regard to this second case which you will find in 4997?—I destroyed one subject by blowing the jugular vein.

6071. You observe that it is stated that the animal was not put under chloroform; I suppose that is true, is it not?—Quite true.

6072. How quickly was it killed?—It died, I should think, in about 30 seconds.

6073. Just look at question 5003. "What was the "effect of the operation? (*A.*) The animal stag- "gered for a time and then regained his feet, and "then they pinned up the vessel and allowed the "animal to live for some time before he was destroyed?" —That is untrue. The horse fell, and after he fell, in order that he might be made use of for dissection, the

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carotid artery was opened at once, and he immediately was bled.

6074. Then you remember the experiment ?—I do.

6075. And it is untrue that the animal was allowed to live any appreciable amount of time ?—Yes.

6076. (*Mr. Huxley.*) Your purpose in performing that experiment, I suppose, was to show the danger of allowing air to enter a vein in the course of bleeding ?—Yes, and also to demonstrate that it was by far the easiest method of destroying animals.

6077. It is a very swift method ?—Most swift.

6078. (*Mr. Forster.*) Now if you look at the evidence from questions 5034 to 5040, there is a statement there that you operated upon a horse bought for the purposes of dissection by some experiment on the tendons of the near fore leg. What have you to say about that ?—I remember nothing about that. I often perform tenotomy upon horses brought in for treatment.

6079. But do you put them under chloroform ?—No ; certainly not.

6080. Will you kindly give us a description of the operation ?—The tendons of the limb contract ; the horse walks upon his toe, and is unable to bring his heel to the ground, and for the purpose of enabling him to bring his heel to the ground the tendons are divided across under the skin. It is an exceedingly simple operation and almost painless.

6081. (*Mr. Huxley.*) It is performed on the human subject for a club foot ?—Yes.

6082. (*Mr. Forster.*) This mentions three cases in which experiments were performed on horses. Is it your custom to perform experiments on horses bought for the purposes of dissection ?—With the exception of operating for the stone, certainly not.

6083. But to some extent you do do so ?—I perform ithotomy.

6084. I have here before me the return which you made to the inquiries which have been sent by this Commission to many institutions. One of the questions is, "State what animals (including frogs) are " used either for original research or class demon-" stration, and give the number of each species"; the reply to which is, "Frogs only" ?—Yes ; that is written by Dr. Young. I never thought of the horse at the time, the thing really escaped my memory.

6085. In signing that you forgot it ?—Yes, I forgot entirely this operation for the stone.

6086. And you, I suppose, also forgot what had happened when you sent the next answer, in which it s said that the animals are always rendered unconscious ?—Yes.

6087. (*Sir J. B. Karslake.*) I believe in practice you never chloroform a horse for the purpose of firing him, do you ?—Well I do sometimes.

6088. Is the operation of giving anæsthetics to horses a difficult one ?—It is very difficult, a horse struggles very violently.

6089. And is it attended with any danger to the horse ?—It is attended with danger. We have to cast he horse before giving him chloroform, and he is n great danger of breaking his back during the struggles.

6090. During the time that the chloroform is being administered you mean ?—Yes.

6091. Now as compared with firing is tenotomy as painful an operation as firing ?—Oh dear no. I generally run a very small scalpel through the skin, underneath the tendons, then I put in a blunt knife hat divides the tendons, which have no sensibility whatever. The pain is simply in the division of the kin ; a mere prick in fact.

6092. Does the same observation apply to neuro-omy ?—No. In dividing the nerve there is con-iderable pain, just at the first cut.

6093. I suppose you have horses sent to you for he purpose of being cured, or rather to have pain lleviated by neurotomy ?—Yes.

6094. Do you then chloroform them ?—No, I do not.

6095. Is that a short operation ?—Very short. I have chloroformed for neurotomy, but I do not as a rule.

6096. With regard to the case that you have told us of, of tenotomy practised upon a horse that was brought in for dissection, was that the only instance, as far as you remember, of an operation being performed, with the exception of the operation for stone, upon animals sent in for dissection ?—You mean the operation by the students.

6097. No ; as I understood, you yourself are represented to have performed tenotomy upon an animal sent in for dissection ?—I do not remember ever performing tenotomy upon a subject.

6098. Do you remember the instance that was spoken to by Mr. Mills of your performing tenotomy ?—I often perform tenotomy upon animals sent in or cure.

6099. Was that upon a horse sent in for cure, or upon a horse sent in for the purposes of dissection ?—I do not remember ever having performed tenotomy on a horse sent in for the purposes of dissection.

6100. I think Mr. Mills said that he was two years under your tuition ?—He was a student for two years.

6101. Then you undertake to say that there was no horse sent in for dissection upon which tenotomy was performed before he was killed for dissection ?—Not by me.

6102. Or to your knowledge by any other person in the college ?—Not to my knowledge.

6103. Who else is there in the college that would have to perform these operations ?—My clinical assistant. As a rule I perform all the major operations myself, such as tenotomy, the division of the nerve, and firing.

6104. Those are operations on horses sent in for cure ?—Yes.

6105. Excluding those, is it the duty of your assistant to perform an operation, such as tenotomy, upon a horse sent in for dissection ?—Such a thing is never permitted.

6106. As far as you know, such a case never occurred during the time that Mr. Mills was there ?—I have no knowledge of it.

6107. You never performed it ?—No.

6108. With regard to the practice of students, is it a rare thing for a horse to be kept as long as a week before he is killed ?—Very rare.

6109. Is it since you saw this evidence that you made inquiries about the particular horse alleged to have had the operations of neurotomy and tenotomy performed upon it ?—Since I saw the evidence.

6110. Was the groom that you spoke to in your service at the time at the college ?—He was.

6111. As far as you know, was the clinical professor or any other professor conversant with the fact ?—No, they were not.

6112. As I understand, you never buy horses yourselves for the purposes of dissection ?—No.

6113. It is only when the students choose to club together, or as individuals, to buy horses, that they are bought ?—They are compelled to dissect so many subjects every session, and they buy them as they can. That has always been the rule in Edinburgh ; they must dissect so many subjects.

6114. And they must procure them themselves ?—Yes.

6115. And authority is given to the students, or to such of them as may choose to do so, to bleed the horse in each jugular vein before the horse is killed ?—Yes.

6116. And beyond that there is no permission given to them to perform any operation at all ?—None whatever.

6117 With regard to the operation for stone, is that an operation which can save a horse's life ?—Yes.

6118. And it is an operation which you are in the habit of performing for cure ?—Certainly ; it is a very rare operation, however.

6119. (*Chairman.*) Do you perform it under chloroform when it is for cure ?—Yes, I have ; but at the time of the first real operation I performed for the stone, chloroform was scarcely known.

6120. But would you perform it under chloroform? —Yes, certainly; the operation is much easier performed in that way.

6121. (*Sir J. B. Karslake.*) After the operation is performed for stone upon an animal, when it is designed to save his life, and the effect of the chloroform goes off, is the pain very considerable from the effect of the operation?—Yes, it is a painful operation; there is a wound made in tissues which are sensitive.

6122. And how long would the pain last severely? —Perhaps it would be severe for a couple of days; it would be as severe at least as the pain from any other wound.

6123. (*Chairman.*) But if the same sort of case arose in a human being, the practice would be, I presume, to use chloroform during the operation; but the human being would submit to the subsequent pain with any ordinary alleviations which might be administered?—Certainly.

6124. Then what I understand you to say is, that you treat a horse which you are operating upon for the cure of the stone exactly in the same way?—Decidedly. I do not believe that the subsequent pain is very great.

6125. (*Mr. Forster.*) I just want to call your attention, if you please, to questions 4970 down to 4977. Now you see that Mr. Mills says that animals were bought and operated upon for the purpose of exhibition and demonstration, and that it was not done at lectures, but was done during the dissecting hours, between 10 and 12, or in the afternoon; that these animals were horses and donkeys; that the operations were not frequently made, because of the difficulty of getting subjects, but that they were sometimes made by the students themselves, and sometimes by the professor. Now, do you know anything of those experiments?—With the exception of the operation for the stone, I know nothing about them.

6126. Do you believe that it was or was not true that there were those experiments made?—That it was not true.

6127. With the exception of the case for lithotomy, you believe that there were no such experiments?—Yes.

6128. Have you inquired in order to find out whether there were or were not?—Yes.

6129. And in what way have you satisfied yourself that these experiments were not made?—I have made inquiry from those about the place, and in fact I think I am in a position to state from my own personal knowledge that, with the exception of this case, which is admitted, no experiments beyond bleeding a horse have been performed within the college for a great number of years.

6130. (*Mr. Huxley.*) I perceive it is stated at question 4964, that the horse which we have had so much talk about was put into a paddock. Were there other horses with it?—No, there would be no horses in the paddock in the winter time.

6131. So that if a horse was put into that paddock, somebody must have seen it?—Yes.

6132. You were not there at the time?—I have no knowledge, except from hearsay, that the thing was done.

6133. You are, of course, perfectly familiar with all forms of veterinary terminology?—Yes.

6134. Will you tell me what this means; it is a document which is signed by Mr. Syme? "We, the " Court of Examiners for Scotland of the Royal Col- " lege of Veterinary Surgeons, desire to express our " opinion that the performance of operations on living " animals is altogether unnecessary and useless for " the purpose of causation." What does "causation" mean there?—I do not understand it; I think that must be a misprint.

6135. That document has been reproduced over and over again, and I have never been able to make anything of it; and you are unaware of any technical sense of the word?—I remember when that thing was drawn up, and I do not understand the word; I believe it must be a misprint.

6136. Can you tell us what was the ground of its being drawn up?—The experiments were performed at a French veterinary school at Alfort; it has reference to that I believe.

6137. What can be the meaning of this last phrase: "useless for the purpose of causation"?—I cannot tell; I think it is a misprint.

6138. From your knowledge of what took place on that occasion, can you express a confident belief that this protest arose out of the proceedings at Alfort, and not out of anything that took place in the three kingdoms?—I can. I remember seeing that in the "Scotsman" the following morning, and I asked those who had signed it what was the reason of it, and they said it had reference to the doings at Alfort.

6139. (*Mr. Forster.*) Do you recollect whether, when you first saw it, it had that phrase in about "causation"?—I think I do, now the thing is brought before me; but I can make inquiry.

6140. (*Mr. Hutton.*) In regard to the bleeding, do the students perform the bleeding of the jugular vein without superintendence? — The thing is so exceedingly trivial that we never thought of superintending that.

6141. If they were not superintended, is it not possible they might perform other and more painful operations without the knowledge of you and your lecturers?—I do not think so; besides I do not think they would do it.

6142. Why was the horse on which lithotomy was performed under chloroform kept so long as 12 or 14 hours? — He regained consciousness, and was destroyed the same evening. I lecture from 9 to 10. He was operated upon in the lecture room during that hour, and he had regained his consciousness before 10 o'clock. He was kept until the evening, and he ate his dinner and was quite well.

6143. I suppose the purpose you had in allowing him to regain consciousness, would have been answered if he had been destroyed in half an hour afterwards? —I do not think so. I wanted to encourage the administration of chloroform, and to overcome this prejudice which they had got hold of, that the horse did not recover from chloroform.

6144. With reference to Mr. Mills's statement that the students often performed operations in their private rooms on cats that they had snared in the streets, you are not able to state whether that happened or is likely to happen in future at all?—I have made inquiries, and I have been told by the persons I asked that they do not believe that any student with the exception of Mr. Mills ever did such a thing.

6145. Mr. Mills's statements were very explicit that others had joined with him?—They might have done so in their own lodgings, but certainly the thing is quite unknown.

6146. (*Chairman.*) Then I understand you, as the upshot of the whole business, to negative, as far as your knowledge and belief are concerned, the notion of cruelty as being practised upon animals by students in the Veterinary College at Edinburgh?—Certainly.

6147. And I understand you to say that if any such experiments are performed they are in violation of the rules?—Certainly.

6148. That you and the persons whom you have referred to in authority consider yourselves responsible for the observance of those rules in the sense of humanity?—Certainly.

6149. I have understood your answer that no experiments are performed, to cover the statement that no experiments are performed for the purpose of acquiring manual dexterity?—With the exception of bleeding.

6150. (*Mr. Hutton.*) During the Christmas holidays would the students be in residence, or would most of them be away?—They are mostly there.

6151. Then it is quite possible that the 70 or 80 students might have been engaged on this horse?—

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At the new college we never have above 60; there might have been 40 or 50 at that time.

6152. And in reference to your lecturers, are they in residence in the Christmas holidays, or not?—Some are away; they do not come to the college; they do not live there.

6153. In fact there would be less authority there during the Christmas holidays than at other times?—Certainly.

6154. (*Chairman.*) Have you anything further to say?—I have nothing further to say.

The witness withdrew.

EDWARDS CRISP, M.D., called in and examined.

E. Crisp, M.D.

6155. (*Chairman.*) You are now in practice?—I am.

6156. You wrote I think to the secretary expressing your readiness to attend and give information to this Commission?—I did so.

6157. What was the particular point which you were desirous to lay before us?—I am rather a penitent upon this question. I have been a vivisector for some time. For several years I cut into animals, removing their spleens and the thyroid glands (the glands of the neck), and performed many other experiments; and as I advanced in age, and I hope in wisdom, I saw fit to alter many opinions that I had formed at an earlier period; and I have come to the conclusion that vivisection as practised, especially on the Continent, has not led to the good that its advocates believe. I think that there are many false inferences drawn, and one especially is as to the use of chloroform. It is assumed that because chloroform or any other anæsthetic is used, little pain is felt. Now I believe that that is a very false inference; for the pain is not during the operation, but the pain is after the operation in many cases. And there is another thing of which I speak especially from my own experience, that animals that are subjected to these operations are very much neglected, I speak of the frogs, I am ashamed to say now, and rabbits and other animals which I have killed which have been neglected. I think I attended, or should have attended to, these animals myself, but I am quite sure that in laboratories and places where they are left to attendants a vast amount of cruelty is practised. So that it is not the vivisection alone, not the cutting into these animals, that is so objectionable. Then as regards the lower animals I have taken the trouble, having paid great attention to the diseases of the lower animals, to look into the foreign journals and our English journals, to see the number of operations that are really required. I happened to be a candidate for the prize given by the Society for the Prevention of Cruelty to Animals, 1864, and that led me to pay more attention to this subject, and look up authorities, and investigate the matter more fully than I should otherwise have done. I found that (as probably all know here) eight students were at about the time when I made this investigation all cutting into a living horse at Alfort, Lyons, and Toulouse in France, and performed sometimes as many as 64 operations upon this poor animal, and as far as I could learn there was no positive good attending them, not the slightest in the world. The very few operations that I found can be practised on the lower animals with advantage were amputation of the penis, which does not require vivisection, lithotomy rarely performed and rarely successful, and a very few other operations. So that positively it appeared to me that all this cruelty was unnecessary, uncalled for, and that there was no good attending it. And then I have also come to the conclusion that, in a practical point of view, very little good has resulted from vivisection as regards the treatment of disease; and my impression is, that some restriction, if possible, should be put upon its frequent and useless employment. I think that with regard to our hospitals a committee should be formed at each hospital, who should regulate and control these matters, and that all useless experiments upon animals, oft repeated, should be done away with. I am not objecting to vivisection in some cases, because if I myself saw my way clearly, by operating upon any animal to benefit the human race, then notwithstanding any law that might be passed, even with the fear of imprisonment, I should do it; and therefore I think it is utterly impossible to suppress vivisection. But I think that this inquiry will do an immense deal of good by preventing the needless and useless operations in the cutting up of animals that have been performed.

6158. Your view is that operations of this kind cannot be altogether prohibited, but that they may be very much diminished in number?—Modified; I think so. May I be allowed to mention this, and I ought to mention it, although it may appear something like puffing myself. If I had been told when I recommenced my inquiries with regard to the structure and use of the spleen, and also the structure and use of the thyroid gland, that vivisection was not to be followed, I confess I should have said I would not investigate the matter at all; and one good resulting from my labours was this. Before I commenced my investigations it was stated that no valves existed in the splenic vein or abdominal veins of any animals; but I discovered from my investigations that they were very numerous in many animals. It is an important physiological fact. I confess I should have been stopped at once if I had been told that vivisection was not to be followed. I mention that as one very slight gain, but still it is worth naming, and so with the thyroid gland. I found that instead of its being the thyroid gland in a vast number of animals, there were two separate glands on each side of the neck. These are matters of no practical advantage as regards the treatment of disease, but still there is no doubt that in these investigations you are stimulated by emulation. I do not say that I had any desire to benefit my fellow creatures, but I was anxious to get a prize, and I wanted to beat other men, and I believe that is the prevailing feeling.

6159. (*Mr. Hutton.*) Will you tell us of any instance that you know of investigations of this kind which you think have been needless and very productive of pain to animals?—I will mention one especially that I am well acquainted with. I may mention, for the benefit of those who are not acquainted with anatomy, that there is a body which we call the supra-renal capsule; wrongly called supra-renal, and wrongly called capsule, because in many animals, as I discovered, it is quite away from the kidney, and in others not above the kidney. When Dr. Addison made a most important discovery with regard to these bodies, namely, that when they were diseased in a certain way death invariably followed, and the skin was discoloured in a peculiar manner, men naturally set to work to extirpate these little bodies, and to see the effect of this extirpation. Phillipeaux, Brown-Séquard, and a great many others did this. Dr. G. Harley, who obtained the prize at the Royal College of Surgeons for the best essay on the structure and use of these glands, 1858, came to a very erroneous conclusion about this disease which Addison had so well described, and which is now as well established as Bright's disease. He ignored the fact altogether, and then he came also to the conclusion, because the supra-renal capsules of a rat had been removed, that this rat after three years, when it became blind, died from old age, and he showed this to the Pathological Society,* of which I am a member. Not knowing at all that the removal

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* The preparation is in the Museum of the Royal College of Surgeons, Lincoln's Inn Fields.

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of the capsules really shortened the animal's life, he came to the conclusion that the rat died at the end of three years from old age. Now the rat lives from 15 to 20 years. This is one instance which I mention; and I could name many others, more especially in regard to the cerebellum. The cerebellum has been sliced in all directions and in various ways, and various conclusions, as is well known, have been drawn; very opposite conclusions indeed. Although, as I said before, I should be very sorry to see vivisection done away with altogether, I believe its advocates have very much exaggerated the good resulting from it, and that some restriction should be put upon the practice. Unfortunately in this country we have almost as many teachers as students. I believe that the students will be about four to each teacher, scarcely that; and we have no central head, no faculty of medicine and surgery, no authority that could be consulted. If we had, perhaps the matter could be better dealt with.

6160. Can you tell me whether these operations of removing the supra-renal capsules are very painful ones ?—They must necessarily be painful.

6161. Were they done, or can they be done, under chloroform ?—Yes, they can be entirely so, and they were done under chloroform. But then you see, of course, operations upon the abdomen would produce a certain amount of inflammation afterwards, and the pain would not be during the operation but during the healing process, and this operation was almost invariably fatal. But I hope I may be allowed to say this, that I do not believe that any medical man has ever undertaken a vivisection without believing that some good would result from it. He has been mistaken—I think that I was sometimes; but I believe that all men who have undertaken these operations have done so under the impression that they might benefit their fellow creatures and promote the advancement of science. I do not think that any man would be so cruel as to do anything of the kind without he believed that he had a good motive.

The witness withdrew.

Adjourned to to-morrow at 12 o'clock.

Wednesday, 15th December 1875.

PRESENT:

THE RIGHT HON. VISCOUNT CARDWELL, IN THE CHAIR.

The Right Hon. LORD WINMARLEIGH.
The Right Hon. W. E. FORSTER, M.P.
Sir J. B. KARSLAKE, M.P.
THOMAS HENRY HUXLEY, Esq.
JOHN ERIC ERICHSEN, Esq.
RICHARD HOLT HUTTON, Esq.

N. BAKER, Esq. Secretary.

Mr. J. M. Holt, M.P.

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Mr. JAMES MADEN HOLT, M.P., called in and examined.

6162. (*Chairman.*) I think you are a member of the committee of the society which has been formed under the name of the Society for the Total Abolition of Vivisection, are you not ?—I am.

6163. Are you well acquainted with the proceedings of the society ?—I am not.

6164. But you are one of the committee ?—I am one of the committee.

6165. How often does the committee meet ?—I am afraid I cannot throw much light upon the subject, for I have not attended a committee meeting in consequence of none being held in London.

6166. Where are they held ?—I think the committee meets very seldom; the members of the committee live at great distances from each other, and I do not think that they are able to come together often.

6167. Then it is rather desirable, is it not, that we should know what the authority of the proceedings of the society is. From whom does the secretary receive his authority ?—The communications which have passed between the members of the committee so far as I am concerned (I cannot speak for anything else) have been by post.

6168. Have you taken a considerable part in the management of the society ?—I have not.

6169. Do you know who has ?—I do not. I have communicated chiefly with Mr. William Harrison, and recently with Sir George Duckett. Would your Lordship allow me to say that I rather objected to join the committee, and wished to be placed as a vice-president or something of that sort in an irresponsible position; and when I did join the committee, I distinctly stated that I should be unable to take much part in its proceedings. I took the position provisionally, and have been endeavouring to get a reorganisation of the society, which has not yet been effected.

6170. The present organisation is not satisfactory to you ?—Not at all.

6171. In what respects is it unsatisfactory ?—Just in that, we do not meet regularly and in some central position which we can all easily reach.

6172. Have you paid much attention to the details of this subject which we are appointed to inquire into ? —I have not. It was my intention as soon as Parliament rose to devote myself to the subject. I was in bed with fever when Parliament rose, and I am sorry to say that subsequent weakness has defeated my intentions altogether.

6173. I am afraid we must consider your authority rather as expressing a strong sentiment of humanity than a detailed acquaintance with the subject which has been submitted to us ?—Any evidence which I can offer must be restricted to the opinion which I have formed, and the considerations which have led me to form it. That I am prepared to give.

6174. Will you be so good as to do so ?—I have made a few memoranda in order to make my statement more connected, that I might bring the subject before the Commission with less diffuseness, and if it be your wish I will put it before you in that form.

6175. If you please, we shall be much obliged to you ?—I share in common with others the pain and indignation with which the country has heard of the introduction into medical schools of the practice of vivisection in this country; and without indiscriminately condemning the whole of the medical profession, I cannot help agreeing with those who think that it inflicts a foul stain on the profession, that the governing bodies of those schools have not interfered earlier than this to prevent it, or to check it in some way or other. I would put before the Commission in the first place, the general objections which I entertain to vivisection, and then some special reasons why I prefer abolition to any attempts at restriction; and then I would enumerate a few consequences which I think will follow from the spread of the practice amongst us. In the first place I must go to the root of the whole thing. I think it is unjustifiable, because I regard it as an abuse of man's power over

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the animals. I do not think, so far as I am able to judge, that it is contained in the original grant by God to man. The grant of power over the animals was not without restriction, because although man was to have the dominion over them, he was not permitted to use them for food. Therefore it is perfectly plain to me that we must not take the words of the grant in their very widest sense; that we must not take them without some limitation or restriction. The grant of the use of animals for food was subsequent to the deluge. Further I think that it is contrary to such indications as we have in scripture of God's will concerning our treatment of His creatures. I do not know that I need enumerate to the Commission, unless your Lordship asks me, the particular instances to which I refer, but I have them noted here; and further than that, it appears to me to be based on a principle which is altogether unsafe. That principle as it seems to me, is that for the purpose of scientific inquiry men may take what steps they choose without regard to the cost or the consequences; and that is a principle which I could not for one moment accept. I think also that it is contrary to the existing legislation which Parliament has sanctioned in this country. We have imposed a penalty on cruelty to animals, and we do not look upon motive at all. A man who overloads his donkey may say that he is doing it for the purpose of gaining a livelihood for his family; and there may be a good deal of truth in that plea;—the animal is required to work up to and beyond its legitimate strength in order to meet the requirements of the man's family; but in the interests of order and public good we impose a penalty on him; and I cannot see that there is any greater obligation upon us to provide means for the healing of our bodies than there is for the keeping of them alive in the ordinary daily course of life. Then so far as I have been able to acquaint myself with the subject, it seems to me that this practice is of very uncertain value; the faculty are not at all agreed upon the subject. I believe that the three instances upon which the supporters of vivisection rely to establish the fact that it is of service, are the discovery of the circulation of the blood by Harvey, the discovery of, I think it is, the double function of the nerves of the spinal cord, by Sir Charles Bell; and also the use of chloroform, by Sir James Simpson. But if I am correctly informed, so far as I have been able to investigate the subject, there is very great doubt as to whether vivisection really brought about any one of these discoveries. I have a few extracts here taken from a book entitled "Plea for Mercy to Animals," by Dr. James Macaulay, which I have relied upon a great deal in support of the opinion which I have formed; and I find at page 138 of that book an extract from the Honourable Robert Boyle's writings, to the effect that Harvey did not say that vivisection had enabled him to establish the theory of the circulation of the blood, but that anatomical research had brought it about, and that vivisection had been used for the purpose, as I understand it, of demonstrating to others that which he had arrived at by other means in the first instance. Then I understand that Sir Charles Bell distinctly denied that he had arrived at his results from vivisection. He discovered them by anatomical research, and he too had recourse to vivisection in order to overcome the prejudices of the profession;—the unwillingness of the profession to accept his discovery.

6176. (*Lord Winmarleigh.*) When does Sir Charles Bell say that?—Dr. Macaulay says, at page 140, "I have lately conversed on the subject with "Mr. Shaw, honorary surgeon of Middlesex Hospital, "Sir Charles Bell's friend and relative, and the able "editor and expositor of his published researches. "Mr. Shaw tells me that Sir Charles always spoke "of his discovery as due entirely to anatomical research." Then I understand from Dr. Macaulay's book that it is quite a mistake to suppose that Sir James Simpson discovered the use of chloroform by vivisection or by experiments on animals. Dr. Macaulay says, at page 138, "The fact is, the use of "chloroform was the result of an experiment, but "it was an experiment, and rather a perilous one, "tried by Sir James Simpson upon himself, and by "his assistant Dr. Keith." For that of course I have simply Dr. Macaulay's authority. Then I think that vivisection is further of very uncertain value, and I am supported in that by the opinion of Dr. Barclay on Muscular Motions, at page 298. He states,—that which I think one may easily conceive to be correct,—that the circumstances inseparable from vivisection must necessarily create a good deal of irritation of the parts and render the phenomena not altogether natural. Consequently in so far as they are not natural they must be misleading. I would submit also to the Commission that it is contrary to the tenor of our Christian civilization and dangerous to the moral tone of society. If we accustom the people of this country to hear of, to read of, and to think of cruelty in any form as allowable, the result will be that we shall encourage those savage unfeeling habits which it is the object of all civilized nations to repress; and I cannot help fearing that in the result there will be a reaction upon society in the form of making men more and more regardless of the feelings of their fellow men and their families. Then I submit further that it is hardening to students. It seems to me to have a tendency to destroy that tenderness of feeling which one wants to find in a medical man, and I think it leads him rather to look upon the patient whom he is attending as a case interesting to science, than as a fellow creature who is suffering and requires relief. For these reasons I have come to the conclusion, which I find expressed in the "Spectator" of the 15th of May of this year, that no alleged benefit which is derivable from vivisection can afford sufficient sanction for it. Of course these reasons go to the total abolition and not to the restriction of the practice. But in addition to these reasons I think there are some special reasons why the imposition of restrictions is undesirable. I for one want to know that medical men are not educated in a manner calculated to inspire distrust. It seems to me if I call in a medical man to attend a member of my family, and I know that he has been educated at a medical school where vivisection is practised, that instead of receiving him as I ought to do and placing full confidence in the recommendations which he may give to me, I shall be disposed to look upon everything with distrust and shall suspect he is likely to experiment on the patient instead of simply endeavouring to afford relief. I believe that the proposed mode of restriction,—that at any rate which has been suggested for the consideration of the country by the two bills which have been before Parliament,—is to be by an enforcement of the administration of anæsthetics. Now I do not think that satisfactory; because I do not see how you can with any certainty enforce the administration of an anæsthetic. There will be a very great amount of opportunity for evasion. I am told that there is a great deal of difficulty sometimes in administering an anæsthetic to an animal. Further I think it would be uncertain in its effects; you cannot be quite sure that the animal is insensible after the administration of the anæsthetic. And I would specially call the attention of the Commission to this. There is one alleged anæsthetic, curari, respecting which the profession is not at all agreed as to the effect which it has in destroying sensation in an animal. I contend that we can never be quite certain as to the particular effect which an anæsthetic may have upon an animal. There may be something beyond the reach of human ken with reference to any particular animal, so that an anæsthetic for some reason or other does not operate with it as it would ordinarily operate with other animals or with human beings. Again I urge that an anæsthetic is temporary in its operation; that it cannot be relied upon to last throughout the whole of the experiment; and that when the animal comes to consciousness it retains all the suffering which may remain as a consequence of the experiment which has been performed, and the anæsthetic will not have the effect of removing that. I am quite aware that that

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objection might be met by ordering the animal to be killed immediately after the operation was performed; but still that would not quite meet this part of the objection which I raise, that the operation of the anæsthetic may cease to be effective before the experiment is at an end. It requires a scientific mind perhaps to deal with that question, but I feel a strong difficulty with reference to it. It is a serious objection in my mind, one upon which I should require a scientific explanation before I could dismiss it. Then I suppose, so far as I understand the matter, that in many cases the administration of the anæsthetic would be impossible, that is to say, that the effect of the experiment would be destroyed by it, and therefore restriction could not avail in that case. And I must also urge that even under anæsthetics, it would be very little less hardening to those who are to witness the experiment or to conduct the experiment, than when an animal is not subjected to anæsthetics. It is one of the misfortunes of the medical profession that the constant familiarity with suffering and with disease in its different forms and with the operations which must necessarily be performed, tends rather to blunt the feelings than otherwise. It may be very fortunate for the medical man that it is so, but it is not always fortunate for the patient. I repeat I think that restriction would be unreliable, and I do not see that even the most careful measures which might be adopted for the purpose of limiting the practise of vivisection would really remove from the public mind the objection which is felt by many persons towards it. On these grounds, finding that there is a very general consent on the part of medical men, not perhaps unanimous, that it is not desirable to continue vivisection for the purpose of demonstrations in hospital lectures, and also that considerable doubt is thrown on the value of those experiments which are conducted for the purpose of physiological research, and considering the difficulties attending the enforcement of restriction, I come to the conclusion that Parliament would do well to abolish the practice altogether,—to forbid it under heavy penalties. I would further call the attention of the Commission to the consequences which are likely to ensue if it prevails. I think they would be very disastrous both to the medical profession and to the country. I have already spoken of the effect of the practice being in my opinion hardening, and the effect of the principle being very objectionable, namely, that in the mind of the medical man the interests of science become of more importance than the immediate well-being of the patient; and I think the result must necessarily be (it is in my own case, and I know it is in the case of many others with whom I have conversed upon the subject) that there will not merely be a want of confidence in, but an absolute distrust of any medical man who was known to be favourable to vivisection, or to have employed it. And that will lead to a sort of exclusive dealing in the medical profession. I know at this moment that there are persons who will not consult a doctor until he has signed his name to a card saying that he objects to vivisection; and I do not think it is desirable that that should prevail throughout the country. Then I think it will lead to continued agitation, and probably that agitation might take the form of a sort of liberation society,—an endeavour to do away with the monopoly which at present medical men enjoy. There is a good deal of feeling in some parts of the country, and in the north of England especially, on the question of vaccination, and I think that the whole of these things will be mixed up together, and that we shall find an agitation prevail which may perhaps make itself felt on the hustings with reference to the exclusive privileges which the medical profession enjoy in this country. I do not know that that would be at all desirable. I think I have now gone through the memoranda that I have made. The conclusion at which I have arrived is that with which I commenced, that abolition is the thing which would be most satisfactory to my own mind, and I believe to the minds of a very large number of persons in this country; and I do not think, even if it were established that certain benefits could be obtained by means of vivisection, that any humane person would consent to be healed on those terms.

6177. Do you belong as well to the Society for the Prevention of Cruelty to Animals as to the society on behalf of which you now appear?—I am not an annual subscriber; I have subscribed to it.

6178. What was the cause of your society being instituted? Was there any other cause than that which you have just assigned, or was it that there was any dissatisfaction on the part of a particular portion of the public with the proceedings of the society already in operation for preventing cruelty to animals?—I believe it was felt that the Society for the Prevention of Cruelty was somewhat slack in its movements upon this question, and that it was desirable to take some steps which should bring public opinion more directly to bear upon vivisection.

6179. Since the existence of your society has it been mainly supported by the public?—Yes; I think it has been supported to a large extent by the public.

6180. Could you tell us about the number of subscribers at the present moment?—No, I am afraid I could not. I must again say to your Lordship that the fact of my having been so poorly has prevented me from taking any part in the matter, or from really acquiring the information on the subject which I had relied on the vacation to work up.

6181. But you believe that you are generally supported by the public?—I believe that we are very largely supported.

6182. That the object of the society is supported by a great body of the public?—I do think so.

6183. The Society for the Prevention of Cruelty to Animals, I believe, has a very large support throughout the country?—I believe so.

6184–5. Do you think it is greater than that which your society receives?—The Prevention of Cruelty Society has been established a much longer time and it has therefore a much greater support, I could not say whether it has a greater support than we have on this particular question.

6186. Are you aware that the Society for the Prevention of Cruelty to Animals sanctioned a Bill in Parliament by which, under certain severe restrictions, vivisection is tolerated?—Yes.

6187. That that society, established for the prevention of cruelty to animals, do recognise the necessity of vivisection to a certain extent?—I am aware that they have not felt themselves prepared to go in for abolition.

6188. That they have approved a bill in which this is inserted,—"That no person shall perform or cause "to be performed, or take part in performing, any "vivisection upon any animal without having first of "all subjected such animal to the influence of an anæs"thetic so as to render it wholly insensible to pain." And then the next section provides,—"That no person "who shall perform or cause to be performed, or take "part in performing, any vivisection upon an animal "so subjected as aforesaid shall omit to destroy such "animal before the effect of the anæsthetic ceases." That you disapprove; you go much beyond that?—Yes, that I disapprove.

6189. Is your society, then, against the infliction of pain of any kind upon animals?—Unjustifiable pain, unnecessary pain.

6190. Supposing that some of the facts which you have stated to this Commission were disproved to the satisfaction of this Commission, and in their belief to the satisfaction of the public, you would not persist then, I presume, in the position which you have laid down with regard to vivisection in its full entirety, would you? Supposing, for instance, that with regard to what you say about anæsthetics, it can be proved that without inflicting any pain on the animal, you could get some important result from the vivisection, you would then not object to it, would you?—I am afraid

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I should, because I do not see how we are to secure the certain administration of the anæsthetic.

6191. But I say supposing that it was proved to the satisfaction of this Commission, and as we believe to the satisfaction of the public, that it can be conducted without pain being inflicted, would you object to vivisection under certain restrictions?—Yes, because I should fear that even though it can be, it would not be conducted without pain.

6192. We have had the evidence of the first medical men, and we must base our report upon that evidence?—I should not trust vivisectors. I think I understand your Lordship's position, that it may be established that it can be done without the infliction of pain; but I should query whether it would be done.

6193. You stated just now that you thought there were a great number of the public, I think you said, that would not submit themselves to a medical man who practised vivisection, from want of confidence in him?—Yes.

6194. Now should you be surprised if I were to tell you that we have examined men of the greatest eminence, Sir William Gull, Sir James Paget, and the first medical men in London, and that they have been in the practice of vivisection, and yet retained more confidence than the great body of the medical men of England?—I feel that the result will be, when it is known, that a great many persons will not consult them again.

6195. Do you think that would be the case with the great body of the public?—I cannot say about the great body of the public; those with whom I have come in contact feel very strongly upon the subject.

6196. You stated that you objected to the introduction of vivisection into England. Did you make that observation under the belief that it is a recent introduction?—Its introduction into the medical schools. I understand that for the purposes of demonstration it is perfectly modern; altogether modern so far as I am informed. I am quite aware that it existed before that.

6197. Now supposing that it should be the opinion of this Commission that vivisection might be permitted under certain severe restrictions, what would be the restrictions (waiving your own individual opinions on the matter for the present) which you would wish to impose upon it?—I have great difficulty in answering that question.

6198. You think that no restriction would do?—I am not prepared to recommend any restriction at this moment.

6199. Can you suggest to yourself any pain inflicted upon animals for the purpose of human food, for the good of the community, and which has been inflicted from the earliest ages, which would come under the operation of your society? Can you suggest to yourself any operations performed upon animals from the earliest ages in this country, and of everyday occurrence, which come under your objection to treating animals with cruelty?—Yes, I can; I can think of several.

6200. Would you abolish all those?—I think that would require some consideration. With regard to the last question which your Lordship put to me, I should like to add that I am contending against the infliction of unnecessary pain, and I contend that this is unnecessary pain. I should not perhaps contend that pain inflicted in other ways for other purposes was unnecessary; but each case must stand upon its own merits.

6201. Then if it should be proved that the pain was necessary you would not object, is that so?—If I were satisfied that the pain was necessary. Your Lordship will pardon me for observing that I do not mean to say that what will satisfy another person would satisfy me.

6202. May I ask you, could you in the course of to-day or to-morrow furnish the Commission with a list of your subscribers and of the state of the subscriptions to the society since its origin?—I am afraid that I could not do that to-morrow, because I should have to communicate with Mr. Jesse, and I suppose it would take longer time than that.

6203. Do you intend in the re-organization of your society to insist upon more constant meetings and more constant consultation amongst all the members?—Yes, I intend to insist upon the appointment of an executive committee that shall take the active control, and that the names of those gentlemen who cannot attend regularly, or who do not wish to be on the executive committee, should be appended as ornamental vice-presidents or patrons or anything they choose. I have quite made up my mind that I shall retire from the committee altogether unless some such changes are made.

6204. (*Mr. Forster.*) Taking your late answer to Lord Winmarleigh, I think I understood that answer to mean that supposing you were convinced that vivisectional experiments under regulation and restriction were necessary in order to enable the science of medicine to arrive at that point that life could be better saved, you would then admit of it?—I take the ground that it is unjustifiable in any case, and I do not think that any amount of argument could convince me that it was right,—that it was allowable.

6205. I certainly gathered that to be your opinion from your earlier answers; but in the recent answer that was given, the qualification that you made was this, as I understood; you said you would admit of pain to animals in cases of necessity?—I did not mean to say that. What I wished to say,—what I was contending for was, that there should be no unnecessary pain inflicted upon animals. That was in reply to a question which Lord Winmarleigh had put to me with reference to certain operations in agriculture; and what I meant in amending my reply to that question was to let it be clearly understood by the Commission that whilst I was opposing in this instance the infliction of pain upon animals altogether, I did not think that the infliction of pain upon animals was a thing which in itself I should oppose if I were satisfied it was necessary.

6206. But the point on which I wish to obtain your opinion is this, whether you base your opposition to all vivisection upon the ground that the pain given is unjustifiable, or upon the ground that you consider it unnecessary?—I base it upon the ground that it is unjustifiable.

6207. Then why would you say that the pain given by vivisection is unjustifiable, independent of the question of necessity, and at the same time admit that pain given in other ways is justifiable upon the ground of necessity?—Simply because the one seems to me to be within the grant of the animals to man, and the other not. If I may put it in this form, I would say, for instance, that where the use of an animal for the purpose of serving man's necessities requires that the animal should be corrected in a manner which causes the infliction of pain upon it, I conceive that man is perfectly justified in training the animal and in making use of correction for the purpose of training it to serve him.

6208. And you do not think that this power that you suppose was given to man over the animals would apply to making use of them for the preservation of human life by the assistance of science?—Not in the form which it has taken, certainly.

6209. Not even supposing that the pain given to one animal, or say to a score of animals, might be the means of enabling a discovery to be made which might save thousands of human lives?—I am not aware that that is so.

6210. But you are aware that there are many eminent men of science who would state that that has been the case?—I am aware that that is the contention of certain persons.

6211. And I should rather gather from your previous answer, in which you stated that all vivisection was unjustifiable, that even supposing it was proved that the pain given to an animal might be the means of a discovery which might save thousands of human lives, you would consider that such pain was not included in the dominion that was granted to man over animals ?—I should.

6212. I suppose you do not consider that any pain inflicted for the purposes of experiment is included in the power given to man over animals ?—I think that animals may be taken and killed for food.

6213. But you do imagine that they might be killed for the purposes of sport in any way ?—If by that you mean for the amusement of man, I would say certainly not.

6214. You would think that all destruction of animals, in so far as it was done for amusement and not for the purpose of obtaining food, was unjustifiable ?—Well, I am not a sportsman and I could not be a sportsman on the very ground that I should not feel justified in amusing myself at the cost of animal suffering.

6215. Do you think that this opinion is held generally by the members of your society in regard to sport ?—I think they make a distinction.

6216. A distinction in what way ?—They think that the question of the pain inflicted by sportsmen is outside this question.

6217. But do you think that the general number of the supporters of your society, while they are so strongly opposed to vivisection, share with you your opinion about sport ?—That I could not say.

6218. You have no opinion upon that matter ?—No ;—I am not prepared to say that they do.

6219. I suppose you would consider that in so far as any destruction of an animal in sport was not the quickest destruction of the animal that could be made for the purposes of food, it would be unjustifiable ?—I see a difficulty in judging the conduct of other persons. I could not myself take pleasure in taking the life of an animal, but I can quite understand that where the life of an animal is to be taken by the use of firearms, for instance, there must be a certain amount of suffering inflicted which cannot be avoided.

6220. I will put before you two cases of pain inflicted upon an animal ; one the case of pain inflicted by a man of science, with the hope, and in his belief the reasonable hope, of a discovery which would greatly alleviate human suffering and save human life ; the other, the case of pain inflicted by a person for the purpose of amusement, the pain consisting in subjecting the animal to a longer and more painful death than it would otherwise undergo for the purpose of food. Would you think that the first was more unjustifiable than the second ?—I should think that as regards the characters of the individuals, the first was more justifiable than the second, that is to say, I should look with less abhorence,—I do not know whether to use that strong word, I will say with less aversion,—upon the character of the man who was acting with the desire to benefit his fellows than I should upon the character of the man who selfishly was amusing himself at the cost of animal suffering.

6221. But with regard to legislation, which of the two cases do you think it would be more incumbent upon the legislature to pass a law for ?—I do not see how we could pass a law with reference to sporting, because it is the taking of animal life for food. It is simply a question of the mode of taking it, and I do not quite know how you could deal with that.

6222. (*Mr. Hutton.*) Fox-hunting is not a case of taking animal life for food ?—Fox-hunting would scarcely come under that.

6223. (*Sir J. B. Karslake.*) When was the Society for the Abolition of Vivisection actually formed ?—I joined it in March of this year. I cannot tell you when it was actually formed, for I do not remember when the first advertisement appeared.

6224. Was it formed by advertisement,—in consequence of advertisement ?—I can only say what happened with reference to myself. I joined it in consequence of seeing an advertisement in the paper.

6225. Is there any office in town ?—I am not aware of any. That is one of the things that I have been seeking to establish.

6226. I mean who would be the person to communicate with if one wanted to join the society ?—Mr. Jesse.

6227. Are there any documents published by the society, or by an authorised body of the society, such as the Committee, stating what the objects of the society are ?—There is a sort of programme, a general prospectus of the objects of the society, which has been published.

6228. Since you have been a member has there been any meeting of the members of the society to discuss the question as to how far evidence, so far as it could be obtained from books, proves the necessity for vivisection ?—Of the members, no. I know of none.

6229. Then, as far as you know, each individual member of the society entertains his own opinions from different premisses and upon different grounds ?—I suppose they concur in the general objects, but may not agree in all details. On that I can say nothing.

6230. But I may understand that there has been no meeting, so that the general views of the society, and the authorities on which they are supported, should be made known to the members of the society generally ?—No ; there has been no general meeting of members.

6231. Nor even of the committee ?—I have not attended any.

6232. And so far as you know there has been none ?—So far as I know.

6233. Have you any paid officers at all for the purposes of the society ?—No.

6234. None whatever ?—None whatever. I do not know at this moment. Mr. Jesse may have a clerk who assists him ; that I cannot tell. It was discussed when two of the committee were in town during the summer ; but of that I do not know anything with certainty. There may be a clerk employed by Mr. Jesse.

6235. That is to say, merely for the purpose of correspondence ?—Yes.

6236. But you have no officers in town for the purpose of ascertaining the extent to which vivisection goes on, or the places at which it is carried on ?—No.

6237. You have said that there has been no meeting of the committee, so far as you know. Suppose a communication appears in the papers which it is necessary to answer, do the committee take upon themselves to direct what answer should be given ?—I think those communications which have appeared hitherto have been matters in which Mr. Jesse has been personally concerned ; and I think the letters written have been more personal letters than letters of the committee.

6238. As far as you know, no committee meeting has ever taken place for the purpose of determining the answer to be given to any correspondence that has appeared affecting the society ?—No.

6239. That would devolve upon Mr. Jesse to determine ? He would be entitled to answer it in any way he thought proper ?—Yes.

6240. And to use the name of the committee as his authority for so answering ?—Well, I should not approve of that.

6241. (*Lord Winmarleigh.*) I think you said that you could, if we gave you time, furnish us with a list of the members of the society, and its funds, the object we have being of course to ascertain what is the amount of public support that your society has got ?—I have no doubt I can. I have not got it myself, but I will write down for it. I will get it as quickly as possible.

6242. (*Sir J. B. Karslake.*) May I take it that you have given us the grounds upon which you have come to the conclusion that vivisection ought to be abolished altogether ?—I think I have given them shortly but pretty fully.

6243. As far as they have occurred to your mind up to the present moment ?—Yes.

6244. You say that you have not had the oppor-

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tunity of studying the pros and cons of the question as fully as you could have wished?—By no means. I meant to devote the vacation to the subject, but have been prevented by illness.

6245. I think you said that one of your objections to vivisection was that in your judgment it hardens the practitioner?—That is my fear.

6246. Do you yourself object to receiving advice from a practitioner who avows that he is friendly to vivisection?—I should object to put myself in his power.

6247. Would you carry it to this extent, that if he told you that in the disease from which you might be suffering it would be impossible to cure you without drawing largely upon the knowledge which he had obtained from works treating on vivisection, you would object to receive him then?— I think I should.

6248. If he told you that without the knowledge that he had acquired from those works it would be impossible for him to effect a cure, or hope to do so, you would reject his services?—I do say that I would reject his services because he had obtained his information in a manner I did not approve, if that is the meaning of the question.

6249. You would object to a man who had actually killed an animal for the purpose of research; would you also object to employ a medical man who avowed that the only knowledge he had as to the treating of the disease was derived from works which described operations upon animals which enabled him to judge how to treat your disease?—I feel a difficulty in answering the question. I do not know quite how it would come to my knowledge how he had acquired the information.

6250. I will assume that he told you so. As I understand many persons to your knowledge require, before they employ a medical man, that he should sign a statement that he does not approve of vivisection. Supposing you asked this gentleman whether he approved of vivisection, and being an honest man he said, "I do not approve of it or disapprove of it, but " I cannot treat your disease without drawing upon the " knowledge which I have obtained of the disease from " having read accounts of operations performed on " animals?"—I do not think that the manner in which his knowledge had been acquired—the source from which he gained his information—would affect my conduct in the matter.

6251. Then you would not think it objectionable for a man to study books treating on vivisection and the results obtained from vivisection, but you would object to a man who had actually vivisected an animal or performed an operation on a living animal?—I should think that the effect of studying such books was only a little removed, though it is removed, from the hardening effect of vivisection itself, and I would object to education either of the one kind or of the other.

6252. You would then think it preferable that human beings should suffer and die because the medical profession ought not to obtain that knowledge which they are supposed to obtain from books?—I would adhere to what I said just now, quoting it from the "Spectator" of 15 May last, that no alleged benefit can afford sufficient sanction for the practice.

6253. I am putting this question to you. Supposing that is wrong; supposing it can be proved to demonstration almost, or to the satisfaction of any reasonable men, that there are many most advantageous results which have been obtained from vivisection, and which could only be obtained by that means, would you then object to be treated by a man who was about to treat you by means of knowledge which he obtained from operations on animals?—I should not like it.

6254. If you are giving your views to the Commission upon the assumption that no good has been obtained from vivisection I will not trouble you further, but assuming this case that by operations performed on a dozen animals you could save the lives of many thousands of animals, would you object to those operations being performed?—I do not see that they are justifiable.

6255. Then your objection, as far as that is concerned, is, that man has no authority over animals to inflict pain of that sort, even for the purpose of saving, it may be, all the flocks and herds in the country?—That is putting it in an extreme form.

6256. It is a test you know?—I feel great doubt about it.

6257. I will put it in this form. Supposing that the sheep were being affected most fearfully with the small-pox, and that by the destruction of a few sheep you were able to stop the progress of the disease by the scientific results obtained by that means, would you then object to a few sheep being sacrificed, and that without pain, or with as little pain as could possibly be inflicted upon them under the circumstances?—I think each case would require to be dealt with on its own merits. As a general rule, I regard it as unjustifiable.

6258. Now let me ask you a little further, if you please, upon that. You will admit, of course, that for the purpose of curing animals or attempting to cure animals pain may be inflicted?—Surgical operations? Quite so.

6259. Now supposing a person were to believe that by an application of a new, and it may be a more painful blister than that which has formerly been applied, a better cure can be effected, would you object to that excess of pain being inflicted upon the animal?—No; not for the curing of disease.

6260. Supposing that the animal that was actually diseased was a valuable animal, whose life you wished to save, and that in lieu of using that animal you were to use an animal which was in the last stage of disease from other causes, and which was to be killed immediately after the effects of the operation were observed, would you think it unjustifiable to use the animal which I have described last for the purpose of ascertaining what the effect of blistering would be?—I think I should.

6261. (*Mr. Huxley.*) I happen to have been rather familiar with the organization of societies, and I beg to put to you two or three questions for the purpose of ascertaining whether within your knowledge the Abolition Society has been raised in the customary way. I understand that you have been a member of it nearly from its foundation?—From March.

6262. When was it founded?—That I am not able accurately to tell you.

6263. Do you know by whose authority the president or chairman was appointed?—No; I do not.

6264. Or the committee?—No. The committee I supposed had been formed of gentlemen who consented to act in that capacity.

6265. I think I understood you to say that so far as your knowledge goes there never has been anything answering to what you could call a general meeting of the society, either at its first formation or at any other time?—Not within my knowledge.

6266. Did you receive a communication on the subject of the formation of the society from Mr. Jesse?—In the first instance I saw an advertisement in the paper.

6267. The society is in possession of some considerable funds, I think?—I daresay.

6268. Has any treasurer been appointed?—I am not aware of any. Mr. Jesse, so far as I know, is treasurer and secretary.

6269. And so far as anything that appears to the contrary goes, he may have appointed the chairman and the committee?—Quite so.

6270. Can you tell me if any auditors are appointed, as is customary in a society that has to deal with funds?—I am not aware that that has been done. That is one of the things that I and other members of the committee are in communication about.

6271. Have you and the committee or other members of the committee, so far as you know, given

Mr. Jesse authority to speak in the name of the society on all occasions ?—No.

6272. To express opinions on behalf of the society? —No; I do not know of that authority having been given.

6273. You have never signed or passed any resolution to that effect; in fact there has not been any meeting I understand ?—I have not attended any.

6274. But you are a member of the committee ?—Yes.

6275. And would have been summoned to any meeting, I presume ?—Yes.

6276. We have had various letters sent by Mr. Jesse to this Commission, and in all those letters he speaks of himself as "the society's representative" (I quote those words), and he writes as if every letter were written by the authority of the society. May I ask if you have ever seen any of these letters? (if the Chairman will allow me I will hand them over to you to see), or if they have been written by your authority or that of any member of the committee ?—I do not suppose I have seen them. (*The letters were handed to the witness.*) No, I know nothing of these.

6277. Then they have not been written by your authority or so far as you know by that of any member of the committee ?—They have not so far as I know.

6278. And certainly the facts have not been submitted to any meeting of the committee ?—That is quite clear, there has been no meeting of the committee of which I know anything.

6279. We have also had laid before us various published letters from Mr. Jesse, letters forming part of correspondence with other persons, some of which are certainly worded in an exceedingly strong manner. In those letters Mr. Jesse speaks of himself as the representative of the society. Were those letters ever submitted to your committee ?—I am not aware that they were.

6280. So far as you can tell us, all this action of Mr. Jesse's has been entirely his own affair ?—Quite so. My belief is that Mr. Jesse got up the society probably with the concurrence of friends in his immediate neighbourhood; I do not know anything about that, but no doubt it is his creation to a great extent.

6281. And I suppose I may take it that practically he has appointed the president and the committee, and that practically everything that is done is his doing ?—I know nothing to the contrary. We have had informal meetings of two members of the committee who happened to be in town during the summer, but those are the only occasions on which there has been any sort of personal consultation between myself and Mr. Jesse.

6282. Had those meetings been called by Mr. Jesse ?—They were informal; I do not know how to answer that question; they were called with his concurrence.

6283. Was he present at them ?—He was at most of them.

6284. But those, not being regularly summoned meetings of the whole committee, of course could not be officially authoritative ?—No.

6285. Then further a number of printed documents have been issued in the name of the society, have you seen those ?—I think I have seen most of them.

6286. Did Mr. Jesse obtain any formal sanction from yourself or any other member of the committee, so far as you know, to the issuing of those documents ?—Not from me.

6287. You will observe that the point is of importance, because Mr. Jesse came before us professing to represent the opinions of a large body of highly respectable gentlemen. It is a matter of great moment therefore to us to ascertain whether he was really doing so, or was merely talking of his own motion ?—Well, as I stated to my Lord in the chair, I have been very much dissatisfied with the manner in which the society has been organized; my object in joining it has been to assist in the promotion of a definite object in connection with other persons whose views in the main I approve; I have been unable to organise it in the particular manner in which I wished to see it organised; and had I been well during the vacation the probability is that I should have either succeeded or retired.

6288. As respects the opinion which you quoted from a book, which you were kind enough to read to us respecting Harvey and his recourse to vivisection, I presume that you have not taken the trouble to go to Harvey's works yourself ?—No, I have not.

6289. And you are therefore not acquainted with a passage which has been before us from Harvey ?—I am not acquainted with Harvey's works; that is just one of the misfortunes arising from my having been laid up during the vacation.

6290. Will you allow me to read this passage to you: "When I first gave my mind to vivisections as a "means of discovering the motions and uses of the "heart, and sought to discover these from actual in-"spection, and not from the writings of others, I found "the task so truly arduous, so full of difficulty, that I "was almost tempted to think with Fracastorius that "the motion of the heart was only to be compre-"hended by God." And then a little further on,—"At length, and by using greater and daily diligence, "having frequent recourse to vivisections, employing "a variety of animals for the purpose, and collecting "numerous observations, I thought that I had at-"tained to the truth." Those are Harvey's own words, and I presume they will modify the opinion that you just now expressed with reference to him ?—My opinion is based upon the statement of Mr. Boyle's which I read.

6291. But these being Harvey's own words will probably carry more weight with you ?—I presume Harvey knew his own mind better than anyone else.

6292. Then as respects Bell, it is not worth while to trouble you upon that subject, but I presume in the same way you have not attended to the evidence on that subject ?—No, I have not had the opportunity of working the thing up as I wished to have done.

6293. I think I understood you to object to vivisection for the purposes of demonstration in medical schools, on the ground that it would accustom medical men to experimentation, and therefore would rather lead them in your judgment to experiment upon their patients, as interesting chiefly to scientific inquiry, rather than attempt to cure them ?—Yes, I should not wish to put it that it would lead them as a general rule to do that, but rather that one could never be sure that they were not doing it. That is what I meant to convey, that it would create distrust, because people could not be sure that the mind of the medical man was not rather directed to some interesting feature in the case which would lead to a scientific discovery than to the immediate welfare of the patient.

6294. Then I apprehend that that objection applies not so much to vivisection itself, as to the experimental method of which that is an exemplification; that a medical man having been used to the experimental method, would attempt to apply it to his patients ?—Quite so.

6295. Therefore in that case your objection would apply equally to all other modes of scientific inquiry which exemplify the experimental method; for example, there would be a parallel objection to the experimental study of chemistry, because that is purely experimental and brings to men's minds more than anything the necessity of experiments, hence there would be an objection in your mind perhaps to the experimental teaching of chemistry ?—No, I do not see that. I think that is one of the evils attendant upon experimentation of every kind that has to be guarded against. I do not say that that of itself alone would be sufficient to cause me to oppose vivisection. I give that as one of a number of reasons.

6296. Still, if a man is taught in any method whatever that experimentation in physical matters is the only way of ascertaining truth, surely the evil consequences of that belief, if there be any, must come out of any such teaching; it does not matter what the

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subject-matter of the experimental teaching is, so long as a man gets it into his head that experiment is the thing to learn by ?—No doubt it trains him in a particular manner, which may predispose him to adopt objectionable practices.

6297. So that on the whole I take it that you think that even the experimental teaching of chemistry is not without its dangers; that it rather leads to make a man a worse practitioner than a better, or a more suspectable practitioner, I will say ?—You may put it in that form as to his being "suspectable;" but I do not say that that is sufficient to justify a distrust of a medical man.

6298. But I thought your objection was to his applying the experimental method to his patients. Now a man may become just as familiar with the experimental method by the study of chemistry or physics as by the study of experimental physiology ?—But there is no hardening effect in the chemical experiments; and it is the two things taken together which constitute the objection. My objection applies to experiments on living animals, not to other forms of experiment; though I feel that a medical man whose education has led him to form habits of experiment ought in practice to keep those habits under strict control.

6299. Now about the hardening effect upon men; is it within your experience that medical men show any less tenderness for the dead bodies of their fellows than other persons do; was there ever a case known of a medical man going with carelessness or disrespect towards a corpse ?—I am scarcely competent to answer that question.

6300. But if during these 300 or 400 years medical men, having been incessantly studying the anatomy of the human body, have not been led to be hardened on that subject, there is some ground for suspecting whether they would be hardened by witnessing experimental physiological operations, is there not ?—I have no personal knowledge of it, but one generally understands that medical students do not always exhibit the very greatest respect towards dead bodies.

6301. Then I understand you to express the opinion that it was generally admitted by medical men that vivisection is not necessary for teaching purposes. Have you made many inquiries among physiologists and medical men on that point ?—No, it is in my general reading upon the subject that I have gathered that there was a considerable amount of consent that there might be restrictions placed upon that portion of the practice.

6302. I put the question because the tendency of the evidence given to us has been the exact contrary. Now you further spoke of a "monopoly" at present enjoyed by medical men; would you be so kind as to explain what that means ?—I mean in the granting of certificates, and that sort of thing.

6303. In the granting of certificates of death for example ?—Yes, and vaccination, and certificates that are required if a man claims exemption in a court of law from serving on a jury or anything of that kind.

6304. You are a member of the House of Commons I think ?—I am.

6305. As such of course you take part in legislation ?—Yes.

6306. And I presume that your experience has convinced you that one of the essential conditions of a good law is that it should be respected by the persons whom it affects ?—Quite so.

6307. That if it is to be respected by the persons whom it affects, its operation should be equal, not partial ?—Yes.

6308. Now supposing that there were any abuse extensively practised, do you think that the legislation which fastened upon one particular fraction of that abuse and stopped it or interfered with it, and let all the other portion remain, would be likely to secure the respect of the community ?—I think it must depend very much upon the circumstances of each case.

6309. Supposing there were a liquor law passed to forbid certain persons from drinking champagne and to allow everybody else to drink as much as he liked, that is not a law which would commend itself to you ?—Not at all.

6310. Now that has a certain application to what we are considering at present. It will be within your knowledge, and the fact has already been brought out in Mr. Forster's questions, that an enormous amount of pain is inflicted upon animals for very various purposes. So far as those purposes relate to sport I understand your personal opinion to be that you do not think the infliction of this amount of pain justifiable ?—Quite so.

6311. I should like to ask you a question or two with respect to many well-known domestic operations. You are familiar with the castration of sheep and the spaying of sows, which is undertaken, not for the benefit of the animal, but simply to make it better for culinary purposes. Now, do you think that object one which justifies the amount of pain inflicted ?—I think it depends in each case upon the particular necessity which exists for it.

6312. That is why I ventured to put a particular concrete case to you. Take that of the castration of the lamb, or take that of the spaying of sows. The object there, there is no question about it, is simply to make them more easily fattened and rather better for the table. With that object many many thousands of animals are mutilated every year; and I wish to know whether you think that the object there justifies the operation and infliction of that amount of pain ?—My experience has not brought me into such contact with either of those operations as to be able to say what necessity there exists in the case. I presume that there are reasons for the castration of lambs, for instance, which may be sufficient. I am not prepared to express an opinion to justify it.

6313. In the case of the spaying of sows, there is no doubt whatever about it, that it simply enables the farmer to fatten the sow a little faster; nobody ever doubts that that is the reason. Would you think that sufficient to justify the amount of pain inflicted, for it is a very severe operation ?—No, I should not.

6314. Take another case; many many thousands of rabbits are brought into Billingsgate market every day; the great majority of those rabbits have been caught in traps, the operation of the trap is such that the rabbit is caught and very often held in intense torture for five or six hours till the keeper comes and takes it out and knocks it on the head. The human race could very well do without rabbits. Do you think that the object attained in that way is sufficient to justify the amount of torture inflicted ?—I think it is a very cruel method of catching them.

6315. I think I might, only it would occupy the time of the Commission too much, put before you a number of similar cases. Now, while this enormous amount of unnecessary pain, or what may be assumed to be unnecessary and severe pain, for very inadequate objects indeed, is being inflicted, is it your opinion that it is a practically fair and equitable legislation to meddle with the comparatively small amount of pain inflicted for scientific purposes, and leave all the rest untouched ?—I should be glad to see a larger measure than one merely addressed to scientific purposes.

6316. You would like to make this measure equal in its operation with regard to all similar classes of what you consider offences ?—It would require very careful consideration how it should be dealt with, but I should like to see not a measure to deal with vivisection alone, but a measure to deal with the cutting and wounding of animals generally.

6317. In fact, you are prepared to apply your principle logically and consistently to all cases which could be shown to come under it ?—I hope so.

6318. Now let me put one other inquiry to you. There seemed to be some doubt in your mind whether vivisection had ever furnished a practical medical man with knowledge valuable to mankind. I do not know exactly whether you may have heard of Dr. Brown-Séquard ?—I know the name.

6319. Who was an exceedingly eminent vivisector, Mr. J. M. Holt, M.P.

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and who during one period of his life spent some four or five years in London, and became a practising physician. He treated specially the diseases of the nervous system, and his knowledge acquired by vivisection was of such a character, in its application to the relief of disease, that he very rapidly had an enormous practice, and no doubt did more than any other man of his day for the relief of human suffering. May I ask whether, supposing you yourself had had the misfortune to suffer from epilepsy or paralysis, you would have absolutely refused to take advantage of Dr. Brown-Séquard's knowledge; or rather, I will not ask you whether you would have refused to do so (because we do not always ourselves act up to our own principles), but whether you would have thought it wrong to do so?—I should have done it very reluctantly.

6320. May I ask if you had had a relative suffering under such a form of disease, and had good ground for believing that Dr. Brown-Séquard would have cured that relative, would you have thought it a wrong thing to take Dr. Brown-Séquard to that relative?—I do not think I should have done it.

6321. (*Mr. Hutton.*) I take it that you would not object to allow the police to make use of the knowledge that they had acquired from burglars in order to prevent burglary, would you?—Well no, I do not know that I should.

6322. Therefore it does not follow at all that because you object to vivisection you should object to use the knowledge acquired from vivisection in the cure of disease?—I cannot see it myself.

6323. I take it that what you meant with regard to the danger you apprehended to the profession from the practice of vivisection was not lest the method of experimentation should lead to abuses, but lest the method of experimentation on suffering should lead to abuses?—Quite so, experimentation upon suffering.

6324. Your idea was, for instance, that if a medical man would not object, according to evidence which we have had before us, to put a dog to very considerable pain for eight or nine hours for the sake of finding the effect of rhubarb upon its liver, he might conceivably not object to put a being differing not very greatly from a dog to a certain amount of not much less pain for the sake of finding the effect of rhubarb on his liver?—Yes.

The witness withdrew.

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Mr. George Henry Lewes called in and examined.

6325. (*Chairman.*) You have devoted yourself a good deal to the study of physiology and the practice of physiological experiments?—Yes, especially during the last 20 years.

6326. You received originally, I think, a medical education?—Yes.

6327. And therefore acquired sufficient familiarity with the subject for that purpose?—Yes.

6328. Have you been a great deal in foreign laboratories?—Principally. I know little of the English laboratories.

6329. But you have formed opinions upon the subject which has been referred to our consideration?—Yes.

6330. Will you have the goodness to state them to us?—Briefly, my view is that vivisection as a portion of experimental science is indispensible, is a necessity, but that it is a very painful necessity; and that being a very painful necessity, each experimenter should consider the question of pain in determining whether he will perform an experiment or not. A great deal of experiment is quite useless; useless because it very often could not prove what is attempted to be proved. The organism is so complex that when you produce even a slight disturbance you are seldom certain of what other disturbances you produce; so that an experiment which seems quite decisive by the phenomena it presents, will turn out to be totally indecisive, because the same results may be obtained by a totally different experiment. I will cite a brief instance. Owsjannikow published an account of the effect of destroying certain ganglia of a crayfish; he did nothing else but destroy these ganglia; and it seemed as if the phenomena were the consequence of that. Meyer immediately afterwards cut off the large nippers of the crayfish, and the same phenomena were observed; he had not touched the ganglia, and yet he got the same result. In the case of the innervation of the heart I scarcely know a single statement which has been made that has not been contradicted by somebody else; and so on. In consequence of this extreme complexity of the organism, and the difficulty of determining the effect of any disturbance, the experimenter requires a very powerful imagination to picture to himself what will probably be the result of the experiment that he is going to perform. When it is a painful experiment, when there is a great mutilation necessary, I think the experimenter is bound by moral considerations to weigh very well and carefully with himself what are the probabilities that this experiment which he is going to perform will have the result that he anticipates, or will have any result at all that is distinguishable from some other disturbance. But I think it is so utterly impossible to lay down laws or general rules in this matter that even physiologists could not always determine when an experiment should be performed, unless they had the whole of the case before them. Each man really must determine it for himself. But a considerable amount of experiment might be got rid of if the students were early impressed, firstly, with the belief of the excessive difficulty of getting at any result: a belief which would prevent any experiment being lightly undertaken; and secondly, to consider whether any experiment that involved great mutilation should be performed at all, unless under a profound sense that it was necessary, and likely to yield a valuable result. At present our journals are crowded with reports of experiments. One man discovers a fact or publishes an experiment, and instantly, all over Europe, certain people set to work to repeat it; they will repeat it, and repeat it, and repeat it. Now it is quite unnecessary to repeat an experiment as to a fact that has been well observed. Of course it will require confirmation, but it does not require to be repeated often; as soon as it has been once established, I think you should have some reasonable doubts of the validity of the fact before you repeat it.

6331. In point of fact it requires a person of very great powers of mind really to form a just conception of whether an experiment will be of service or not?—Decidedly; and even then he can do it only with a very obscure sense of the probable results.

6332. I need not ask you whether a great multitude of persons must necessarily be entirely incompetent for any such office?—Entirely.

6333. And whether, therefore, it is not important that, if possible, the discharge of such a function should be limited to the class of people you have spoken of as the only class competent to perform it properly?—Certainly.

6334. And if, therefore, society should endeavour to take some steps for limiting the discharge of those functions to such persons, society would be acting in its right and in its duty, would it not?—Yes; but there is this difficulty, that a man who may not be a recognised physiologist may yet, in fact, be a physiologist. For instance, I should not have been recognised; I am a member of no medical college; I am not a member of the profession; and yet there are very few members of the profession who have done more physiological work than I have. I should be excluded, upon the supposition that only physiologists who were recognised as such were allowed to perform these

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experiments. When I began my work I should have been excluded and not allowed to perform experiments. That is one difficulty. I say, therefore, that it seems to me it is a question to be determined by each person and in each case. It seems to me that the vivisection of which we are now speaking is very much like vivisection in another department, that of Literature, that is to say, criticism, which is also vivisection. There is a great deal of real torture inflicted upon authors by critics, which lasts for a considerable time in sensitive minds.

6335. (*Sir J. B. Karslake.*) And without anæsthetics ?—And without anæsthetics.

6336. (*Mr. Erichsen.*) And by incompetent persons ?—Not only by incompetent persons, but by persons who, even when they are competent, are often reckless. It is quite true that for the benefit of literature, and consequently of society, criticism is a necessity ; and I suppose that everybody possessed of right feeling, who has exercised that office, has often felt great pain in giving pain. But a great many people do not feel any pain at all about it ; a slashing review is a thing that they like.

6337. (*Chairman.*) Is there not this difference, that you may get so much accustomed to the moral vivisection, of which you speak, as to become indifferent to it ?—Public men may get indifferent to criticisms which they get every day ; but I do not think that actors and authors, who do not get it every day, get accustomed to it.

6338. But I suppose you would scarcely compare that in point of necessity of control with the fact of living animals being cut up ?—Why not ? Surely here are human beings who suffer frightfully ? I do not think you could control that ; but then I do not think you could control vivisection profitably.

6339. Do you not think it would be possible to contrive some arrangement by which utterly ignorant and incompetent people should be prevented from exercising their fancy in cutting up living animals ?—If I thought that ignorant people did it, I should certainly say that they ought to be stopped, if possible ; but as far as my observation goes, even medical students are extremely reluctant to perform experiments at all. They do not like to follow anything which gives them much trouble ; and the outside world certainly do not, except from mere cruelty and love of tormenting. Experiment is very troublesome and tiresome ; and the students have not generally sufficient intellectual interest in the result to take the trouble involved.

6340. (*Mr. Erichsen.*) And their time is much occupied in other matters ?—Yes. When I was in Munich a great many years ago I assisted in preparing the heart of a frog for Professor Bischoff's lecture ; there were six preparations made, and the students had only to look through the microscopes and they would have seen the ganglia of the heart which perhaps they never would have seen otherwise ; and yet, although these six microscopes were there for these students to look through, if they chose to take the trouble, when the lecture was over there was not a single one that did.

6341. (*Chairman.*) Is there any pain inflicted upon these frogs so put under the microscope ?—No ; it was only the dead heart. So little interest do students habitually show in anything like scientific inquiry which is not to come into their examination, that I do not fear at all that there will be much experimenting done by students who are ignorant. But as to the students who are curious and of a really scientific bent, that is another matter ; they may be, and I think ought to be encouraged to perform experiments.

6342. Would you encourage an unpractised student to perform painful experiments ?—No, certainly not ; no experiments at all. He should be made to feel the excessive difficulty of reaching any valid result, until a large knowledge guided him.

6343. If I collect your opinion rightly, you see difficulty in providing any control or restriction, but if control or restriction could be provided, which should operate upon the unpractised and the incompetent, it would not go contrary to your views ?—I should look with great jealousy upon anything like a definite restriction. In each individual case the professor should discourage, I think, the needless performance of experiments.

6344. But I understand you to say that you may read every day in published medical journals of experiments all over Europe which are unnecessary repetitions, which are abuses of the power of man over the lower animals, and which therefore I presume you would be glad to see restrained if you could ?—Decidedly.

6345. And therefore it is only the difficulty of accomplishing the object which is in your mind the objection ?—Yes. My difficulty is that anything like a definite restriction would very seriously interfere with the prosecution of the science. If it did not, then I could only wish it to be done. I think for the sake of science it ought to be restricted ; but I think it must come from the professors themselves inculcating a sense partly of responsibility to the animals, and partly of responsibility to science, not to encumber science with useless lumber, which the mass of it is. And I think that the repetition of experiments for confirmation of established facts is very unjustifiable. It is difficult of course to say what is established and what is not ; but unless you have a doubt, and a rational doubt upon any one point, you ought not to put an animal to any inconvenience even—I will not say *pain*, because there is really very little pain going on (that is to be observed), for the pain of an operation is in the *wounding*, not in the *wound*. The wounding is almost always done under chloroform or ether, for various excellent reasons ; unless you want to test the pain, then of course you do not use anæsthetics. When the animal recovers he has got a wound ; he may sometimes be dreadfully mutilated ; but habitually he has simply a wound which is of very little consequence. It is not as with us ; unless inflammation sets in afterwards, the wound is of very little consequence. I have seen a rabbit wake out of the narcotic state with its spinal cord cut in two, and begin to eat a cabbage as if nothing whatever had happened ; so that there really is very little pain. I should not care to put that forward however, and make people say that vivisection is not painful, because there is a good deal of it which is as bad as pain. You disable an animal, and it is a cruel thing to an animal to disable it ; but there is very little pain, as absolute pain, when chloroform is used.

6346. (*Lord Winmarleigh.*) At the same time I suppose you would not deny that if an animal has been experimented upon and is allowed to live afterwards, the wound that has been inflicted may be the cause of great pain to it ?—It would depend of course upon the nature of the wound ; but the wound heals very rapidly. I saw a bitch in Goltz's laboratory that had its spinal cord cut through when a puppy ; and it was a lively bitch, and bore pups afterwards ; and yet that cutting of the spinal cord is a terrific operation.

6347. Did I apprehend you rightly to state that you thought that no experiment should be made without a definite object ?—Certainly. I think morally every man is bound not only to have a definite object in performing an experiment, but also only to perform it after due consideration of whether the experiment could have the result, or any available result.

6348. You would object altogether to a man making an experiment to see what would come of it ?—Altogether ; it is preposterous. Not only that, but I should object to his doing it without due consideration of the anatomical relations, or without the necessary physiological knowledge.

6349. I daresay you are aware that the object of this Commission is to recommend a course to be adopted on this subject. Could you yourself suggest what you think would be the best mode of preventing any unnecessary cruelty in this respect ?—No. As I have said, no more than I could suggest how you are to

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prevent the unnecessary cruelty in criticisms; yet there is a great deal of it.

6350. You must leave it to the good feeling of people, you think ?—Yes; unfortunately a slashing review is very popular, and therefore the critic is encouraged in doing what he would not if the public disapproved of it. Now if the scientific public strongly disapproved of useless experimentation, that would be a check.

6351. You say that you have been abroad a good deal ?—Yes.

6352. Is there a country that you know of in which there is any legislation upon this subject ?—No.

6353. (*Mr. Forster.*) You have studied physiology very much, although you state that you would not be considered a professed physiologist ?—Abroad I am recognized, but I should not be recognized in England as a physiologist by any constituted body, because I have no diploma.

6354. In your study, and for the objects of discovery which you have had, you have yourself tried a good many experiments ?—A great many.

6355. Do you know of any other investigators like yourself who are not connected with any of the physiological schools, who are not professed physiologists, who try experiments ?—No; I know very few, even among physiologists, who do.

6356. When you speak of this constant repetition of an experiment, I suppose you do not mean by that that there are a great many people like yourself who would be trying experiments, but without the same care to avoid useless repetition ?—No, I think there are few people who perform experiments at all in England; I wish there were many more.

6357. I was going to ask you a question with regard to the mode in which you have conducted your experiments, and I will explain the reason why I do it. You are the first what I may call private investigator that we have seen. We have asked the public investigators who have come here questions as regards the manner in which they have conducted their experiments, and therefore if I ask you the same question you must understand that it is not from any suspicion of carelessness on your part, but merely that we may obtain information on that part of the subject. Now about how many animals in the course of the year would you be likely to experiment on ?—That I could not answer, because my experiments have been mostly upon frogs and other cold-blooded animals. They have been almost exclusively upon those; and they have been done in great quantities when they have been done at all. Sometimes it would be several frogs to a single experiment, sometimes a single frog would suffice.

6358. Would that be a painful experiment or not ?—It might or might not. If you mean an experiment in which the animal would feel pain, that would be very rare indeed; it is only when you want to determine the effect of pain, *as* pain, in checking the action of the heart, for instance, that you do not etherize; but I should say that in 70 cases out of 80 the frogs are etherized.

6359. That is the anæsthetic which you employ for the frog ?—Yes, because chloroform almost always kills it.

6360. Have you tried any experiments upon dogs or cats ?—No, I could not bear it.

6361. Rabbits ?—Yes.

6362. Can you give us the number of your experiments on rabbits, or any warm-blooded animal ?—Very few rabbits, and pigeons.

6363. A dozen of rabbits or pigeons in the course of the year ?—No, not more than that in as many years.

6364. When you do try experiments on warm blooded animals, you give anæsthetics ?—Yes.

6365. I suppose in every case except where the experiment relates to the nerves ?—Almost all my experiments were relating to the nervous system.

6366. When you give anæsthetics what do you give ?—Ether for the frog, and chloroform or ether for the warm-blooded animals, and sometimes chloral.

6367. When the animal recovers from the chloroform or other anæsthetic do you, in case of its being likely to suffer much pain, kill the animal at once, as long as it can be done without sacrificing the experiment ?—Always.

6368. Have you ever used wourali or curari ?—No.

6369. Have you formed any opinion as to whether it really is an anæsthetic or not ?—It is probably an absolute anæsthetic.

6370. What we have had brought before us is that there was originally very great doubt whether it was an anæsthetic in the sense of depriving the animal of sensation, though it did deprive it of mucular motion, and that now there is some little difference of opinion on the question ?—I should add that I disbelieve entirely in sensation—I mean *conscious* sensation—without motion.

6371. Then it is your opinion that inasmuch as curari destroys the animal's power of motion it also destroys sensation ?—It would take too long to explain my meaning, but I will merely say that no sensation so complex as that of pain could be produced in an animal that had been curarized.

6372. I do not want to ask you the name of any operator abroad, but would you consider that there is a different feeling with regard to the prevention of pain in these experiments on the continent from what there is in England ?—Very great; and a difference between France and Germany. The French and the Italians are very much more indifferent to the question of pain than the Germans are; strikingly so. And it is the same in Italy, partly I believe owing to the indifference of the whole nation to the sufferings of animals. In Italy and Spain the people are more cruel to their animals than they are in either France or Germany.

6373. Without comparing France and Germany, you would say that in England the feeling is decidedly more in favour of the animals ?—Decidedly stronger.

6374. But I think you said that you wished there were more experiments, and I suppose you would think that the progress of experimental science altogether would have a tendency to cause more experiments to be made ?—Yes, what I wish is that there should be more thoughtful experimenters, and fewer needless experiments.

6375. But do you not think that without some kind of legal provision there would be some danger of England following the example of France and Italy ?—Not the least.

6376. Why do you think so ?—Because it is entirely a social difference. I should say the state of feeling in Italy and Spain with regard to animals (I begin with those), from the lowest strata of society upwards, is different from what it is in England. I do not wish to assume for ourselves a moral superiority, but our morality has certainly been cultivated in the direction of greater sympathy with animals; and of course the surgeons and medical men are taken out of the general mass of the population, and in those countries of which I have spoken they bring with them what I should call the national indifference. In England we are not indifferent to those things; at least not so much; there is a great deal of indifference in England, but still it is less. Then I may say, without naming them, that two of the very greatest experimenters in Europe whom I have seen at work (not in England,) always use anæsthetics; and one of them many years ago explained to me why it was that he did so: not in the least out of consideration for the animal, but entirely out of consideration for the perfection of the experiment: that he might know what he was doing. If you have an anæsthetized animal it lies like a log of wood before you; it does not move, it does not flinch, and you can make the most delicate operation with certainty; you know how far you cut.

6377. For that purpose curari would answer for him ?—Perfectly.

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6378. And do you think he generally used curari?—No, chloroform.

6379. (*Mr. Hutton.*) You said that you had performed many hundreds of experiments, and some of them extremely painful, but in very rare cases. Would you give us instances of the very rare cases in which they have been extremely distressing to yourself?—Well, it is always distressing to hear the cries of an animal or witness its struggles; to me especially, being very fond of animals, and so much so that I could not see experiments upon dogs.

6380. But would you give us an instance or two of the most distressing experiments that you have performed. Have you ever performed Goltz's experiment of boiling a frog till it died?—No; but to disprove his conclusion, I dipped a frog in boiling water.

6381. A frog without its brain having been destroyed?—No, the brain had been removed.

6382. Do you suppose that the frogs suffer very much under that?—No, I do not. Personally, I do not believe that even when they have the brain frogs suffer pain at all; but I should not like to state that publicly, because it would seem like an attempt to justify our practice on equivocal grounds. I should justify it even if the pain were there.

6383. What grounds could you give us for that supposition that frogs do not suffer?—Because we know that the great mass of our own sensations are totally divested of pain and cannot be exaggerated into pain; even with hyper-æsthesia there is often no pain. We know that among human beings, especially when you descend to the savages, the sensibility to pain becomes less and less. Coming to animals, we know that, for instance, a horse that has had his leg shattered in battle will crop the grass, which no animal in great pain could do. There has been an immense mass of evidence collected, which I cannot at this moment recall, showing that pain is quite a special form of nervous sensibility. We know that anæsthetics destroy pain without destroying sensibility; a patient will feel the surgeon's knife, but no pain; and in certain diseases an increased sensibility to touch and temperature co-exists with absolute insensibility to pain. On any evidence that I have before me I should say that the fishes and the reptiles have sensibility, but none of that which we call pain.

6384. But your answer applies to horses quite as much as to the lower animals?—I say they do not suffer the pain that we do; yet a horse's skin is excessively sensitive, as we know.

6385. Then you do not agree with the opinion that has been expressed before us, that there is more real difference between one individual and another individual in the same species in point of pain than there is between individuals of different species?—No.

6386. Can you give me any account of an experiment on one of the warm-blooded animals that you thought extremely painful?—Well, dividing the spinal cord and cutting the roots of a spinal nerve.

6387. Is that the one on recurrent sensibility?—No; simply the dividing the roots of the nerve.

6388. Then, I suppose, you performed that under chloroform, did you not?—Yes.

6389. But the irritation afterwards was without an anæsthetic?—Yes; it was not feasible to give an anæsthetic afterwards.

6390. Was that on a rabbit?—On a rabbit.

6391. Did that experiment answer its object? Did you discover something?—No.

6392. It was an ineffectual one?—Quite.

6393. Do you not hold some rather peculiar views about the phenomena of reflex action?—Yes.

6394. You differ from the physiologists in general, do not you, as to the action of the spinal cord?—Yes; I believe that sensibility is present in all reflex action.

6395. You mean by sensibility, not suffering, I understand?—No; sensibility.

6396. (*Lord Winmarleigh.*) What is the difference?—For instance, you have in the eye sensibility to light, but you have no pain in that at all. Your optic nerve might be cut through and you would have a vivid sensation of light, but none of pain.

6397. (*Mr. Hutton.*) You mean a kind of consciousness, do you not?—It may be a kind of consciousness: that is a question of definition.

6398. (*Lord Winmarleigh.*) Then it is the extent of the sensibility that is pain, is it not?—No; you may have exaggerated sensibility without any pain.

6399. Take the instance of a worm. You put a hook into a worm and there are violent contortions; that we are told is reflex action?—There may be or may not be pain, but the contortions do not prove pain at all; no more than the struggles of a patient under chloroform.

6400. But there would be sensibility there?—Yes.

6401. (*Mr. Hutton.*) May I put this question to you to raise the point. Do you hold, for instance, that supposing a complete frog would suffer by being put into boiling water, then a frog with its brain destroyed would suffer equally by being put into boiling water, or would suffer in a proportionate degree?—No; it would not follow. When you have removed the brain you have removed a great part of the nervous organism.

6402. Still, would you hold that some suffering would remain in the brainless frog?—No; no *suffering* at all, but sensation.

6403. Then the same kind of power of sensibility is not in the spinal cord as in the brain itself. It is not a mere question of degree, I mean?—No. You would say that the sensibility of the eye or the optic apparatus to light was different from the sensibility of the auditory, and that again different from that of the gustatory, but all three are forms of sensibility, and the form known as pain is quite as special.

6404. Do you suppose that the lower part of a man whose spinal cord is divided suffers, although he is not conscious of the suffering, when any painful operation is performed on his lower limbs?—How do you use the word "suffering," may I ask?

6405. That there is an animal, though not that animal which is conscious, through the brain, that is suffering; that there is a part of the animal that is suffering?—Certainly; I believe that there is sensibility, or as we say that the leg feels it. I believe that a fragment of the spinal cord has the same sensibility that the whole spinal cord has, and the whole spinal cord has the same as the whole spinal axis, including the brain; but the forms this sensibility assumes depend on the organs innervated.

6406. Supposing a case were brought before you as it has been brought before us, of a man under the action of curari, who, although he could not stir a muscle and had to have respiration kept up by artificial processes, gave a full account of all that happened to him during the action of curari; would you not argue from that, that it was quite possible that sensibility would remain after the motor power was absolutely destroyed?—But what motor power? Were all the muscles destroyed, or simply the ability to move his limbs?

6407. All the motor power?—You must remember that there may be motor vibrations going on without a limb being moved.

6408. As I understand even the power of breathing is entirely destroyed in that case without artificial respiration?—I do not know such a case.

6409. And you do not believe in it?—I cannot say that I do not believe in it. I do not know it.

6410. I understood you to say that you did not believe that motor action could be destroyed without sensibility being destroyed?—I should have limited that; I should not have said motor action, but rather motor impulse; because if the muscles do not contract synchronously there will be no movement of the limbs, and yet every muscle and fibre may be in a state of vibration itself; but they do not contract synchronously.

6411. (*Chairman*). Before you go let me ask you one question,—do you consider that in the training of

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medical students in physiology it is necessary that they should see experiments performed before their eyes?—Well that again would depend upon the nature of the experiment. Many experiments may be rendered so intelligible that a diagram would do; in such cases experiment is *very* objectionable; but in more complex cases they must be seen. It impresses the facts upon the student to see the experiment.

6412. You consider that to state that medical students could be completely trained without having seen experiments on living animals would be a mistake?—Yes, they must see some.

6413. (*Mr. Erichsen.*) You stated, I think, that in performing experiments on warm-blooded animals your feelings prevented you from using dogs and cats, and you invariably had recourse to a rabbit?—Yes, or a pigeon.

6414. Do you think that a rabbit may be substituted for a dog or cat, as a general rule, in all experiments on warm-blooded animals?—I should not like to say that.

6415. Do you think that there are cases in which it could not be used with advantage?—It would entirely depend upon what the man was going to prove. I could not myself use dogs or cats.

6416. I simply meant to ask whether in your opinion a rabbit, as a general rule, could be substituted for other warm-blooded animals, say a dog or a cat?—As a general rule it is, because it is much more tenacious of life and much cheaper.

6417. But there are cases you think in which it could not be substituted?—I should not like to express any definite opinion on that subject.

The witness withdrew.

Adjourned.

Monday, 20th December 1875.

PRESENT:

THE RIGHT HON. VISCOUNT CARDWELL, IN THE CHAIR.

The Right Hon. W. E. FORSTER, M.P.
Sir J. B. KARSLAKE, M.P.
THOMAS HENRY HUXLEY, Esq.
JOHN ERIC ERICHSEN, Esq.
RICHARD HOLT HUTTON, Esq.
N. BAKER, Esq., Secretary.

Mr. GEORGE RICHARD JESSE recalled and further examined.

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6418. (*Chairman.*) When your last examination closed, we understood that you wished to say something more?—Yes.

6419. Will you be so good as to tell us what it is?—The Society for the Abolition of Vivisection wishes to observe, that evidence, such as it has been giving to Her Majesty's Commissioners, of the torture of animals for so-termed scientific objects, the corrupting moral influence generated by these practices, and the errors and fallacies spread abroad by them, it can continue to give, if Her Majesty's Royal Commission is not satisfied that, from the mouths of vivisectors, the Society has proved them to be all that it has asserted of them in the opening statement which it made before this Commission. The Society wishes respectfully to inquire of Her Majesty's Commissioners if they are convinced on these points, and if not, what further evidence they require?

6420. The Commission have given you the opportunity which we understood you to ask for, to give further evidence before us, and we are now assembled for the purpose of hearing you?—Very good. Then we wish to tender in further evidence the opinion of the late Sir Charles Bell, the eminent surgeon, who has said, "Anatomy is already looked on with pre-" judice; let not its professors unnecessarily incur the " censures of the humane. Experiments (vivisec-" tions) have never been the means of discovery; " and the survey of what has been attempted of late " years will prove that the opening of living animals " has done more to perpetuate error than to enforce " the just views taken from anatomy and the natural " motions." Again, Sir Charles observes, "In a " foreign review of my former papers the results have " been considered in favour of experiments (on living " animals). They are, on the contrary, deductions " from anatomy; and I have had recourse to experi-" ments, not to form my opinions, but to impress " them on others. It must be my apology, that my " utmost powers of persuasion were lost whilst I " urged my statements on the ground of observation " alone."

I will now quote from "The Life and Labours of " Sir Charles Bell, by Amédée Pichot, M.D. " London: Richard Bentley, New Burlington Street, " Publisher in Ordinary to Her Majesty, 1860." At page 68, speaking of one of his ideas, he says, "He " thought more of it for his own gratification than for " the benefit of others, and at last brought himself to " a conviction that the pursuit of his discovery was " the egotistical gratification of a scientific vanity. " In this point of view he looked upon it as an act of " barbarism to sacrifice living animals to his fruitless " experiments. 'I should be writing,' he said to his " brother, 'but I cannot proceed without making " 'some experiments, which are so unpleasant to " 'make, that I defer them. You will think me " 'silly, but I cannot perfectly convince myself that " 'I am authorised in nature or religion to do these " 'cruelties. For what? For a little egotism or " 'self-aggrandisement. And yet, what are my " 'experiments in comparison with those which are " 'daily done for nothing?' This sensibility made " Sir Astley Cooper smile, for to his human autopsies " he added hecatombs of animals. Fortunately, too, " it did not prevent Charles Bell from becoming a " brilliant operator. His 'System of Operative " 'Surgery' (a work published in 1807, which has " gone through three editions) contained no descrip-" tion of an operation he had not himself performed; " from 'bleeding in the arm, to lithotomy with the " 'knife alone; from tying the umbilical cord, to the " 'Cæsarian section.'" So that in his case, one of the most brilliant operators, vivisection was not necessary. At page 127 I find this, "In his study of " the system of circulation, as in that of the nerves, " Charles Bell was necessarily compelled to make " more than one experiment in comparative anatomy, " but he abstained as much as possible from torturing " animals, which he considered, in most cases, a " useless act of cruely, less certain in result than was " commonly supposed, and less profitable than an " attentive study of pathological phenomena, because " vivisection not only alters the substance of the " mutilated organs, but disturbs, more or less pro-" foundly, the natural condition of life, and excites " through pain irregular motions, differing from " those expected or previously observed, &c. He " admits that such is not the opinion of some of the " best and most virtuous men he has ever known, but

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"that for his own part he never could convince himself "either by the experiments he witnessed or by any "of those related to him." At page 140 I find this, "The function of the cerebellum is demonstrated as "regards animals, but man has not been subjected to "these experiments like rabbits and pigeons. Pathology "alone could not tell us whether there is a "perfect identity here between the human and "animal creation." At page 118 it says, speaking of an experiment which he wished to perform, that "he never could be induced to make the first experiment "on a human subject," though he tried it on a monkey. Our object in reading those two passages is to show the tendencies on the part of these men not to stop with the animals in their experiments.

(*The witness was directed to withdraw.*)

(*After a short time, he was again called in.*)

6421. (*Chairman.*) The Commission wish me to suggest to you that the opinions of this writer upon Sir Charles Bell and his conduct —— ?—Excuse me for interrupting you, but I am not reading the opinions of Pichot, but those of Sir Charles Bell himself.

6422. Will you allow me to say first what the Commission wish me to say, which is that their opinion is that the views of this writer upon Sir Charles Bell are not evidence which is likely to assist them in forming their judgment ?—I thought I had confined myself to what he quoted from Sir Charles Bell's writings; but at all events in future I will take care, as far as I can, in reading from the book, to confine myself to Bell's statements.

6423. You will allow me to say that this is the third occasion on which we have had the pleasure of seeing you, and we are desirous now to bring your evidence within proper and practicable limits ?—Of course. I have no wish to the contrary. I am not here from pleasure. Then at page 202 I read this: "'Pain,' remarks Sir Charles Bell in his 'Anatomy "and Philosophy of Expression,' 'is affirmed to be an "'unmitigated evil; yet pain is necessary to our "'existence. At birth it rouses the dormant faculties, "'and gives us consciousness. To imagine the absence "'of pain is not only to imagine a new state of "'being, but a change in the earth and all upon it. "'As inhabitants of earth, and as a consequence of "'the great law of gravitation, the human body must "'have weight. It must have bones as columns of "'support and levers for the action of the muscles; "'and this mechanical structure implies a complication "'and delicacy of texture beyond our conception. "'For that fine texture a sensibility to pain is "'destined to be the protection; it is the safeguard "'of the body; it makes us alive to those injuries "'which would otherwise destroy us, and arms us to "'avoid them.'" We give that evidence from Sir Charles Bell, because it is stated that many of these experiments are made with a view to the alleviation of pain. Some people seem to think that pain may be in time entirely abolished.

At page 198 there is this quotation from Sir Charles Bell: "Whoever has sat on a sunny stone in the midst "of a stream, and played with the osier twigs and "running waters, must, if he have a soul, remember "the day should he live a hundred years; and to "return to such a spot after twenty years of a struggling "life in the great world of man's invention; to "come back thus to nature in her simple guise; again "to look up to the same dark hill; again to the same "trees, still in their youth and freshness; the same "clear running waters;—if he can do this, and think "himself better than a cork floating on the stream, he "has more conceit than I." The book proceeds: "The great Newton, a short time before his death, "uttered this memorable sentiment: 'I do not know "'what I may appear to the world, but to myself I "'seem to have been only like a boy playing on the "'seashore, and diverting myself in now and then "'finding a smoother pebble or a prettier shell than "'ordinary, whilst the great ocean of truth lay all "'undiscovered before me.'" Men are very apt to exaggerate the value of these discoveries, as they call them. We think it is well to show what a really great mind thought of his discoveries, which were of vast importance to mankind.

6424. You must allow me to suggest to you that we have many of us heard that passage from Sir Isaac Newton before ?—I have no doubt of it.

6425. And that what we want is evidence which has some reference to the question of trying experiments upon living animals, into which alone the Crown has commissioned us to inquire; and the evidence which you have given us in the two last passages appeared to me not to have the smallest bearing whatever upon the subject referred to us by the Crown ?—I am sorry, my Lord, you should think so. To us they seem very cogent and to the point, for the reason which I have just stated. I have given the opinion of a very eminent modern surgeon. I will now give the opinion of a Roman surgeon, as corroborating Sir Charles Bell. I have here a translation of the eight books of Celsus on Medicine, second edition, by G. F. Collier, M.D. London: Simpkin and Marshall, 1831. At page 7 I find this: "But now "remains . . . . the "most appalling."

6426. Do you quote that as showing anything bearing upon the subject to which our inquiry has reference ?—I am surprised, my Lord (I say it with all courtesy), that you should ask me such a question. I am reading, as I said, the opinion of a Roman medical man on the subject of vivisection.

6427. You think it calculated to assist us in coming to a conclusion ?—I should think so, most decidedly. Most emphatically do I say so. It ought to do so. That he was no ordinary man this is a proof: here is a translation of his book by an English physician, made only a few years ago. At page 13, I find this: "To return to . . . . "conformable to humanity." I propose now to quote from the "Sporting Magazine," January 1825.

6428. What do you propose to read us the "Sporting Magazine" for ?—To show the opinion of a very skilful man, a veterinary surgeon, in regard to vivisection.

6429. In the year 1825 ?—That is the date of this volume; perhaps not of the particular number of the "Sporting Magazine."

(*The witness was directed to withdraw.*)

(*After a short time he was again called in.*)

6430. (*Chairman.*) The Commission wish you to confine yourself to evidence directly calculated to assist them in coming to a conclusion upon the point referred to them, and they wish me to repeat to you their opinion that some of the passages which you have recently quoted are not of that character ?—Well, I am at a loss what to do, because the evidence that our society wishes to tender to the Royal Commission through me, the Commission thinks is not to the purpose.

6431. The Commission think that some of that which you have tendered is not at all calculated to assist them in coming to a conclusion. They wish you in your future evidence to furnish them with that which may render them some assistance ?—That is what I have been trying to do all along; it appears that I have not succeeded. I thought that the opinion of this man, this veterinary surgeon, a man of considerable eminence, had, at all events, a value as his opinion.

6432. What we understand you to tender to us is an opinion ?—I hope that you will not think the worse of it because it is in the "Sporting Magazine;" it is anything but trifling.

6433. What we understand you to tender to us is an opinion expressed in the "Sporting Magazine" in 1825, that is to say, more than 20 years before the discovery of anæsthetics, upon the subject of the Bill brought in by Mr. Martin ?—Yes.

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6434. We do not think that that is calculated to assist us in the inquiry which we are prosecuting?—I am sorry to hear it, because we thought we had convinced the Commission by this time that as to so-called experimenting on animals under anæsthetics, it was, in the great majority of cases, a mere delusion and a snare in regard to the public. Among those that I mentioned, I gave a very recent instance in which Professor Ferrier was concerned, and you saw what that amounted to,—the animals uttering shrieks and gnawing their own legs, and giving excessive manifestations of agony; and yet one gentleman says they felt no more than a pianoforte. We will leave the public to judge of that, with the common sense of the English nation.

6435. We should be obliged if you will have the kindness, in your future evidence, to give us what is directly to the point?—I must hold my ground as to that. I believe that is what I have been doing. We differ, unfortunately, in opinion. It is not my wish to come here, except for the purpose of transacting this very painful business; and not one word should I say, except what I think is tending to that end. Then am I to understand that you decline to receive this?

6436. We do?—Very good. The next book I will refer to is, "Memoirs of John Abernethy, with a view " of his Lectures, &c., by George Macilwain, Fellow " of the Royal College of Surgeons. Third edition. " London: Hatchard and Company, Piccadilly, 1856." These are the words of Abernethy himself, at page 212: "Mr. Hunter, whom I should not have " believed to be very scrupulous about inflicting suf" ferings upon animals, nevertheless censures Spalan" zani for the unmeaning repetition of similar experi" ments. Having resolved publicly to express my " own opinion with respect to this subject, I choose " the present opportunity to do it, because I believe " Spalanzani to have been one of those who have tor" tured and destroyed animals in vain. I do not " perceive that in the two principal subjects which " he sought to elucidate, he has added any important " fact to our stock of knowledge; besides, some of his " experiments are of a nature that a good man would " have blushed to think of, and a wise man ashamed " to publish, for they prove no fact requiring to be " proved, and only show that the aforesaid Abbé was " a filthy-minded fellow."

At page 99 of the same book I find this: "In the " foregoing . . . . most " alluring science."

6437. Is that last passage Mr. Macilwain's opinion or Mr. Abernethy's?—The former quotation that I read from the book is in Abernethy's own words. This is partly from Abernethy and partly from himself, Mr. Macilwain; most of it from Macilwain.

6438. We have had Mr. Macilwain as a witness here before us, therefore it is not necessary for you to read his sentiments to us from a book?—I do not know, my Lord; you may not have exhausted the subject through Mr. Macilwain; I understood him to say that you did not. At page 101 I read this, "As we have already observed we think it demonstrable."

6439. Whose sentiments are these that you are reading now?—I do not see any inverted commas; therefore if they are not wanting through error, it would be Mr. Macilwain's.

6440. Then we will dispense with them, if you please?—Just as you please. Of course, my Lord, you have the power to receive or reject what you like, I suppose. And that I suppose applies to any other passages giving Mr. Macilwain's opinion.

6441. Yes?—Very good. I will now give the opinion of a man of the very first eminence and who is still with us, Sir William Fergusson.

6442. We have had Sir William Fergusson's evidence; we have already heard his opinion, and we do not wish to hear it therefore through you?—It is not through me. This is evidence which he gave in a court of justice on oath.

6443. We do not wish to receive it?—Very good. He says that he performed experiments once himself, and that he now regrets it. When I had the honour of attending here on a previous occasion, I asked if the Royal Commission could supply the Society for the Abolition of Vivisection (of course it is the object of the Commission to give all information possible on this subject throughout the kingdom) with the Croonian lecture delivered by Professor Ferrier to the Royal Society. As I have stated, there is an abstract published of it by the publishers of the Royal Society; but we made application for the lecture itself, which the society thinks it highly desirable to have.

6444. You have not obtained it?—No. I applied to you for it, but I have not received it.

6444*a*. We have not undertaken to furnish you with information; we are looking to you for information?—That lecture would put me in the way of giving it to you.

6445. Will you proceed with anything which you wish to say to us?—Certainly. Then you will not supply us with that?

6446. We are not possessed of it?—But you have the power to call for books and records to be furnished to the Commission, and I thought that you might supply us with it.

6447. We cannot undertake to supply the society with information, we are looking to you for information?—I imagined you had the power granted to you by Her Majesty to call for that very thing; and then if you did, it would place it in our power to tender it as evidence.

6448. (*Mr. Huxley.*) Has the society applied to Messrs. Taylor and Francis, the publishers to the Royal Society?—Yes.

6449. Did they refuse to sell a copy of their proceedings containing that lecture?—I never said it was in the proceedings. I said there was an abstract of it. The abstract we have; but what I applied for before was the lecture itself and not the abstract.

6450. Has the lecture ever been printed?—I do not know; not that I have heard of.

6451. Have you inquired of the Royal Society?—Of their publishers we have.

6452. Have you inquired at the office of the Royal Society itself?—No; I thought it would be out of the course of business; that they would consider it an intrusion on our part to ask it; and that the publishers would be the proper persons to apply to. If we could get it by applying there we would apply very soon. I suppose you have no intention to convey that it is a fact that we could have it by application?

6453. I have no intention to convey any impression whatever. I have asked you a question, and I now have your answer?—Very good; that is very clear then. I will now quote from the "Edinburgh Medical " and Surgical Journal, Volume 63, Article 1. An " experimental Inquiry into the Pathology and Treat" ment of Asphyxia, by John E. Erichsen, Lecturer " on General Anatomy and Physiology at the West" minster Hospital, London." You smile, my Lord, but I am not aware of anything in that title which should call forth smiles. At pages 16 and 17 I find this——

6454. (*Mr. Erichsen.*) Will you give the date of that, please?—January 1845.

6455. Thirty years ago?—It is none the less true for that, I suppose?

6456. No; but I have a special reason for mentioning it?—"Experiment 9. Three mongrel terriers " A, B, and C, were properly secured in such a way " that their heads might be brought into close appo" sition. A tube, furnished with a stop-cock, was " then introduced into the proximal end of the left " carotid artery of A, and another into that of the " right carotid artery of C. These vessels had pre" viously been ligatured beyond the point at which " the pipes were introduced, so that no hæmorrhage " might occur. The force of the heart's impulse in " the lateral dogs A and C was now measured by the " hæmadynamometer."

6457. (*Chairman.*) I think you are repeating what you read to us on a former occasion, are you not?—

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Not knowingly, certainly. In fact I am certain that I did not read it before. What I was reading on the last occasion was the work of Dr. John Reid, in which he referred to Mr. Erichsen.

6458. But are you going to read to us over again the same thing ?—No, I am not. I am too good a man of business, I hope, to do that. The passage proceeds, "and found to amount to from $4\frac{1}{2}$ to 5 inches " in each of them. A tube, furnished with a stop-" cock, was next adapted to the trachea of the centre " dog B, and a pipe was introduced into the distal " extremities of both its carotid arteries, which were " tied below the point at which the pipes were in-" serted. The animal did not appear in the least to " suffer from the ligature of both these arteries. One " of the jugular veins of the centre dog was then " exposed, and a ligature was passed under it, in " order that it might be punctured, so as to avoid the " occurrence of plethora and apoplexy, when the " carotid arteries of the two lateral dogs were con-" nected with the corresponding vessels of the central " one. The pipes in the carotid artery of A and C " were then adapted, by means of connecting pieces, to " those in the central dog, and were, besides, tightly " tied together, so that they could not slip during the " struggles of the animal. When this arrangement " had been properly and securely made, the trachea " of the centre dog was closed, the jugular vein was " punctured, and the stop-cocks connecting its carotid " arteries with those of the lateral dogs were opened. " As soon as this had been done, the vertebrals of the " dog B (the centre one) were compressed with the " fingers of an assistant, in order that the circulation " through the brain might be confined as nearly as " possible to arterial blood. The centre dog remained " quiet for about a minute and a quarter; it then " began to struggle; and in three minutes all move-" ment had ceased, and animal life was extinct. The " distal extremities of the carotid arteries of the " centre dog were then examined, as they had been " several times during the experiment, and were " found still to pulsate, although somewhat feebly, " from the impulse of the blood sent direct from the " hearts of the lateral dogs. Nearly a pint of blood " had flowed from the jugular vein during the experi-" ment, so that the animal had clearly not died from " plethora. The lateral dogs were both alive, but " evidently enfeebled by loss of blood; and on the " pressure in the carotid artery of one of them being " measured by the hæmadynamometer it was found to " amount to not more than 3 or $3\frac{1}{4}$ inches of mercury. " The centre dog was opened about 10 minutes after " its death, by which time the action of the heart had " entirely ceased." I would go on with more of these experiments, but I think one is, perhaps, sufficient; but I wish it to go down in evidence that others are detailed in the same journal.

6459. (*Mr. Erichsen.*) Those experiments were made by me, in conjunction partly with Dr. Sharpey, from a grant. We were appointed in the year 1842 by the British Association for the Advancement of Science to inquire into the subject of asphyxia. A grant of money was given by that Association for that purpose. Those experiments were made for the Association and reported to the Association, and they were considered of sufficient importance by the Royal Humane Society for them to award to me their large Fothergillian gold medal, the only time that it has been awarded, except in one instance, and that was to the present Sir James Kay Shuttleworth, for other experiments on the same subject. Are you aware of those facts? —No. I was not aware of all of them. I think I was aware of one, but I cannot be sure till I look further at this one which I referred to just now. I see that this is a report laid before the British Association; that is all that I know in regard to what you have just stated. But what you have just stated does not alter the views of our society about these experiments at all; it is only an additional proof of them. I think they are experiments from which conclusions were drawn which Dr. Reid afterwards, from his experiments, differed from. I think it will be found in my previous evidence that they led to inaccurate results. Bayle, "Dictionaire Historique et Critique de Pierre Bayle. Paris, 1820;" 12th volume; article, "Rorarius;" page 593; note C. I do not wish to appear to try to get in evidence of the nature which the Commission has declined to receive. The Chairman knows Bayle's Dictionary very well, no doubt. I am not aware if Bayle was a vivisector, but he has been quoted by Lecky, on the subject of vivisection, in his "History of European Morals."

6460. (*Chairman.*) You can put in the reference, cannot you?—Yes, I can do so; but we prefer reading the passage; it is very short.

6460a. Will you put in the reference, if you please? —As I have stated, it is at page 593; note C; article, "Rorarius."

The next book to which I will refer is the "Hand-" book of Physiology, by William Senhouse Kirkes, " M.D., edited by W. Morrant Baker, F.R.C.S., Lec-" turer on Physiology and Assistant Surgeon to " St. Bartholomew's Hospital; Surgeon to the Eve-" lina Hospital for Sick Children. Eighth edition. " London: John Murray, Albemarle Street, 1872." We produce this work in evidence as a handbook placed in the hands of young men, and to show what teaching they receive,—teaching which, in the opinion of our society, tends to demoralise their minds and to render them inhuman. At page 182 I find this: " Some experiments, performed by Dr. Kellie, ap-" peared to establish the correctness of this view. " But Dr. Burrows, having repeated these experi-" ments and performed additional ones, obtained dif-" ferent results. He found that in animals bled to " death without any aperture being made in the " cranium, the brain became pale and anæmic, like " other parts. And in proof that during life the " cerebral circulation is influenced by the same " general circumstances that influence the circulation " elsewhere, he found congestion of the cerebral " vessels in rabbits killed by strangling or drowning; " while in others killed by prussic acid, he observed " that the quantity of blood in the cavity of the " cranium was determined by the position in which " the animal was placed after death, the cerebral ves-" sels being congested when the animal was suspended " with its head downwards, and comparatively empty " when the animal was kept suspended by the ears." We think that a book of this kind is calculated to make these young men try these so-called experiments in their lodgings and elsewhere. I fancy it is pretty well known what they do do to a certain extent in that way.

6461. Before you go any further, will you allow me to ask you how many pages there are of the book altogether?—There are 835 pages.

6462. You do not propose to read any large part of them, do you?—No; they are short passages that I propose to read.

6463. In your opinion, have they any special appropriateness to this inquiry?—Yes, decidedly.

6464. You have already referred to the whole book as objectionable in your view?—When you say the whole book, there may be parts of the book which are exceedingly proper. If it tells you how to set the broken leg of a man, for instance, I should consider that quite the right thing.

6465. But I understood you to say, a little while ago, that you referred to the whole book?—I do, as far as it is imbued with that principle. I am not going to condemn every page in the book. The next passage I will read is at page 206: "The great force of the " inspiratory efforts during apnœa was well shown in " some of the experiments performed by the Medico-" Chirurgical Society's Committee on Suspended Ani-" mation. On inserting a glass tube into the trachea " of a dog, and immersing the other end of the tube " in a vessel of mercury, the respiratory efforts during " apnœa were so great as to draw the mercury four " inches up the tube. The influence of the same " force was shown in other experiments, in which the

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"heads of animals were immersed both in mercury and in liquid plaster of Paris. In both cases the material was found, after death, to have been drawn up into all the bronchial tubes, filling the tissue of the lungs." Then at page 229 I find this: "In some experiments performed by a committee appointed by the Medico-Chirurgical Society to investigate the subject of suspended animation, it was found that, in the dog, during simple apnœa, *i.e.*, simple privation of air, as by plugging the trachea, the average duration of the respiratory movements after the animal had been deprived of air was 4 minutes 5 seconds, the extremes being 3 minutes 30 seconds and 4 minutes 40 seconds. The average duration of the heart's action, on the other hand, was 7 minutes 11 seconds, the extremes being 6 minutes 40 seconds and 7 minutes 45 seconds. It would seem, therefore, that, on an average, the heart's action continues for 3 minutes 15 seconds after the animal has ceased to make respiratory efforts. A very similar relation was observed in the rabbit. Recovery never took place after the heart's action had ceased." Then lower down on the same page it says: "In proof of the correctness of this explanation, it was found that when two dogs of the same size, one, however, having his windpipe plugged, the other not, were submerged at the same moment, and taken out after being under water for two minutes, the former recovered on removal of the plug, and the latter did not."

6466. You quote these as instances of experiments described in that book which you consider to be very cruel experiments, do not you?—I do consider them very cruel.

6467. I mean that is the purpose of your quoting them?—That is partly the purpose, and also what I before stated, to show how the young are being educated.

6468. But are those examples fair samples of what you wish us to gather from that book?—What do you mean by "fair samples?"

6469. What I mean is, do they sufficiently indicate the sort of conclusion which you wish us to draw from the contents of that book?—I do not exactly see what you are leading up to.

6470. What I am pointing at is this: I want to know why we should have any more examples read to us when the whole book has been already put in by you as evidence?—But I do not put in the whole book in that way. I say it is imbued with that, but I do not say that every page contains cruelties.

6471. But do you mean to read to us every page that does?—No, I do not; I have taken a selection.

6472. Will you be so good as to be limited in your selection?—I am so.

6473. We think that you had rather forgotten that our time, and your time, and all time, is limited?—You do me an injustice in that. I do not know as to all time being limited; that remains to be proved; but as to my time, I am quite aware of that.

Then at page 244 I find this: "Walther found that rabbits and dogs, when tied to a board and exposed to a hot sun, reached a temperature of 114·8° F., and then died. Cases of sunstroke furnish us with similar examples in the case of man; for it would seem that here death ensues chiefly or solely from elevation of the temperature." A little lower down on the same page I read this: "From experiments by Walther, it appears that rabbits can be cooled down to 48° F. before they die, if artificial respiration be kept up. Cooled down to 64° F. they cannot recover, unless external warmth be applied together with the employment of artificial respiration. Rabbits not cooled below 77° F. recover by external warmth alone." Then come some experiments extracted from Magendie, Tiedemann, and Gmelin,—experiments on dogs, but I do not think it is necessary to read them. We have abstained as much as possible from going upon the continent; we have got enough at home. I should like a note, however, taken that it is in the book. Then at page 250 I find this: "One of the most notable effects of starvation, as might be expected, is loss of weight, the loss being greatest at first, as a rule, but afterwards not varying very much, day by day, until death ensues. Chossat found that the ultimate proportional loss was, in different animals experimented on, almost exactly the same, death occurring when the body had lost two fifths (40 per cent.) of its original weight." At page 251, "The effect of starvation on the temperature of the various animals experimented on by Chossat was very marked." Then at page 295, under the heading, "Digestion of the stomach after death," I find this passage: "This phenomenon is not unfrequently observed in post-mortem examinations of the human body; but, as Dr. Pavy observes, the effect may be rendered by experiment more strikingly manifest. 'If, for instance,' he remarks, 'an animal, as a rabbit be killed at a period of digestion, and afterward exposed to artificial warmth to prevent its temperature from falling, not only the stomach, but many of the surrounding parts, will be found to have been dissolved,'" and so on. Lower down on the same page I read, "It is only necessary to refer to the idea of Bernard, that the living stomach finds protection from its secretion in the presence of epithalium and mucus, which are constantly renewed in the same degree that they are constantly dissolved, in order to remark that this theory has been disproved by experiments of Pavy's, in which the mucus membrane of the stomachs of dogs was dissected off for a small space, and, on killing the animals some days afterwards, no sign of digestion of the stomach was visible." So that there is the vivisector of Paris, at least one of them, disproved here by the vivisector in London, according to this book. No man can go through the works of these vivisectionists without being very very frequently struck with the simple fact, that one man lays down a theory and a succeeding man overthrows it. And I would take this opportunity respectfully to suggest, on the part of the Society which I represent, that Her Majesty's Commission might do well to call forward some of these vivisectionists, and ask them what it is they have discovered. Let them define it; and then call forward other vivisectionists and competent professional men, and take their opinion whether they have discovered anything or not.

Then at page 297 I find this: "Dr. Pavy's theory is the best and most ingenious hitherto framed in connexion with this subject, but the experiments adduced in its favour are open to many objections, and afford only a negative support to the conclusions they are intended to prove. The matter, therefore, can scarcely be considered finally settled." On page 386 I read this, "The same fact was illustrated by some experiments of Dr. Baly, in which, having in salamanders cut off the end of the tail, and then thrust a thin wire some distance up the spinal canal, so as to destroy the cord, he found that the end of the tail was reproduced more slowly than in other salamanders in whom the spinal cord was left uninjured above the point at which the tail was amputated." At page 559 there is this passage: "The most probable account" (so I suppose there are others which are probable, but this is "the most probable") "of the particular functions which the branches of the pneumo-gastric nerve discharge in the several parts to which they are distributed may be drawn from Dr. John Reid's experiments on dogs. They show," and so on. I do not know whether that refers to the experiments that Mr. Erichsen mentioned.

At page 525 I find this: "The physiology of the cerebellum may be considered in its relation to sensation, voluntary motion, and the instincts or higher faculties of the mind. It is itself insensible to irritation, and may be all cut away without eliciting signs of pain (Longet), yet, if any of its crura be touched, pain is indicated, and, if the

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" restiform tracts of the medulla oblongata be " irritated, the most acute suffering appears to be " produced. Its removal or disorganization by " disease is also generally unaccompanied with loss or " disorder of sensibility. Animals from which it is " removed can smell, see, hear, and feel pain, to all " appearance, as perfectly as before (Flourens, Ma- " gendie). So that, although the restiform tracts of " the medulla oblongata, which themselves appear so " sensitive, enter the cerebellum, it cannot be regarded " as a principal organ of sensibility."

At page 335 I read "Instead of adopting Bernard's " view, that normally during life glycogen passes as " sugar into the hepatic venous blood and thereby is " conveyed to the lungs to be further disposed of, " Pavy inclines to believe that it may represent an " intermediate stage in the formation of fat from ma- " terials absorbed from the alimentary canal."

As I have already said to-day, if we have not succeeded in convincing the minds of the Commission in regard to our opening statement being correct, we are prepared to go on with further documentary evidence, but if we have been fortunate enough to do so, of course we need go no further. I have given to-day the opinion of Sir Charles Bell, and I think nothing could be in stronger words, and nothing could come from a man of greater eminence.

6474. I am afraid you misunderstand our relative position in this particular,—that we came here to ask you questions, and not to answer questions put by you?—I am aware of that; I stated so on a previous occasion.

6475. Now if you will proceed to finish what you have got to say, we are quite ready to hear you?—You do not say whether you are convinced or not, but in the opinion of the society we have most fully and amply established the position which we took up; and if we have not produced conviction by the evidence which we have tendered from the works of vivisectors themselves and other competent men, professional men and others, we think that no amount of evidence could do it, and therefore of course it would be unnecessary to proceed. I will therefore, if you will allow me, conclude the evidence which I have tendered to Her Majesty's Commission, with some remarks from the society itself, which is for the abolition of the very practices upon which Her Majesty's Commission is sitting to inquire. The mere reading of these cruelties creates disquietude and distress of mind, indignation, abhorrence, and in some persons even illness of body. Thousands of letters have been received by the society for the abolition of vivisection, from people of education, testifying the above. The feeling is not confined to the upper and middle classes of this nation. The Society for the Abolition of Vivisection would, had it been allowed by the Royal Commission to do so, have read some of these letters in evidence. Such strong opinion and vehement feeling could hardly have been evoked without a strong exciting cause, and as an illustrious French writer, a man of science, says, "The heart has its arguments as well as the understanding." It is the voice of nature which exclaims against these inhuman, these unnatural, these, in some cases, bestial tyranies perpetrated by man on his helpless and unoffending fellow creatures. Can deeds be right which call forth such intense feeling against them from men and women of sense and respectability? Even if any discoveries in knowledge had been extorted and wrung out by torture from these unhappy and innocent victims (and this is denied by competent authorities like Sir Charles Bell and several others), such discoveries are gained by injustice, and being so, will ultimately prove profitless and worse than profitless to the human race. Dishonesty takes the short cut to everything. It seems easier to steal than to earn. But the homely adage is true, "Ill gotten wealth seldom prospers." Samuel Johnson said of these practices that they extinguish those sensations which give man confidence in man, and make the physician more dreaded than the gout or stone. Were the so-termed discoveries made by the torturers of animals far greater than they are, the physical gain to the human race can never balance the moral evil generated by their cruelties. Science can never make an adequate gain by practices which demoralize the minds of those who perpetrate them, and who moreover at last sometimes take, as a distinguished surgeon, Sir Philip Crampton, has said, an insane gratification of cruel lust in perpetrating what has been called the "crime of fools." This groping with a lanthorn and a fork in the gutter of a shambles can never elevate mankind. To torture an innocent creature to death, be that creature man or other creature, is a wrong, and in the latter instance a most dastardly wrong. Those beings who stretch animals on the rack and inflict countless ingenious refinements of agony on creatures which they get within their power would, it seems probable, do the very same to their own particular species did they dare to do so. It has been done and may be done again. At Montpellier physicians dissected criminals alive, and even in this country suspicions connected with baby farms have arisen. We are told that the bodies of dead animals in a hospital have been packed up with the bodies of anatomised subjects, in the coffins of the latter. The money given by the benevolent public to hospitals for the cure of patients appears to be to some extent misappropriated. Money given to cure the sick, to assuage pain, is expended in inflicting torture on animals. The yearly prospectuses issued by the authorities of the medical schools are evidence which cannot be successfully denied; and are not these practices enjoined by the examining and licensing bodies and council of education? A practice wanting in dignity, as it is carried on with closed doors, is contrary to law, and demoralizing to thousands of students. I might mention, perhaps, the Brown Institution and Guy's Hospital as cases in point. This moral contagion may well spread, and what has been done in America may be done here, and the very poor and friendless be experimented on. No hard and fast line can be drawn, we are told, between man and animals, as to his physical nature. Can such a line be drawn morally? We say no. Will any vivisectionist propose to torture to death on the score of utility idiots, foundlings, paupers, or even criminals? How then can it be justifiable to torture an animal? Not on the score of mind; for an animal has mind, an idiot or an infant has none. The plea of knowledge to be obtained is no excuse; the good of humanity is no excuse. We have more knowledge already than we use for good. Every man, more or less, knowingly does wrong. A moral reform and the diffusion of knowledge are required far more than increase in knowledge. What practical follies are still generally practised by society! Look at the moral injury and the waste of wealth by drink. Look at the cruel and silly bearing rein. If every man always spoke the truth, and tried to do as he would be done by, the happiness of the world would be vastly increased, almost infinitely more so than by any physical discoveries which have ever been made. Edmund Burke even said that the manners of a people were more conducive to its happiness than were its laws. "The gods are just, and of our pleasant vices make instruments to scourge us." No scientific armour will ever protect mankind from their thongs, any more than from the sting of conscience and the agonies of remorse. Folly, vice, ignorance, dirt, and selfishness create disease. To torture animals to escape the natural penalty of viciousness is ridiculous. It is striking at the effect and not the cause. The degradation of mind which accrues from these cruelties has been well proved in the instances of Spalazani, Brachet, &c., and we believe by the French, as displayed in their unnatural experiments on hybridism. Such must inevitably tend to blunt the moral sense, and lead to the ultimate destruction and downfall of a nation. The words of an illustrious writer apply to vivisectors, "Man has a glimmering of Heaven's light. He calls " it reason, and uses it only to be the most brutal

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"of brutes,"—the words of the most intellectual of the Germans. Man is cruel. Read his history, written by himself. Southey termed it a "Chronicle of crime." Shakspere says, "The earth is wronged by man's injustice." Thomson says, "Oh, man! Tyrannic lord. "How long, how long shall prostrate nature groan "beneath thy rage awaiting renovation?" Pope called him "Foe to nature;" and Burns wrote of him

"But man, to whom alone is given
"A ray direct from pitying Heaven,
"Glories in his heart humane
"And creatures for his pleasure slain."

And again—

"Plumes himself in freedom's pride,
"Tyrant stern to all beside."

The conduct of the ruthless Spaniards towards the unoffending races of America is a case in point. They considered them as animals, and the Pope, to protect them, issued a bull asserting that they had souls, and were not to be treated as mere beasts of burden. The negro was regarded very much in the same way. And dire as were the cruelties of the slave ship, the "Legrees" of these poor wretches have been equalled by the torturers of the dumb. In the laboratory of the vivisector,—screams, looks for mercy, appeals for compassion, cries for help, convulsions of agony, are disregarded and never heard beyond its walls. The vivisectors have disclosed awful scenes of cruelty; but what are those which they have never told? All this iniquitous torture of our weaker fellow creatures tends to the incarnation of evil, namely intellect divorced from moral principle. How can mercy form part of religious instruction in the face of such education as is conveyed in Professor Huxley's "Elementary Physiology for Boys and Girls"? The practice of vivisection strikes some minds as unnatural; as absurd as it would be for an analytical chemist to pick to bits and test and scrutinise under the microscope a Titian or a Turner in the hope of discovering their systems of colouring and design. Nevertheless these purblind gropers in the entrails of animals, who begin by theft (for we hear they obtain their miserable victims from some slave in Seven Dials or St. Giles's, or the purlieus of Leadenhall Market, who brings the poor beasts, under cover of the night, to their laboratories; they thus consort and shake hands with some of the basest of mankind),—who begin by breaking the law, doing these things under the cover of secrecy,—go on day after day, month after month, year after year, torturing and murdering, callous and heartless, without pity, love, or fear, and are capable (their lives prove it) of tying down and torturing (were it possible to prevent its taking refuge in death) the same suffering creature for 50 consecutive years. Whether they torture 50 for one year apiece, or one for 50 years, what is the difference? Is this justifiable on any pretext whatever? Such beings are more detestable than the other bigots of the middle ages, and (as we are informed the late gentle and gallant Sir Hope Grant stated) "deserve to be put to death." Too many physiologists seem to fancy that the remotest prospect of a discovery useful to human beings will justify the infliction on the lower creation, as it is called, of the most excruciating pain. That position we believe to be untenable before a tribunal to which we are all amenable, however the matter may stand in a court composed of our human fellow-creatures. We would earnestly request the Commissioners to make especial inquiries of notorious vivisectors (I shall repeat myself a little here, but very little) with regard to what they themselves individually have discovered by means of vivisection; not what they affirm has been discovered, but what each one of themselves has personally found out of benefit to the human race; the opinion of independent witnesses being also ascertained as to whether the so-termed discoveries are really discoveries or not; and, moreover, whether in their opinion such discoveries, if they are esteemed so by professional and other competent and impartial witnesses, might not have been otherwise arrived at, or the so-called necessity for them be rendered nugatory by obedience to the moral and physical laws of God and Nature. The Society for the Abolition of Vivisection has no connexion with the Society for the Prevention of Cruelty to Animals of Jermyn Street, but deeply regrets the supineness of its executive, for years past, on the cruelties of vivisectors. Neither has it any connexion with the new association termed the "Society for the Protection of Animals liable to "Vivisection," as the object of our society is not permission, but the total prohibition and suppression of the torture of living animals. Any scheme for permitting and then "regulating" scientific cruelties the Society for the Abolition of Vivisection deems to be doing evil in the fallacious hope that good may come; and it denounces strongly all legislation that will license cruelty under whatever pretence, or repeal in any degree the Magna Charta of Animals, 12 and 13 Victoria, chapter 92, as to do so is palpably protecting the wrong-doer, and actually diminishing the protection to the animal creation which the existing laws of England afford. The regulation by law of cruelty is an unjustifiable and retrograde movement. Better let the law remain exactly as it now is, and enforce it, than enter into any compromise with those who advocate the infliction of systematic and unnumbered miseries in the name of science, on the helpless, dumb creation of our Common Parent. High medical and physiological authorities have given it as their opinion that researches carried out on the mangled or tortured bodies of living animals are of no value, or even worse than useless, inasmuch as they actually lead into error. From Celsus to Nélaton, eminent physiologists, medical practitioners, and others have condemned vivisection. Gall, Cuvier, Sir Charles Bell, Sir Philip Crampton, Dr. John Reid, Golding, Dr. Hull, McWhirter, Macilwain, and numbers of other scientific, medical, as well as literary men of the highest intellect endorse these views as to its uselessness. Sir William Fergusson recently stated on oath, —"I do not perform experiments myself; formerly I "did; but I now regret them." It is a belief with many persons, including even medical men, that Harvey discovered the circulation of the blood, Hunter the cure of aneurism, and Bell the nature of the nervous system by vivisection. The belief is erroneous. The Society for the Abolition of Vivisection states it as its conviction that the present time is premature for legislation, inasmuch as the nation is but slightly informed of the existence, extent, and nature of vivisection. Till that be effected there is no base of operations, no fulcrum whereon to place the lever that can move Parliament. The vivisectionists are wise in pushing on the parliamentary conflict ere a public opinion has had time to grow and spread and be a power. We want first and most a wide diffusion of the horrid details. We should then gain an overwhelming force. This is in some measure proved by thousands of letters received by the abolition society from various parts of the kingdom and from abroad, breathing the utmost sorrow, indignation, horror, and disgust at the atrocities published by the society, atrocities perpetrated in England and Scotland, and the accounts of which have in great measure been extracted from the books of vivisectors themselves. As to the assertions which have been made that the society's statements have been hastily prepared, such assertions not only are incorrect, but utterly contrary to the truth, and calculated to give a most erroneous impression. No statements made by the Society for the Abolition of Vivisection have been otherwise than accurate. When challenged, they have been made good, and the published correspondence with Dr. J. Crichton Browne, Professor Ferrier, Professor Rutherford, Sir John Rose Cormack, M.D., and that in the "Standard," "Daily News," "Morning Post," "Echo," "Globe," "Jewish Chronicle," "Scotsman," and other newspapers, has worsted every antagonist who has met the society openly in the field. As to Lord Henniker's bill for licensing vivisection, it had neither the sympathy nor the support of our society, and we so stated in the "Times." The committee of the Society for

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the Abolition of Vivisection earnestly appeals to those who prefer to protect the dumb creation to the uttermost, rather than adopt specious remedies for the prevention of cruelty, or embrace any mischievous projects of expediency; and so long as the committee continues to receive the support of the public and is encouraged to persevere, it will oppose to the uttermost all compromise with these barbarous and demoralizing outrages on nature on the sentient creatures of our Almighty Father. To this end the committee will neither ask nor accept anything less than a total prohibition of vivisection, or the mangling living animals with saws and dissecting knives, and subjecting them to other inhuman, elaborate, and prolonged sufferings. Those are the remarks which I had to make; and I wish to say this in conclusion: I had not an opportunity given me to answer a question put on a previous occasion here by the author of an educational book for the young, termed "Lessons in Elementary Physiology." The question was, "What was the object in reading passages from it?" I am desirous on the part of our society of answering that question. Do you wish me to answer it?

6476. I understood you wished to answer it?—I do wish to answer it.

6477. Will you be so good as to do so?—The object is (as the society has a strong aversion to the book as dangerously qualified to vitiate and demoralize the minds of the young), to prove from the book itself how vivisection is spreading through the land, and how the rising generation in this kingdom is being actually taught vice, and that vice the worst, namely, cruelty, (Cowper says, "the most devilish of them all,") by the infliction of torture on domestic animals; moreover to publish to the world how callous a member of the Commission is which has been expressly appointed by Her Majesty to inquire into the extent of these cruelties.

6478. You have made these statement on behalf of the society, is that so?—Yes.

6479. Were they adopted at any meeting of the society?—I do not think I am called upon to answer questions in regard to the internal arrangements of our society.

6480. You decline to do so?—I do.

6481. (*Mr. Forster.*) You are aware that you have stated over and over again in the course of your evidence that you speak on behalf of the society?—I do; I am the honorary secretary.

6482. Have you read to any committee of the society the statements that you have made to-day?—I will answer your question in the same way in which I just answered it to Lord Cardwell. In regard to the private arrangements of our society I decline to answer questions.

6482*a*. Do I rightly understand you to decline to give us any information as to what extent, and in what manner, the society is responsible for what you say?—I have given my answer already; I decline to say anything further.

6483. (*Chairman.*) You decline to mention the name of any other party than yourself who is responsible for these statements?—You have put the question to me, and I have answered it.

6483*a*. (*Mr. Huxley.*) I must put a question of the same kind, because I am familiar with the organization of societies, and know the circumstances under which societies of credit and authority act. It is for the witness to make up his mind whether he will answer the question or not, but I shall put a question, or rather a series of questions to him. My first is, when was the society instituted, answer that?—I have no objection to answer that. I cannot say for certain, but you will see the advertisement in the "Times," if you look back.

6484. I have asked you, as honorary secretary for the society, when the society was instituted?—I believe that will tell you better than I can, for I do not remember the date.

6484*a*. Was it this year or the year before?—This year.

6485. Has any general meeting of the society ever been held?—What do you call a "general meeting?"

6486. Every person who understands the organization of respectably-conducted societies —— ?—You mean to infer that ours is not one?

6487. I do nothing of the kind. On this occasion I must ask you to be so good as to confine yourself to the grammatical construction of my words?—I confine myself to the grammatical construction of your words, and the natural inference from them. I have met with something already here, and will not submit to anything further.

6488. I have now to inform you that I put these questions to you for the purpose of enabling you to understand that I have no intention of suggesting anything whatever about your society, except that it may be conducted with propriety so far as I know; but I desire to learn, not only for my own information, but for that of the public, which is very much interested in this matter, whether this society, in the name of which you have over and over again solemnly professed to act, and of which you have declared yourself formally to us to be the representative, is a society organized in the manner in which all respectably-organized societies are organized. I therefore ask you the question whether you have ever had a general meeting?—I do not know what you mean by "how respectably-organized societies are organized."

6489. I am perfectly familiar with the organization of all the most important scientific societies in London; I call those respectable societies; and I wish to ascertain whether your society, the respectability of which I do not doubt, is organized in the same fashion?—I do not know how those are organized; therefore it is impossible for me to tell you.

6490. I now ask you whether you have ever had a general meeting of your society?—We have had a meeting of the committee, some members of it.

6491. (*Chairman.*) Have you any objection to state whether the society has ever had a general meeting?—I have answered the question. I said that there had been a meeting of some members of the committee.

6492. Have you any objection to say how many?—Yes, I have, because I think you are getting very inquisitorial in regard to the private concerns of the society, with which I do not see (I speak with all courtesy) that you have anything to do.

6493. You consider that the constitution of your society is a matter of private concern, with which the public has nothing to do?—I have given my answer.

6494. (*Mr. Forster.*) You have not answered that question?—I think I did.

6495. (*Chairman.*) My question was this: You consider that the constitution of your society is a matter of private concern, with which the public has nothing to do?—Yes.

6496. (*Mr. Huxley.*) I now would ask by whom the committee was appointed?—I think that all this is very idle talk. I do not see what it concerns anybody how the committee was appointed at all.

6497. Is that your answer to my question?—Yes, it is. You can see the names of the committee advertised in the papers, at least they have been very frequently; and the committee which was first appointed still remains the committee of the society; and I must request you, if you have anything to say similar to that which you have just been saying, to communicate with them. Perhaps you had better write to Sir George Duckett, Bart., Weald Manor House, Oxfordshire.

6498. I wish to have your answer or refusal?—I have given you my answer, and have nothing else to say.

6499. Is there any treasurer to the society?—Well, what is that to you? You are not a subscriber to it.

6500. Have the accounts of the society ever been audited?—I really shall not answer any more questions of this sort. I beg you will take that answer once for all. I consider them exceedingly inquisitorial. You have no more right to put questions to

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me like that, than you have to ask what money I have in my pocket, or what I am going to have for dinner.

6501. (*Chairman.*) You have given us on several occasions what you have told us to be the views of the society?—Yes.

6502. Do I correctly understand you that now, when we inquire what the constitution of the society is, you think it an inquisitorial mode of examination, and decline to give an answer?—Most decidedly.

6503. (*Mr. Huxley.*) The only further questions that I have to put are questions on which I am sorry to occupy the time of the Commission ——?—Do not do it then.

6504. (*Chairman.*) There must be a certain amount of courtesy on your part. You have experienced a great deal of patience from us, and you must now be so courteous, and so mindful of the duties of a witness, as to listen to the questions of a Commissioner, and not interrupt him?—Allow me to answer that remark. I do not think I have laid myself open to any such remark as you have just made about courtesy, however it might apply to the other side. And as to one of the Commissioners, he said that he was sorry to take up your time with asking a question. Then why should he do it? My time is of some consequence as well as that of other persons; and when you speak of me coming here as if I was come here as a matter of favour, I come here to better inform you. Her Majesty's powers granted to you expressly state that; and I ought to be received here, and every witness, with the greatest courtesy. We are here to discharge a public duty, not here for pleasure or for profit. I come here not only at inconvenience to myself, but at positive risk to my health, as I have told you before.

6505. What you are asked to do is to listen to the end of a question before you answer it, and not to interrupt it with an answer?—I should under ordinary circumstances, but he stated that he was wasting your time, or other words to that effect, by asking it.

6506. (*Mr. Huxley.*) If the witness had waited till the end of my question, he would have heard that, in my judgment, it is a waste of time to put the question which I am about to put, but that, as my silence may be misinterpreted, I proceed to put that question. Now, in order to remove this matter entirely from the personal region into which it might drift, I may remark, that the witness referred just now to certain "Lessons in Elementary Physiology, by Thomas H. "Huxley." I wish to know upon what grounds the witness bases the statement which he made at the end of his remarks, that this book recommended the practice of vivisection to young people in schools?—My former evidence will answer that, in which I read quotations from that book.

6507. In those quotations there is nothing but a description of certain experiments, and of the results which followed from them, given with the intention, as is obvious from the book itself, of impressing the results of those experiments, which are of great importance, distinctly upon the minds of the students. So that on the face of it it does not appear that the account of such experiments encourages vivisection. Is there any distinct encouragement in the book of vivisection, anything beyond what you have just cited?—Most distinctly, I think.

6508. In what passage?—Those that I have quoted.

6509. A passage is quoted in the evidence which you have already given to this effect: "For the "purpose of acquiring a practical though elementary "acquaintance with physiological anatomy and his- "tology, the organs and tissues of the commonest "domestic animals afford ample materials." Is that, in your judgment, an encouragement to vivisection?—You must take it all together. The whole of it certainly is.

6510. I request a distinct answer to that question?—I would like to have a proof of my former evidence before me. I have not yet examined that proof, and therefore I do not know whether it is accurate or not.

(*The proof was handed to the witness.*)

6511. The passage in your evidence to which I call your attention is this: "In the preface to the second "edition I find, 'As the majority of the readers of "'these lessons will assuredly have no opportunity "'of studying anatomy or physiology upon the "'human subject, these remarks may seem dis- "'couraging. But they are not so in reality. For "'the purpose of acquiring a practical though "'elementary acquaintance with physiological ana- "'tomy and histology, the organs and tissues of the "'commonest domestic animals afford ample mate- "'rials.'" Do you consider that passage to inculcate or suggest vivisection?—It gives a strong impression to that effect, and coupled with the extracts which were taken from the work elsewhere they certainly have left a most decided impression upon me that that book does what I say.

6512. Is vivisection of the smallest use in the study of anatomy?—As I said, I came here at the request of the Commission; I did not tender myself as a witness; and, as I first stated, I do not attend here as a medical or scientific witness; and I do not wish to be entangled by any professional questions of that sort, or any others. Whether vivisection is a benefit to medicine or not, I leave abler men to decide. I have given you the opinions of many able men, who say that it does not do good, on the contrary, that it propagates error.

6513. You have based a charge here upon a definite passage in the work of this writer. You are therefore bound to show that you understand what the language, which you charge with a certain effect, means. I now put to you a question for the purpose of inquiring whether you really do know what it means; and I ask whether there is anything there to show, and whether you hold the opinion, that vivisection is of any good for anatomical purposes purely?—That I consider is a question not to the point.

6514. I ask you if you know what histology means?—I am not going to answer any professional questions, as I have said already.

6515. (*Chairman.*) But you have made a charge against this book, that it has a certain tendency, and the object of these questions is to discover upon what that charge rests?—It rests upon these passages.

6516. Then, to push it a little further, do you consider that anatomy has anything to do with experiments upon living animals?—As I said before these professional questions I must decline to answer. I stated (I must say it over again), when I acceded to the request of the Royal Commission to attend here, that I should do so not as a medical or scientific witness. Now that question is a question for a medical or scientific witness.

6517. No, what I want to know is simply this. A charge has been made by you against the writer of that book?—Most decidedly.

6518. You have quoted certain extracts as proving that charge?—Most decidedly.

6519. You are therefore asked whether you put a certain construction upon those extracts?—I do.

6520. You understand those extracts as recommending experiments upon living animals?—Most decidedly.

6521. And it was because you understood them as meaning experiments on living animals that you condemned them?—Most decidedly.

6522. If therefore they should not have that meaning, I presume your condemnation would fall to the ground?—You ask me what I consider to be an impossibility. It struck my mind as plainly as anything can do, that that book certainly does advise young people to make experiments upon living domestic animals; and I do not think that any fair and candid man of common sense can think otherwise.

6523. That is the ground upon which the charge

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is rested ?—Yes, most decidedly; the charge is made upon the book itself.

6524. (*Mr. Huxley.*) The second passage which is quoted is this (I will not read the whole of it, because the first is quite sufficient for my purpose). "If when the cord is cut across in an animal, the " cut end of the portion below the division or away " from the brain be irritated, violent movements of " all the muscles supplied by nerves given off from " the lower part of the cord take place, but there is " no sensation." Do you think that is evidence that I would have encouraged a young person to make that experiment ?—I do not want any wire-drawing about it; take the thing in the full. You have had my opinion upon it, upon all the passages put together.

6525. I take that passage, and ask you whether you consider that is evidence that I wished to encourage young persons in vivisection ?—If that is one of the passages which I quoted on a previous occasion, I quoted it with that view, and every one of them.

6526. Now then I will take you, if you please, to a passage which you have not quoted out of the same work. The passage is at page 12 of the 5th edition, and runs thus: "If the upper arm of a man, whose " arm is stretched out, be tightly grasped by another " person, the latter, as the man bends up his fore-" arm, will feel a great soft mass, which lies at the " fore part of the upper arm, swell, harden, and " become prominent. As the arm is extended again " the swelling and hardness vanish. On removing " the skin, the body which thus changes its con-" figuration is found to be a mass of red flesh " sheathed in connective tissue." Are you of opinion that that passage suggests that boys and girls should remove the skin from one another's arms ?—I do not know. I never thought of it before. I should like to take it home and look it over.

6527. The book is accessible to you. I now take you to page 16 of the same work. "Let any person " in the erect position receive a violent blow on the " head, and you know what occurs. On the instant " he drops prostrate, in a heap, with his limbs relaxed " and powerless." Do you think that that is an encouragement to boys and girls to knock one another on the head ?—I do not know. It has rather a tendency that way, I should think, if they are experimentally inclined.

6528. (*Mr. Erichsen.*) You made a statement with regard to the expenditure of funds of hospitals, about which I should like to have some definite information. You stated that money given to hospitals to cure the sick is expended to torture animals. That is the substance of what you stated ?—What I said was that money given by the benevolent public to hospitals for the cure of patients appears to be to some extent misappropriated. Money given to cure the sick, to assuage pain, is expended in inflicting torture on animals.

6529. Will you give me a single instance in which that money has been so misappropriated ?—If you get these prospectuses to which I allude, issued by the authorities of the medical schools (which you know, of course, very well) you will find statements as to experiments being performed on living animals.

6530. Do you believe that the money required for those experiments comes out of the funds of the charity ?—That is the impression that I have, certainly, and that many other people have.

6531. Are you aware that the funds expended by the medical schools for the maintenance of those medical schools, for every purpose connected with experiments in those medical schools, is paid out of the fees of the students belonging to those schools, and that those constitute funds totally distinct from those that are appropriated to the charity and that are subscribed by the public to the hospital as a charity ?—I do not understand you to say that, taking one of those prospectuses (I have not got them in my mind at this moment) you call in question that it is so as to experiments upon living animals. You see the name of Dr. so and so, and then it says that experiments will be performed on living animals, to show the action of the heart, or something or other. Now, do I understand you to say that the money which pays for the animals that are provided for that purpose does not come from the funds of the hospital ?

6532. I want you to give me an instance in which it does. You state broadly that the funds are misappropriated, and I ask you to give me a single instance. No man can make a general sweeping charge against all the medical charities of this country without, I presume, some definite ground to go on; and I want you to tell me the ground on which you go when you make that sweeping charge against the hospital authorities of this country ?—The statement is made upon the strength of prospectuses or whatever is the term for them—yearly prospectuses of these hospitals, in which it is stated (it has been so in several cases, though I think they have left them out of last year's, I know it has been so in one of them, where the statement appeared before they have taken care to leave it out) that experiments are performed on living animals.

6533. (*Chairman.*) But have you any fact to state which bears out the assertion that money contributed for the relief of the sick is applied to the purposes of experiments ?—Surely. Where does the money come from ?

6534. But do you know of any case ?—I go upon the fact of their own prospectuses. As they do spend the money in that way, I suppose they do not spend it out of their private pockets.

6535. (*Mr. Erichsen.*) Are you aware that the funds of all medical schools connected with hospitals are totally distinct from the funds of those hospitals as charities ?—I am not aware of anything of that kind.

6536. Then before you made that statement do you not think that it would have been well to have made yourself acquainted with the source from which the funds that go to medical schools come ?—I made it from the statements of the hospitals themselves. As the money does not come, I presume, out of their private pockets, it is a fair inference that it comes out of the funds of the hospitals.

6537. Are you not aware that medical students pay fees ? If you look at these prospectuses you will see a schedule of fees that they have to pay for each class which they enter, and is it not reasonable to suppose that the funds for the purpose of experiments come out of those fees, and not out of the funds of the charity ?—I do not know that at all.

6538. (*Chairman.*) At any rate you have no information on that point ?—I will just refer to some notes given me by medical men, and perhaps I shall be able to give you something more definite. (*The witness referred to his notes.*) The statement is based, as I said, upon the prospectuses issued by the hospital authorities, at least I presume they are issued by them; you can buy them in shops.

6539. But you have no knowledge of any particular case in which experiments have been paid for from any funds which were contributed for the relief of the sick ?—You mean to ask whether I am aware of any money being handed over to buy a dog or a cat to be dissected alive, or not ?

6540. I mean whether you know of any single case which furnishes an example of that general statement which you have made—any single case in which the fact took place ?—The statement is based upon the prospectuses issued by themselves; those prospectuses show that they peform experiments upon living animals, and that seems to be held out as a source of attraction.

6541. But do you not perceive that there is a complete distinction between saying that experiments on living animals are performed, and saying that the expense of that proceeding is paid for out of the funds contributed for the hospitals ?—I do not think it matters very much where it comes from, as long as the thing is done.

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6542. But the statement is that the funds are misappropriated from the purpose for which they were contributed. Do you not think it makes a difference if it turns out that those funds are wholly applied to the purpose for which they were contributed?—Most decidedly I should think so.

6543. Then I ask, are you aware of any single case in which that has not been so?—I go upon their statement that they spend their funds (for they cannot do it without funds) in the torture of animals.

6544. (*Mr. Erichsen.*) I must press this question. You have made the gravest possible charge, not against the medical officers of hospitals, who have nothing to do with the distribution of the funds, but the gravest possible charge against the great body of treasurers and governing bodies of hospitals in this country that can possibly be made against any body of gentlemen, namely, that they have misappropriated funds entrusted to them for the cure and relief of the sick, to the performance of experiments upon animals; and I ask you to give me one single instance in Great Britain or Ireland by which you substantiate that charge against these gentlemen, who are not members of the medical profession, and who have no connexion with the medical profession?—You do not deny that those experiments are performed?

6545. I ask you, yes or no, or to give me an instance?—I say then, that my answer is based upon the prospectuses of those hospitals, where they say that living animals are experimented upon. The funds, therefore, must come out of the concern somehow.

6546. I would ask you further, whether it would not have been wise for you, before making that sweeping charge, to have made yourself acquainted with the fact that those prospectuses are issued by the medical schools in connexion with the hospitals, and that the funds of those schools are totally distinct from the funds of the charities? I would ask whether it would not have been wise for you to have ascertained that first of all?—I do not know. In this matter there is a great difficulty in arriving at information, and I might have tried a long time before I got any information on the subject. There is the simple fact that it is stated in these prospectuses of hospitals that experiments are performed on living animals; and we draw the very natural conclusion that the money for so doing is taken out of the funds of the hospital.

6547. Would it not have been quite sufficient to have written to the treasurer of any one hospital in connexion with which such a prospectus is issued, and to have asked him to afford you, for your society, information upon that subiect?—I do not suppose we should have received it. We have been very often baffled in trying to get information.

6548. Do you believe that the treasurers would mislead you?—I do not know; but I know that these practices are performed in secret. I am told that the doors are closed, and that strangers are not admitted; and therefore I do not think it probable that they would be communicative on the subject.

6549. I am not speaking with reference to the medical teachers in the schools. The charge which you have made does not affect a single medical man in the kingdom, but it affects the treasurers of hospitals in the country?—I have nothing further to say.

6550. You have also stated that the medical colleges and councils either sanction or advocate vivisection. Are you aware that the Council of the College of Surgeons in London expressly states in its regulations that vivisection is not required in physiological laboratories?—No, I am not. Is that so?

6551. Most decidedly?—I am very glad indeed to hear it. It is a sign, I hope, of better days.

The witness withdrew.

APPENDIX.

APPENDIX I.

LETTER addressed by command of Her Most Gracious Majesty the QUEEN to the President of the Royal Society for the Prevention of Cruelty to Animals, by Lieutenant-General Sir T. M. BIDDULPH, K.C.B.

MY DEAR LORD, Buckingham Palace, 10th June 1874.

THE Queen has commanded me to address you as President of the Society for the Prevention of Cruelty to Animals, on the occasion of the assemblage in this country of the foreign delegates connected with similar associations, and of the jubilee of the Society, to request you to give expression publicly of Her Majesty's warm interest in the success of the efforts which are being made here and abroad for the purpose of diminishing the cruelties practised on dumb animals. The Queen hears and reads with horror of the sufferings which the brute creation often undergo from the thoughtlessness of the ignorant, and she fears also sometimes from experiments in the pursuit of science. For the removal of the former, the Queen trusts much to the progress of education; and in regard to the pursuit of science, she hopes that the entire advantage of those anæsthetic discoveries from which man has derived so much benefit himself, in the alleviation of suffering, may be fully extended to the lower animals. Her Majesty rejoices that the Society awakens the interest of the young by the presentation of prizes for essays connected with the subject, and hears with gratification that her son and daughter-in-law show their interest and sympathy by presenting those prizes at your meeting. Her Majesty desires me to announce a donation of one hundred guineas towards the funds of the Society.

I am, &c.,
T. M. BIDDULPH.

The Earl of Harrowby, K.G.,
&c. &c.

App. II.

APPENDIX II.

§ 1. Correspondence with Sir George Duckett, Bart., of Weald Manor House, Bampton, Oxfordshire, whose name is at the head of the list of the Committee of the Society for the Abolition of Vivisection.

Vivisection Commission,
13, Delahay Street, S.W.,
11th December 1875.

Sir,

I am directed to request you will have the goodness to attend here on Wednesday next, the 15th instant, at 12 o'clock, to give evidence before this Commission.

I have, &c.,
Sir G. F. Duckett, Bart., &c. &c.
Nath. Baker, Sec.

Weald Manor House,
Bampton, Oxon,
12th December.

Sir,

I shall of course be ready to attend on the Vivisection Commission, but as my testimony is in no way connected with any personal experience, and that I can simply express my horror and repugnance that in a Christian country such monstrosities should be permitted, it might be desirable to state this to Lord Cardwell, especially as I can only attend at some personal inconvenience.

All that I could say would be, what the major part of the kingdom would say,—that the practice of vivisection, an abomination introduced from the continent, is horrid and monstrous, and goes hand in hand with Atheism.

Medical science has arrived probably at its extreme limits, and has little to learn, and nothing can be gained by *repetition* of experiments on living animals.

I am, &c.,
Nath. Baker, Esq., Sec., &c. &c.
George Duckett.

P.S.—If my attendance can be dispensed with as not material, perhaps you will let me hear by return of post.

Vivisection Commission,
13, Delahay Street,
13th December 1875.

Sir,

Your letter of the 12th instant was read at the meeting of the Commission to-day, and I was directed to inform you that they were most desirous that you should attend on Wednesnay next at 12 o'clock, and to assure you that they would not *unnecessarily* put you to the inconvenience of attending.

I am, &c.,
Sir George Duckett, Bart., &c. &c.
Nath. Baker, Sec.

Weald Manor,
Bampton, Oxford,
14th December 1875.

Sir,

Being under the impression that my attendance before the Commission is not compulsory, and having explained to you, for the information of that Commission, that I had no evidence to offer, I decline to attend.

If the Commission really wish for evidence of value, which can testify to the *hellish* practice of subjecting animals to torture, I beg to say that those I have named in the margin (*see below*) are the proper persons to afford it.

I am, &c.,
Nathaniel Baker, Esq., Sec., &c. &c.
George Duckett.

Geo. Hoggan, Esq., 13, Granville Place, Portman Square.
Edwd. Curtis May, F.R.C.S., Tottenham, Middlesex.
W. B. A. Scott, Esq., 16, Stonebridge Park, Willesden, N.W.
Mr. Abel, Surgeon, Gloucester.
Mr. Carter, Surgeon, Gloucester.
Mr. Lawrence, Hotel Keeper, Gloucester.

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§ 2. Correspondence with Dr. Emanuel Klein, Assistant Professor at the Brown Institution, and Lecturer on general histology at St. Bartholomew's Hospital; and his amended proof as submitted to the Commission.

Vivisection Commission,
13, Delahay Street,
3rd November 1875.

Sir,

I beg to forward you a proof of your evidence for your correction.

I enclose a copy of a letter from Dr. Burdon Sanderson, which refers to your answers to one or two questions put to you, and shall be glad if you will inform me whether you wish to amend your answers.

I am, &c.,
Dr. E. Klein.
Nath. Baker, Sec.

49, Queen Anne Street, W.
November 1st, 1875.

My Lord,

I have been informed by a witness who appeared before the Commission last Thursday, Dr. E. Klein, that he (no doubt unintentionally) made a statement in relation to the investigations undertaken by me during the years 1873 and 1874 as to the pathology of certain infectious diseases for the Lords of Her Majesty's Council, to the effect that the living animals used in those investigations were purchased and maintained at the public expense.

It is important to me personally that this statement should be contradicted. The animals used were acquired by myself and maintained at my own expense throughout the investigation.

Dr. Klein was at this time employed by me as my assistant, and had no cognizance of my arrangements as to expenditure.

I have, &c.,
J. B. Sanderson.

The Brown Institution,
Wandsworth Road, S.W.
11th November.

Dear Sir,

May I take the liberty of asking you for another proof of my evidence, as I have unfortunately blotted my first proof so much that I cannot send it in to the printers in this condition.

Believe me, &c.,
N. Baker, Esq., Sec., &c. &c.
E. Klein.

Vivisection Commission,
13, Delahay Street,
12th November 1875.

Sir,

I beg to forward you another proof of your evidence, which I shall be glad to have back as soon as possible, for I am obliged to keep back the evidence of the 28th, and subsequent days, until I get your evidence.

I must also ask you to return the damaged proof at the same time, for I am answerable for all the copies I am supplied with.

I am, &c.,
Dr. E. Klein.
Nath. Baker, Sec.

The Brown Institution,
Wandsworth Road, S.W.,
16th November 1875.

Dear Sir,

I return you my evidence corrected. I may mention to you that when under vivâ voce examination, the fact of my being a foreigner made me often not able to appreciate all the purport of the questions, which were asked of me and that therefore my answers were not always such as I would have desired to give, if I had quite understood the questions. In revising the evidence I have endeavoured to remove the effect of any such misapprehensions.

I am, &c.
N. Baker, Esq., Sec., E. Klein.
&c. &c.

Vivisection Commission,
13, Delahay Street,
17th November 1875.

Sir,

I beg to acknowledge the receipt of your letter of the 16th instant, together with the proof of your evidence, and to reply that as your corrections appear to me to have exceeded the latitude usually allowed to witnesses, I must lay it before the Commission, and take their directions as to the course I am to pursue.

I am, &c.
Nath. Baker, Sec.

P.S.—I must ask you to return me the proof I originally sent you, which you damaged, for, as I told you when I sent you the duplicate, I am answerable for all the copies that are supplied to me.

Dr. E. Klein,
&c. &c.

Vivisection Commission,
13, Delahay Street,
19th November 1875.

Sir,

In my letter of the 12th instant, in which I forwarded you a second proof, I told you that I must have the original proof back.

On the 17th instant I again asked you to return it to me. I regret that I must now formally request you to return it to me at once.

I have, &c.
To Dr. E. Klein, Nath. Baker, Secretary.
&c. &c.

The Brown Institution,
Wandsworth Road, S.W.,
19th November.

Dear Sir,

I am very sorry to have given you so much annoyance as it appears from your letter. As I have stated on the occasion of my asking for a second proof of my evidence, the first proof has been accidentally damaged so much that it was quite useless. I therefore destroyed it and am not in a position, consequently, to comply with your request. I beg again to apologize if I should have put you to any inconvenience.

Believe me, &c.
N. Baker, Esq., Sec., E. Klein.
&c. &c.

Vivisection Commission,
13, Delahay Street,
11th December 1875.

Sir,

Your letter of the 16th November has been considered by the Commission, with the proposed alterations of your evidence. These appear to the Commission to be so much at variance with the letter and the spirit of the answers you gave at your examination, that the substitution cannot be permitted.

The Commissioners direct me to say that they observe with regret that after my letter of 12th November you should have destroyed the proof I originally sent you, and requested you to return.

If you desire it, the Commissioners will include in the Appendix your letter and the proposed alteration, with the correspondence which has passed with reference thereto.

I am, &c.
Dr. E. Klein, M.D.,
&c. &c. Secretary.

The Brown Institution,
Wandsworth Road, S.W.,
14th December 1875.

Sir,

I have received a letter which has no name subscribed to it, but which from its date and contents I presume to be written by you, informing me that the Vivisection Commission decline to receive on the ordinary footing the corrections which I made in the proof copy of my evidence, but are willing to include in the Appendix of their Report my letter and proposed alterations with the correspondence which has passed with reference thereto.

On this I must beg leave to repeat that the corrections or most of them, which I made in the proofs of my evidence, are necessary to express my real answers to the questions as I now (in reading) understand them; and that any publication of my evidence without those corrections will seriously misrepresent my meaning.

If therefore the Commission decline to accept my corrections as such, I must beg leave to withdraw the evidence as it now stands in proof.

Should the Commission, however, not permit me to withdraw it, I would respectfully request that they will allow this note to be included in their proposed publication as my protest against being held responsible for language, which does not convey my meaning and which I am told has already been much misunderstood.

With regard to the passage in your letter which expresses the regret of the Commission that after your letter of the 12th November I should have destroyed the proof you originally sent me, I beg to inform you, that that spoilt proof was destroyed before your letter of the 17th reached me. All my rough notes for the corrections had been written on it long before I received your letter of the 12th, and when I had afterwards transcribed these notes on to the second proof which you were so good as to send me, I regarded the first as merely waste paper and informed you accordingly.

Yours, &c.
N. Baker, Esq., Sec., E. Klein.
&c. &c.

Thursday, 28th October 1875.

Present:

The Right Hon. Viscount CARDWELL in the Chair.

The Right Hon. Lord Winmarleigh.
The Right Hon. W. E. Forster, M.P.
Sir J. B. Karslake, M.P.
John Eric Erichsen, Esq.
Richard Holt Hutton, Esq.
N. Baker, Esq., Secretary.

Mr. Emanuel Klein, M.D., called in and examined.

3528. (*Chairman.*) Are you Assistant Professor at the Laboratory of the Brown Institution?—Yes.

3529. Do you hold any other public appointment?—I am Lecturer on General Histology at the Medical School of St. Bartholomew's Hospital.

3530. Are you the author of the first section of this book, which is known as a handbook for the physiological laboratory?—Yes.

3531. Now are any of the experiments which you describe in that book experiments of a painful character?—Many of them are.

3532. Necessarily so?—Necessarily so.

3533. What was the intention in your view of the book as regards the practice of those experiments; are there any limitations to that practice?—It is a guide for students working in a laboratory under the supervision of some competent person who has to direct them how to perform it; that they should not perform it anyhow, but according to certain principles accepted in every laboratory.

3534. You have drawn in that book, I think, no distinction as to where anæsthetics are to be used, and where they are not?—No.

3535. Is that purposely on your part?—*It is distinctly understood that the experiments mentioned there are done under anæsthetics, except where it is expressly mentioned that they are not so.*

3536. But there is no general indication in that book, I think, that such is the case?—No, I do not think there is, nor is it necessary.

3537. Do you think there ought to have been?—Well, as I said before, it is to be a guide in a laboratory, not for private persons or amateurs; and there exist in every laboratory certain clear rules from which no one is allowed to deviate.

3538. What is your own practice with regard to the use of anæsthetics in experiments that are otherwise painful?—My own experiments do not involve very painful operations; and in them, except for teaching purposes, for demonstration, I never use anæsthetics, where it is not necessary to facilitate the proceedings. If I demonstrate, I use anæsthetics. If I do the experiments for my *pathological* research, except for convenience sake, as for instance on dogs and cats, I do not use them. On frogs and like animals I never use them.

3539. When you say that you only use them for convenience sake, do you mean that you have no regard at all to the sufferings of the animals?—No regard at all for such little suffering as is in my operations.

3540. You are prepared to establish that as a principle which you approve?—I think that with regard to an experimenter; a man who conducts special research and performs an experiment has no time, so to speak, for thinking what the animal feels or suffers. If anæsthetics ought to be used, he uses them. If not, he is like a man who performs a surgical operation in like circumstances. His only purpose is to perform the experiment, to learn as much from it as possible, and to do it as quickly as possible.

3541. Then for your own purposes you disregard entirely the question of the suffering of the animal in performing a painful experiment?—To the small extent which I have described, I do.

3542. Why do you regard it then when it is for a demonstration?—Because I know that there is a great deal of feeling against it in this country, and when it is unnecessary, one should not perhaps act against the opinion or the belief of certain individuals of the auditorium. One ought to take regard of the feelings and opinions of those persons before whom one does the experiment.

3543. Then am I wrong in attributing to you that you separate yourself entirely from the feeling which you observe to prevail in this country in regard to humanity to animals?—I separate myself as an investigator from myself as a teacher. I separate myself as an investigator, when I consult only my own feelings, from my position as a teacher, who has to consult and respect the feelings of others. I am as much opposed as anyone in this country to unnecessary or unprofitable cruelty to animals; but, for the purposes of scientific investigation, I hold that I have the same right to use the lower animals as has the sportsman and others in this country.

3544. But in regard to your proceedings as an investigator, you are prepared to acknowledge that you hold as entirely indifferent the sufferings of the animal which is subjected to your investigation?—During the time of the experiment and so far as indispensable for its purpose, yes.

3545. (*Lord Winmarleigh.*) Had you practised before coming to England?—Yes, in Vienna.

3546. Do you believe that that is a general practice on the Continent, to disregard altogether the feelings of the animals?—I believe that, there as here, in cases where it is necessary to inflict the pain, the experimenter (like an operating surgeon) would disregard the pain.

3547. Have you, since you have come to this country, had any proof of what you state now with regard to the different feeling that pervades the inhabitants of England with regard to the feelings of the animals on which you operate? Have you had any instances of the contrary feeling to that which you have just mentioned, on the part of Englishmen, since you have come to this country?—Yes, there is a great deal of difference.

3548. You have seen it exhibited?—Yes.

3549. Would you give the Commission an instance in which any such feeling has been exhibited?—I mean with regard to the journals; the agitation carried on in the different journals against what is called vivisection. There is no such thing abroad; there the general public takes no view, does not claim to pronounce any criticism or any judgment about scientific teaching and research in general. It assumes that men of science, like men in general, have conscience enough not needlessly to hurt brute animals.

3550. But with regard to the pupils before whom you lecture, have you ever seen any indication on their part of a disinclination to inflict unnecessary pain on animals?—If I do happen to perform a painful experiment where chloroform cannot be used, because it is against the purpose of the experiment, which is very rare, then I ask those before whom I do the experiment, whether they object to it.

3551. And what have you found?—If some of them object then I do not do it before them.

3552. Have they objected?—Very seldom. With the exception of one or two it has never happened that they have objected.

3553. But you believe that generally speaking there is a very different feeling in England?—Not amongst the physiologists; I do not think there is, but I cannot speak with authority.

3554. But amongst the people of England do you think there is a very different feeling from what exists upon the Continent on this subject?—Yes, I think so. In England they seem more disposed to take care of other people's consciences in matters they do not clearly understand.

3555. Seeing that there is that feeling, have you found that in yielding to that feeling in your lectures your experiments have not been so effective as they would have been if you had acted without anæsthetics?—Well, really I could not say. I have had no opportunity of judging.

3556. How long have you practised in England?—Four years and a half now.

3557. Always in London?—Always in London.

3558. You have never given lectures in the country?—No. I never perform experiments in my lectures; I only perform experiments for teaching purposes in my private class.

3559. To demonstrate?—To demonstrate and teach.

3560. Could you tell the Commission any particular experiment in which you think it is absolutely necessary to act in demonstration without anæsthetics?—Supposing, for instance, one intends to bleed a young dog to death by opening a vessel. With young dogs it is almost impossible to produce narcosis quickly, it takes always a considerable time; it is much sooner done by an assistant fixing the dog; at a single incision the carotid may be exposed, and the vessel may be opened in a few seconds, whereas with chloroform it would take a much longer time; the animal would howl, and would, I have no doubt, suffer a great deal more than when it was done without anæsthetics.

3561. Would the dog howl and exhibit pain while under anæsthetics?—While being chloroformed it would, and would suffer more pain and discomfort than if a single incision were made.

3562. Could you explain more in detail why you think it necessary, besides the point of time, to abstain from the use of anæsthetics in cases of research?—Let me again explain that my experiments do not involve severely painful operations, and I therefore am speaking of the (so to speak) superfluous use of anæsthetics. Where superfluous, they are to be objected to, because the whole attention of the investigator ought to be given to the purpose of the experiment. He has made clear to himself what he is going to do, how he is going to do it, and what he is going to learn by it. He generally chloroforms a dog when he experiments on a dog for convenience sake, in order not to be disturbed by the howling and the resistance; and so with cats. He does not do it with frogs. I do not think we have any right to regard the sensibility and feeling of a frog as being of a very high degree. And just as little as a sportsman or a cook goes inquiring while the sportsman is hunting or the cook putting a lobster into boiling water, just as little as one may expect these persons go inquiring into the detail of the feeling of the animal, just as little can the physiologist or the investigator be expected to devote time and thought to inquiring what this animal feels while he is doing the experiment. His whole attention is only directed to the making the experiment, how to do it quickly, and to learn the most from it that he can.

3563. But do you think that where it is only a question of time a professor of physiology is not bound to consult humanitarian feelings?—Certainly; but I must again draw a distinction between an investigator and a professor of physiology. I understand a professor of physiology to be a man who teaches, and there I think it is quite right that when one performs an experiment before a class one should use anæsthetics, but an investigator has no occasion to use anæsthetics except from the real necessity of the case and where severely painful operations are in question.

3564. Is that really the only reason that you can give for not using anæsthetics?—It is to a great extent; it is the chief reason I should say; there is no place for considering that point.

3565. (*Mr. Forster.*) You mentioned, I think, that there were certain rules in laboratories; what are those rules?—That no one is allowed to make an experiment except a professor or the assistant professor is present and tells him how to do it, and makes him acquainted with the purpose of the experiment, doing it perhaps the first time before the student is going to repeat it.

3566. Do those rules in the English laboratories say anything about anæsthetics?—Well, generally it is understood, where it is not against the purpose of the experiment, that they are used; it is a thing understood by itself.

3567. Is that a written rule?—There is no written rule that I know of any kind.

3568. Have you found much progress in physiological science lately in this country?—I think so very considerable.

3569. You find more attention given to it than when you first came?—Yes, considerably more.

3570. And several students, I suppose, that attend your lectures take great interest in it?—Yes.

3571. Do you think that many of them are carrying on private investigations?—No; I do not believe that any private person carries on investigations except in a laboratory.

3572. Do they come to your laboratory and try experiments?—Yes; those who carry on special investigations come to the laboratory of the Brown Institution, and do it under me or Dr. Sanderson.

3573. What is your connexion with the Brown Institution?—As I said, I am assistant professor in the laboratory, but I have nothing to do with it as a hospital for animals. I have only the use of the laboratory, which is part of the Brown Institution. My official position connects me with the work which is done for Mr. Simon.

3574. Your laboratory is in the rooms connected with the Brown Institution?—Yes, on the same ground.

3575. How do you get the animals that are practised upon in the Brown Institution?—I buy them.

3576. And do you buy them with funds that come out of the Brown Institution?—No; those used for pathological researches are not bought by the Brown Institution. The researches that I am conducting myself are for the medical officer of the Privy Council.

3577. (*Chairman.*) By what medical officer?—Of the Privy Council, Mr. Simon. The experiments have been very few in the last few years for special research, but those that have been made have been always in connexion with scientific investigation into the causes of disease carried out for Mr. Simon.

3578. (*Mr. Forster.*) And have the animals upon which you have been experimenting in your researches and investigations been furnished by Mr. Simon?—No. Those for teaching purposes are bought quite privately.

3579. I want to know with regard to your own private investigations, are the animals that are obtained for that purpose, and upon which you try experiments, obtained with the assistance of Mr. Simon's grant out of the Privy Council?—I do not do any private experiments on higher animals except for pathological researches for Mr. Simon. The animals for these investigations were provided by Dr. Sanderson, as whose assistant I was acting.

3580. Have you done any other?—No.

3581. What are the experiments in pathological researches?—For instance, producing artifical tuberculosis, or trying to communicate typhoid fever to the lower animals, experiments on small-pox in sheep, and on pyemia.

3582. You spoke about your own personal experience in what may be strictly called vivisection; that, I suppose, is not so much in this country as abroad?—No, not to such an extent.

3583. Can you at all recollect what was the vivisectional experiment in this country with which you had to do which caused the greatest pain without anæsthetics?—I should think applying lunar caustic to the cornea of a frog; it is mentioned at page 38 of the handbook.

3584. Would you be good enough to tell me the time that that experiment would last from the period at which you would first be obliged to give pain to the frog?—It greatly depends upon what kind of frog it is. If it is a large vigorous frog, for instance, a large frog from the Continent, it takes a much shorter time than with a small frog.

3585. This is not the frog which you get here, I suppose?—We get them from Holland.

3586. (*Chairman.*) You import them for the purpose?—Yes.

3587. (*Mr. Forster.*) Take one of those frogs, then, and tell me how long the experiment would last?—At present I do not practise it so long as the passage in the book would imply; I do not allow it to stay an hour after the cauterisation. When doing it on living frogs the animals are allowed to live say 10 or 20 minutes after cauterisation, the cauterisation itself takes little time.

3588. And what becomes of the frog after that time?—It is killed.

3589. (*Lord Winmarleigh.*) Always?—Generally killed; I may say always killed.

3590. (*Mr. Forster.*) Have you performed any experiments in England upon dogs or cats; upon dogs, for instance?—Yes, I have.

3591. What sort of experiments?—Pathological experiments; I have injected pyemic fluids into the veins of dogs to produce pyemia, or septicæmia. So with cats. Then I have used dogs for producing tuberculosis, and I have used dogs and cats for producing typhoid fever.

3592. Have you cut up any dogs or cats by vivisection?—If I inject into the vein, I must cut much as might be done in bleeding a patient from the arm; you cannot do it without cutting.

3593. Those are what you call pathological experiments?—Yes, for pathological purposes.

3594. I am now speaking of the other kind of experiments on living animals, which would be more strictly perhaps called vivisection; have you done them to dogs or cats in England?—No.

3595. But have you for private investigation?—As I mentioned just now for pathological experiments on pyemia, typhoid fever, septicæmia, and tuberculosis; not for physiological purpose.

3596. Have you, for private investigation, tried any experiments on dogs or cats, not pathological, but physiological experiments?—No, not directly for physiological experiments. I may have used a dog which has been used at the same time for pathological purposes, also for physiological.

3597. (*Chairman.*) When you use a dog for physiological purposes, do you adopt any particular mode of fastening that dog?—No, it is fastened on a large board, the four limbs are fastened.

3598. And how are they fastened?—With broad bands as when giving chloroform.

3599. (*Mr. Forster.*) You were stating that you considered it was so important to be absorbed upon the object of the experiment, that you could think of nothing else, and therefore could not really, in your opinion, afford time to consider the feelings of the animal; do you not sometimes find an inconvenient interruption from the cries of the animal?—I do then use chloroform, that is what I said; I use anæsthetics for convenience sake in the sense already spoken of.

3600. Do you ever use curare for that purpose, to stop the cries?—Curare involves other difficulties, the animal ceases to breathe becauses it paralyses the movement for breathing, and that might be against the object of the experiment. The animal, for instance, is to be kept alive for some time after the experiment is over; that could not be done after artificial respiration has been induced.

3601. But practically, has not the howling of the dogs interfered with experiments?—Dogs do howl also when you chloroform them.

3602. Do you try experiments with any animals that do not signify pain so loudly?—Rabbits.

3603. They do not howl, I suppose?—They do not.

3604. Then of course the same motive would not induce you to use chloroform in their case?—No.

3605. In fact, I suppose with rabbits you would not use chloroform?—I prefer and use choral hydrate; but, as a general rule, for my not severely painful scientific investigations, I do not use chloroform, or any other anæsthetic, except for convenience sake, in dogs and cats, and for no other animals as a general rule. There may be exceptions perhaps, but as a general rule, I think I am safe in saying I do not use it. I wish distinctly that it be understood that I refer only to such experiments as I myself make, namely, injection into the abdominal cavity and injection into the vein.

3606. You gave it as your opinion, that your views on the subject, although not shared by the British public generally, were the views of the British physiologists?—I would not say that distinctly, but I know a few of them, and I think that is the view held by them, but of course being a foreigner I have no authority of saying so.

3607. What is your precise connexion with the Government department of the medical branch of the Privy Council?—I have been engaged carrying out pathological investigations for Mr. Simon.

3608. That is to say, that Mr. Simon asks you to give him your assistance in a particular matter; there is no arrangement with you by which you may be considered an officer of the Privy Council, is there?—I do not think there is.

3609. (*Chairman.*) But the animals that are used for these purposes are paid for by the public money?—I do not know; they are provided by Dr. Sanderson. I must mention that I make extremely few pathological inquiries. I have not made any other experiments on living animals except those for artificial tuberculosis, for small-pox, and for typhoid fever. I understood that I was asked about my opinion, and I gave it, but it is another question whether I actually do make severe experiments. If I were to be called upon to perform experiments of this kind, I would perform them.

3610. (*Mr. Forster.*) I understand you to say, that as regards physiological experiments, you have not tried physiological experiments to any extent in England?—Very few indeed, and chiefly only on frogs.

3611. Have you done so at all in connexion with your employment by Mr. Simon?—No, none whatever.

3612. But what experiments you have tried have been tried in the Brown Institution at your own cost, I suppose?—In my private room. Those that I do for teaching purposes, physiological purposes just referred to, I do in my private room; I live there.

3613. Can you at all give us any idea of the number of animals that you find it necessary to obtain for experiments for medical investigations connected with the Privy Council?—At my work I do not think there have been more than 14 or 15 animals used in the last two years.

3614. (*Lord Winmarleigh.*) What were they?—There were three monkeys; of course, I would not call that vivisection; it was simply feeding them on typhoid material, and this has nothing to do with vivisection. I will leave out these therefore. Then there were three dogs and two cats, and a few mice, four white mice, I think, and several sheep for small-pox investigation.

3615. (*Chairman.*) Now in regard to the Brown Institution, what is your precise relation to the Brown Institution, do you receive any remuneration from it?—None whatsoever.

3616. You live in the building?—Yes.

3617. And this private room of which you speak is a private room belonging to the Brown Institution?—It belongs to the laboratory of the Brown Institution.

3618. That is to say, it is a part of the property held in trust by the London University?—I believe so.

3619. And you reside in it?—Yes.

3620. What is your particular duty to the London University?—I am ashamed to say I do not know; it has never been made clear to me what my position is there except that a title has been given to me, that of assistant professor to Dr. Sanderson, and that I am working in the laboratory, conducting pathological researches; those gentlemen who come there to undertake special pathological researches work under my direction.

3621. And are those pathological researches for the benefit of the Brown Institution?—I do not know that it has ever been put down in that way. Of course, investigations which would illustrate the nature of the diseases of animals would promote one of the objects for which the institution was founded.

3622. The pathological researches are made upon animals?—Yes.

3623. Are those animals furnished to you by the Brown Institution?—No; I make *pathological* experiments only for the medical officer of the Privy Council.

3624. Then for the London University you make no pathological experiments?—None.

3625. Then no animals are furnished to you by the London University?—None.

3626. Are these pupils of yours in any way connected with the London University?—No; they are pupils from different hospitals who wish to enter more closely into the study of microscopical anatomy.

3627. And in the teaching of those pupils you draw no distinction between painful experiments and non-painful experiments if the students themselves raise no objection to see the animal subjected to pain?—Yes; I think that would be quite what I expressed before.

3628. Therefore any students who come there, so far as your teaching and influence are concerned, adopt, I presume, the principle that you have adopted?—Yes.

3629. And consider that a physiological inquirer has too much to do to think about the sufferings of the animals?—Yes, except beforehand; may I be allowed to state, with regard to experiments for teaching purposes, that there are extremely few that I really make. I wish again to repeat that my investigations are almost entirely of a histological and pathological character, that is into the study of healthy and diseased structures.

3630. But I presume that the experiments which you perform for original research are, many of them, of a painful nature?—I have performed experiments which eventually produce pain with reference to artificial tuberculosis, because I regard it as painful to produce a disease coming on very slowly in an animal by injecting into its abdominal cavity a foreign material; but I do not regard it as a painful experiment to produce typhoid fever in an animal, seeing that the material has no effect whatsoever.

3631. But now coming to vivisection proper, you do perform in this laboratory operations which involved a great deal of pain to the animal?—Not as operations, but in their eventual results we do occasionally; of course they are very few, they are as follows: injection into the abdominal cavity and into the vein.

3632. And without any question of employing anæsthetics, unless it happens to be for your own convenience to do so?—Yes, anæsthetics would not be applicable, but the animal is generally not allowed to continue long in the diseased state.

3633. And that principle, so far as your influence goes, is derived from you by the pupils who come to benefit by your teaching?—That may be so or not, that applies only to the experiments which are for teaching purposes.

3634. (*Mr. Forster.*) With regard to the pathological experiments which you tried in connexion with Mr. Simon, you mentioned that there was one that is particularly painful, the one connected with tuberculosis?—Yes, pyemia, in its results but not in the operation itself.

3635. And that is done by injecting?—Yes, by injecting a certain fluid into the abdominal cavity, or into a vein.

3636. I suppose the operation of the injection is painful?—No, the injection itself is nothing, the results which follows thereafter may be painful.

3637. Is there no operation which is painful?—I daresay injecting into the vein of a dog is a painful operation, the making of a cut into the skin, but that is all. About as painful as bleeding.

3638. How long would that last?—Not half a minute.

3639. You give no chloroform, I suppose, when that is done?—No, it is quite unnecessary.

3640. Are there any other of these experiments which require what may be called operations in doing them?—No, only injecting into veins and injecting into the abdominal cavities. I would not regard that as a painful operation any more than a subcutaneous injection into a man for the relief of pain. Besides those I do not remember any.

3641. When you take hold of an animal for this purpose, what is done with it; do you bind it up while you are making this injection?—If it is a large vigorous animal, as a dog, we do bind it. A cat we generally must chloroform.

3642. Why do you not chloroform a dog?—I chloroform a cat because I am afraid of being scratched.

3643. Why not a dog?—If it is a small dog there is no fear of being bitten by the dog.

3644. I hardly need ask you, with your earnestness in investigation, you would run the risk of a scratch if you thought that giving chloroform would spoil the experiment?—Yes; if I had to open an artery to insert a cannula into it (I am talking only of conditions which may be; I do not say it has happened with me), I should chloroform the animal, in order not to be scratched, or the cannula to be torn out.

3645. Have you had any directions from Mr. Simon with regard to pathological investigations in consequence of any minute which I wrote when I was in the Privy Council?—Yes, I remember.

3646. Do you remember what they were?—Yes, I think I remember that it was your express desire to use animals for pathological experiments as little as possible, and to produce as little pain as possible; those were the contents.

3647. Do you recollect anything definite in those instructions, or in Mr. Simon's instructions to you upon those instructions, with regard to the use of chloroform?—The fact is that I do not remember that we used any animal strictly for vivisection, except injecting into the vein and injecting into the abdominal cavity; for one could not call it vivisection injecting lymph of small-pox into the skin of a sheep, any more than one could injecting vaccine lymph into the arm of a child.

3648. Have you ever had instructions from Mr. Simon, that with regard to experiments, pathological or physiological, conducted by you, chloroform was to be used in the case of any severely painful operation?—I had no direct relations with Mr. Simon, and besides we had no severely painful operations.

3649. Would you inform him beforehand of what you were going to do in the way of vivisection?—No, there were no vivisections, as the word is used, to be performed, and I think Mr. Simon knows too much himself about scientific investigation to interfere with an investigator.

3650. I want to know whether he has ever told you, as a general rule, for your guidance, that in any severely painful operation chloroform is to be administered for any experiment connected with the Privy Council?—The only direction which Mr. Simon gives is general. He asks us to do this or that investigation; but to say how we are to do it, or to criticise the results we obtain, I do not think he ever undertakes, and besides I have had no direct relations with Mr. Simon.

3651. (*Lord Winmarleigh.*) He never gives directions under those circumstances, whether it should be done under anæsthetics or not; is that what you mean?—There is no occasion for him to give them.

3652. (*Sir J. B. Karslake.*) Have you given us now the most painful experiments that you have ever performed by

Ir. Simon's instructions ?—I have given you a list com- rising injection of pyæmic matter or tuberculous matter ito the abdominal cavities of animals, or into the jugular ein, injecting lymph in the skin of sheep, and feeding nimals on typhoid dejecta.

3653. Then it has not fallen to your lot at present to erform what you would call a more severe experiment vith the knife than that of opening the jugular vein of an nimal ?—No other.

3654. Now is there any pain caused to a dog in the dministration of chloroform ?—I think there is; at least e see a great deal of struggling going on in the animal; ie animal struggles very hard.

3655. Before it takes effect you mean ?—Before it takes ffect.

3656. In each of those cases which you have mentioned me in which you performed the experiments by the irection of Mr. Simon, was it necessary to keep the animal live after the actual incision or actual injection ?—Yes, lways.

3657. For how long a time ?—Sometimes for weeks, ometimes for days.

3658. Then after the original injection or incision, the ain would be caused by the development of the disease ?— ooner or later, yes.

3659. In those cases, I suppose, chloroform would be seless ?—Quite so, it would not be used.

3660. As I understand you, if you were directed to per- orm an operation for the purpose of ascertaining some act, or supposed fact, with reference to the nerves of a og, and it became necessary to cut the back of the dog everely for the purpose of exposing the dog's nerves, for ie sake of saving yourself inconvenience, you would at nce perform that without the use of anæsthetics ?—Yes, if were against the purpose of the experiment.

3661. And it is only because the dog might howl, or get ito contortions, that you would use anæsthetics at all ?— es, that is to say, this would be one reason for use, where therwise the anæsthetics might be unnecessary.

3662. But it so happens that for Mr. Simon's purposes, t all events, you have never been called upon to perform uch an experiment ?—Never.

3663. Do I understand you to say, that you have any ublic class, in contradistinction to the private class you ave spoken of, of individuals who attend the laboratory ?— o public class.

3664. Then the experiments you perform involving pain animals are in the presence of certain gentlemen who ome to the laboratory for the purpose of learning ?—Yes, nd altogether there are only three or four such experi- ients.

3665. How long have you been performing those ex- eriments before a private class ?—I may say three years.

3666. Can you tell me in the course of each year how iany experiments you have performed before that class ivolving pain to animals ?—Two.

3667. Two in each year ?—Yes; sometimes two, and ometimes three.

3668. Will you give us the three which you have per- ormed ?—The cauterisation of the cornea of a frog; then ie injection into the blood vessels of a frog; then the auterisation of the cornea of a kitten, but this is done nder hydrate of chloral.

3669. That is an anæsthetic ?—Yes.

3670. And was that done in that way to satisfy the upils, or for your own convenience; I am now upon the xperiments for teaching purposes ?—That was also done or keeping the animal quiet while doing it, and for the upils convenience, and for mine.

3671. Have you ever enunciated to these private pupils vho come before you your view that a physiologist en- aged in the research should pay no attention to the pain f animals, but should avoid the use of anæsthetics ?—That, s you state it, is not my position, but I have never had ccasion to speak to pupils of private research, of special esearch.

3672. As I understand you, the case has occurred in your rivate laboratory ?—Not remonstrance exactly. I remem- er it only once; I said, "I am going to perform this or hat experiment."

3673. Just tell us what it was ?—The application of austic to the cornea of a frog. I said, "Do you object to hat," and only on one occasion I remember one said, Yes, I think it is useless."

3674. How many pupils were present at that time ?—I ave never more than five or six at a time.

3675. Did you continue to perform the experiment not- vithstanding that remonstrance ?—No, I did not do it; I lo not generally do these experiments for all the pupils at nce; because my pupils are not as in a class carried hrough instruction in the same subject, but every one works in a special subject; and so I do the experiment for one.

3676. But as I understand, the whole number of experiments before every pupil in the class, or every individual, does not exceed three ?—I mean three kinds; but the experiment may repeat itself several times.

3677. How many animals are sacrificed, do you suppose, in the course of the year for the purpose of these experiments for teaching your private pupils ?—I can say only in a rough estimate. I should not think more than 10 or 12.

3678. And of those, what is the proportion that would be of the lower animals—frogs for instance ?—Frogs form the greatest number; one kitten, perhaps, among the whole lot.

3679. You say one kitten; have you only destroyed ono kitten in these experiments ?—No; I have only made one experiment on a kitten. I have cut off the heads of many kittens, but have made an experiment on a living kitten for teaching purposes only once.

3680. Have you ever made an experiment on a living dog for teaching purposes ?—No; I think not.

3681. In the case of frogs, you never take out any part of the brain before you perform these experiments, do you ? —No.

3682. That is a short process, is it not ?—Yes.

3683. But you think it unnecessary, because you say that a physiologist has a right to do as he likes with the animal ?—Yes.

3684. Now in your own private research, what animals have you chiefly used since you have been in England ?— Guinea-pigs, rabbits, rats, mice, frogs, dogs, cats, monkeys, and sheep.

3685. But in the case of monkeys, as I understand, that was only for the purpose of ascertaining the effect that food would produce upon them ?—Yes, feeding them with typhoid material.

3686. Can you give me what, in your judgment, was the most painful experiment that in your private research you have tried since you have been in England, the one causing the most pain to the animal; I refer to the actual pain inflicted in the course of cutting the animal ?—I do not think I have done any more severe experiment than cutting the skin, and injecting into the jugular vein.

3687. That is the most painful experiment you have performed in England, you think ?—That is the most painful operation I have performed in England in my special research.

3688. I think you have said that you have been engaged in this Brown Institute a little more than four years ?— Yes.

3689. Did you come direct from Germany ?—Yes, from Vienna.

3690. Have you studied in Germany ?—Yes, in Vienna.

3691. What laboratory did you come from ?—Professor Stricker's, in Vienna.

3692. How many years had you studied under him ?— I have studied only pathology and histology under him for about four years.

3693. How many lectures of his did you attend in the course of the year ?—No lectures.

3694. Or how many demonstrations ?—His laboratory was a laboratory where special researches were carried out, and I being his private assistant was more or less connected with all the experiments that were carried out there, either directly or indirectly.

3695. And might I ask you, in the course of the last year that you were engaged as assistant to Dr. Stricker, about how many experiments were performed in his laboratory ?—I could not say.

3696. Was the number in hundreds ?—No, far less.

3697. What animals did he chiefly use ?—Rabbits and dogs.

3698. Not frogs ?—Yes, frogs also.

3699. However the largest animals he used were dogs and rabbits ?—Yes.

3700. Now there was one answer which you gave in the course of the questions put to you in which you said that other physiologists in England take the same view of the subject that you do. Do you know any physiologist that works in his private laboratory except yourself in England ? —I think that is more a matter of private talk.

3701. You have given an answer; I want to know whether you have any accurate information which enables you to say that other physiologists in England take the same view of the subject that you do ?—I have no accurate knowledge about it. I only expressed an opinion, a belief. I believe it is so; I could not prove it, and I do not know for certain whether it is so; I believe that there are other physiologists who take the same view that I do.

3702. (*Mr. Erichsen.*) You make a distinction, I suppose, between physiologists and practical physicians?—Yes.

3703. Of late years there has been a great change, has there not, in that respect in the scientific world in England. Formerly experiments were usually made by practising physicians and surgeons in order to determine some special point in practice?—Yes.

3704. For instance, a man ligatured an artery in order to see what the effect was?—Yes.

3705. But of late years a class has grown up in the medical profession, biologists and physiologists, who do not practice medicine in any way whatever, who may or may not have been educated for the medical profession, but who devote the whole of their time to the study of the laws of life upon animals?—Yes, and whose results are afterwards turned to account by the practitioners.

3706. And it was to them that your remarks were applicable?—Yes.

3707. (*Mr. Hutton.*) Did I rightly understand you to say that the frogs were always killed after this cornea operation, or only sometimes?—As a general rule they are killed.

3708. You have to excise the cornea, have you not?—Yes.

3709. And if they are not killed what is done with them? At that time they are general killed—at least to my knowledge always—but there may be one or two occasions where I have not had time myself to see whether my orders are carried out or not.

3710. (*Chairman.*) Do you mean when you say "killed" that the operation killed them?—Before the cornea is cut out the animal is generally decapitated, and then the cornea is cut out.

3711. (*Mr. Hutton.*) Whether that is always so done you are not sure?—I cannot speak with certainty about any one thing, but as far as my knowledge goes it is so.

3712. I take it that the operation of scraping the cornea which precedes, I believe, the cauterising it, would be a very painful one to the human being?—Yes; but caustic is ordinarily applied to the human eye without anæsthetics.

3713. You suppose that a frog does not suffer as a man in that operation?—Yes, I think it does not.

3714. But you say it would be a painful one; and the frog is killed at the end of an hour, or what period?—I would say 20 minutes.

3715. Is that the English or German frog?—The German frog.

3716. And how long would the English frog be kept alive?—Twice as long.

3717. Have you ever performed in England this operation described in your part of this handbook, at page 108, on the mesentery of a frog?—I have done it.

3718. It would be a much more painful one, would it not?—That is done always under curare.

3719. But it is quite an open question whether curare s, or is not, an anæsthetic for a frog?—I think it is; Schiff made some experiments to prove that it is.

3720. Claude Bernard thinks it is not, I believe?—Those that believe in experiments will, I think, agree that it is.

3721. But supposing it is not an anæsthetic, this experiment on the mesentery would be much more painful, would it not, than the cornea experiment?—Well, there is another question necessarily involved in this, namely, how much less would a frog feel if he does not breathe?—The fact is, that the frog does not breathe with his lungs after the application of curare, but breathes only through the large vein of the skin.

3722. And you think that might diminish the suffering?—I have no doubt it does.

3723. Have you performed this experiment on the mesentery of a frog before your private class, or not?—I have done it perhaps twice, not before a class, but before persons who were there for teaching purposes. I have done it several times for the purpose of pathological investigation.

3724. You have done it always under curare?—Yes; it could not be done without it.

3725. You have to keep the creature still, you mean?—Yes.

3726. And that is the reason for your using it?—Yes; and even if curare were not an anæsthetic (which I think it is) I believe that the frog would undoubtedly feel much less when the breathing is reduced to that very small amount which takes place through the vein of the skin.

3727. And have you ever done this experiment at page 98, about the injection of small mammalian animals during life?—I never did that before pupils.

3728. Have you done it in this country for private research?—I have never done it myself in England; I have done it in Vienna.

3729. Have you ever had to make experiments on arterial pressure in England, in the course of your teaching?—No.

3730. Can you give me the least idea of about the quantity of animals required for the Vienna laboratory in a year?—No.

3731. Not even an approximate number?—No.

3732. It is a very large laboratory?—Yes. Professor Stricker's laboratory is; the room is small, but the amount of work done there is very great, or it was so.

3733. And of course a considerable number of animals are used there?—Yes, very considerable.

3734. Is it at all as large as Ludwig's laboratory at Leipsic?—No.

3735. Have you ever studied there?—No.

3736. You could not give any idea what the consumption of animals there is?—Not in the least.

3737. Are there any other foreign laboratories that you have studied in?—No. I have studied only in Professor Brücke's and Professor Stricker's, in Vienna.

3738. And Professor Stricker's is the larger of the two?—Yes.

3739. And you think that the view of scientific men on the Continent is your view, that animal suffering is so entirely unimportant compared with scientific research that it should not be taken into account at all?—Yes, in cases where important results to mankind are in question, and the results cannot be got without animal suffering.

3740. Have you told us everything that is done in the Brown Institute; have you had no inquiries there but what you have mentioned in answer to Sir John Karslake?—I believe I have not omitted anything.

3741. With regard to any experiments on dogs which you have made, have they been simply cutting the carotid artery or the jugular vein?—Yes, I think that is all.

3742. There is nothing that has lasted then for any considerable time?—No; I could not remember now anything else.

3743. I thought I understood you once to say that you had performed on dogs operations that had lasted half an hour?—Half a minute, perhaps, but not half an hour.

3744. I thought you said operations that might take half an hour, but that they were never done before pupils?—I do not remember anything of the kind except bleeding a puppy to death by opening the carotid, and that is a matter of half a minute.

3745. (*Sir John Karslake.*) What was that for?—To draw out as much blood as possible, say for injection of the lymphatics of the lung. That can be done most conveniently, and with best success, by first bleeding the animal to death.

3746. (*Mr. Forster.*) Do you recollect whether Mr. Simon informed you that when I was in office I had said something to him about this, or did he give you a minute that I wrote?—I think he spoke to me about it; but really it is so long ago that I could not be certain.

3747. You cannot recollect whether he gave you a minute?—No; my business relations with him at that time were exclusively through Dr. Sanderson.

3748. You do not recollect his giving you any words written by me, to this effect, "That no experiments on "living animals should be conducted at the cost of the "State without the employment of some anæsthetic in "case of painful operation, and without a report from time "to time by the gentleman conducting the experiments, "explaining their object and showing their necessity for "the purpose of discovery." Do you recollect seeing those words?—No. May I be allowed to repeat that at that time I was not connected directly with Mr. Simon. I was at that time simply an assistant of Dr. Burdon Sanderson, so that Mr. Simon could not have had occasion to give me that instruction in an official way.

3749. When you were put directly under him you had not that minute laid before you, as I understand you?—I am only quite recently in direct business relation with Mr. Simon, and this is not in experimental investigation.

3750. (*Mr. Hutton.*) Have you ever discussed this question of the relation of animal pain to physiological inquiry with Dr. Sanderson?—I think we did.

3751. And I think that he differs from you on that point?—Yes; he does differ from me.

3752. But he understands your views on the subject?—I do not think we have entered very minutely into it; but I remember I had repeatedly occasion to say, "I do not "think it is a matter of necessity in our experiments to "administer chloroform in a general way."

3753. (*Lord Winmarleigh.*) You stated just now that you believed that curare was an anæsthetic for certain animals?—Yes.

3754. What are your reasons for that belief?—My reasons are chiefly the experiments given by Schiff; he made some experiment, and I think it proves that curare does under certain conditions act on the sensitive nerves. We know that it paralyses the mortor nerves.

3755. But Claude Bernard says, does he not, that it does not destroy the sensitiveness?—That was the general belief until the experiments of Schiff's were known.

3755. Have you read Claude Bernard's reasons for his view?—No, I have not.

3757. (*Mr. Hutton.*) You are perhaps aware that Claude Bernard describes two experiments on man with curare?—No.

3758. (*Lord Winmarleigh.*) Will you state, as shortly as you can, the exact reason for supposing that curare is an anæsthetic?—First of all, the reason which I gave to Mr. Hutton, namely, that if you poison a frog with curare you stop the breathing through the lungs; the animal breathes only through the cutanean veins; and it is quite clear that it does not keep up its vitality, its sensitiveness, to such an extent by the small amount of oxygen it may get through the vein.

3759. Would that destroy sensitiveness?—It would lower it.

3760. But not destroy it?—I do not think it would absolutely destroy it, but it would lower it undoubtedly. And secondly, I believe, with Schiff, that curare does act as an anæsthetic for this reason: if you ligature, for instance, the artery of the lower limb of a frog and then poison the animal with curare, you will find that the movement of that limb is not destroyed, because curare could not penetrate into the muscles of that region of which the artery has been ligatured. If now you pinch the leg which is *not* curarised the frog will show movement with the same limb, because its muscles are not paralysed. But if then you pinch that limb where the curare is acting on the muscles, the animal does *not* exhibit any movement with the uncurarised limb. That shows that where the muscles have been paralysed also the sensitive nerves have been paralysed.

3761. Is that movement indicative of pain?—Yes; we take it cæteris paribus, that when we pinch the frog and it does not move it does not feel.

3762. Dr. Schiff practises in Florence, does he not?—Yes.

3763. Is he a large experimentor in physiology?—Yes; from the written publications of his researches, I may say perhaps, that he is one of the largest.

3764. Are you aware whether he uses anæsthetics?—I am not. I could not say.

The witness withdrew.

App. III.

APPENDIX III.

§ 1. Vivisection. By Sir Thomas Watson, Bart., one of the Physicians in Ordinary to the Queen, and formerly President of the Royal College of Physicians.

Vivisection—the cutting into live animals for scientific purposes—is it defensible? If so, for what special purposes may it be practised, and under what conditions and limitations?

If it be true—as it certainly is true—that at a small expense of suffering in one of the lower animals, we may obtain knowledge which enables us to prevent or mitigate pain much more severe and lasting—or even to 'ward off peril to life, or to prolong life—in a human being, surely vivisection is more than justifiable. For the teacher, who must first be a learner, it may become, within assignable restrictions and conditions, nothing less than a positive duty.

What are those restrictions?

1. I hold that no vivisections are excusable which are made at random, simply to see what will happen. To justify them at all there must be some definite object in view of a previously instructed mind, some plain question to settle, some important doubt or uncertainty to remove, some hypothesis containing the promise of service to humanity to be confirmed or confuted, at least some reasonable hope and prospect of resulting benefit.

2. I hold that no man is justified in making any painful experiment upon a living creature who does not possess the skill, judgment, intelligence, and previous knowledge requisite to render the experiment successful and instructive; that all possible care should be taken by the experimenter to prevent the frustration of his object through want of foresight, of needful preparation of the necessary apparatus and implements; through a bungling use of these: or through the ignorance or stupidity of assistants; and to guard against everything that might prolong or enhance the pain which may be inseparable from the experiment, or that might defeat its conclusiveness, and so waste the painful trial altogether.

3. I hold that no teacher, or man of science, who by his own previous experiments or by his absolute knowledge of trustworthy and conclusive experiments made by others, has thoroughly satisfied himself of the solution of any physiological problem—is justified in repeating the experiments, however mercifully they may be conducted, or even in taking away the animal's life, merely to appease the natural curiosity of a class of students, or of scientific friends and acquaintances; still less for the sake of display or self-glorification.

4. If the alleged inferences from former experiments are not generally accepted by competent judges as just, or thoroughly established, then a single repetition of the experiments, to settle once for all a disputed point of importance, may reasonably be allowed.

It is probable that the general public are not fully aware of the methods whereby the pain incidental to vivisection may be abated or prevented.

There are two ways in which this most desirable end may be attained.

First, by pursuing the proposed inquiry immediately after the sudden decapitation of the animal. The pain of such decapitation is certainly very brief in duration, and presumably very slight in degree.

Some explanation becomes necessary here.

We feel, in common with all animals, by means of our nervous system. Our nervous system consists of the brain, with its prolongation or appendage the spinal cord, from which last nerves proceed and are distributed in branches innumerable to every part of the surface of the body, as well as to its internal organs. It is a demonstrable fact that the faculty of sensation resides (so to speak) in that part of the nervous system which lies within the skull. The trunk of a headless animal can feel nothing. The mere organic life that may remain within it for awhile, is perfectly analogous to the life of a vegetable; and the movements which may be evoked by mechanical or chemical irritation are as entirely without sensation of any kind, as are the movements of a sensitive plant when its leaves are handled. I hold this to be absolutely certain, although it may be difficult for any one to believe me who looks upon the animal movements, some of which appear to indicate deliberate intention or choice. But we have clear evidence in proof of my assertion, in what occasionally happens in the living human body. Disease, or injury, sometimes furnishes us with that sort of opportunity for observing, which, when we contrive to obtain it upon one of the lower animals, we call an experiment. Disease, or accident, sometimes shuts off from a part of the body the sentient faculty, by severing the spinal cord at a point where its severance is not fatal to life. What happens? The parts supplied with nerves from the severed portion of the cord are completely paralysed. The subject of this condition loses all sensation and sensibility, and all power of voluntary motion in his lower limbs. He feels in them no touch, no prick, or cut; you may burn them with a red hot iron, but he is not conscious of the burning. He cannot move a muscle belonging to the palsied limbs by the strongest effort of his will. Yet let the soles of his feet be tickled, even with a feather, and his legs and thighs start up in vigorous convulsive movements; movements which he cannot prevent or control if he wishes to do so, and of which, indeed, he himself is not even aware, unless he happens to see them.

Facts of this kind, to the reality of which I can bear personal testimony, convince us that similar movements in an animal recently beheaded—even movements which seem, but only seem, to be directed by conscious choice and purpose—are really as independent of all bodily suffering or sensation as are the motions of a mechanical automaton, contrived and constructed by human ingenuity. We view these phenomena with a deep sense of admiring wonder; wonder at the exquisite skill (if that word may with reverence be used), the incomparable skill and power manifested by the Creator, in so ordaining the fashion and endowments of the animal frame.

To proceedings such as I have now described, in which sentient life is suddenly extinguished by removing the animal's head, unjustifiable cruelty can hardly be imputed. If we are warranted in putting the lower animals to death in order to nourish and sustain the human body—and even the Brahmin cannot help doing this—we surely are warranted in taking similar means in order to learn how to cure or prevent its diseases.

It may perhaps be objected that, apart from the rapid beheading, there is in such cases no vivisection at all. The subject of the experiments is virtually dead. The animal life is gone; the slowly ebbing organic vitality alone subsists to furnish for a time, by its peculiar properties and reactions, the information of which the operator is in quest.

But—leaving this quibble—*secondly*, a temporary suspension of sensibility to pain, similar and equal to that which is absolute and final after decapitation, may be effected by the use of what are well known under the title of anæsthetic agents. And if, as is often the case, the knowledge sought for can thus be gained without destruction of life, and with little or no subsequent pain or detriment to the animal, surely this also is a proceeding exempt from blame, and not to be denounced.

There may be, there doubtless are, instances in which the manifestation of actual pain by the subject of the experiment, is the very thing necessary to test and solve the physiological question at issue. In these cases, which I believe are not very numerous, the restrictions and limitations of which I have spoken require to be most strictly observed and enforced.

If the experiment be such as to entail permanent harm, or even abiding discomfort to the animal after its sensibility is restored by the passing off of the anæsthetic influence —then it will be the most merciful, and therefore the right proceeding, to prolong that influence till the whole life, organic as well as animal (or sentient), is extinct.

Much more might be said on the whole question, but I shall have done enough if I have been enabled to calm disquietude upon the subject of vivisection, by showing that, however fearfully it may often have been abused, it may be both lawfully and mercifully practised; and that, when so practised, it is not open to the charge of wanton cruelty; but on the contrary, is deserving of our approval and gratitude. It cannot be reckoned a pleasurable exercise; and it ought never to be resorted to for the satisaction of mere curiosity, but solely for the immediate or the ultimate benefit of society, which not unnaturally is inclined to look upon it with horror and disgust.

The consolatory thought remains, that in proportion as our knowledge of the functions of the nervous system approaches to completeness, will the need for these painful methods of "interrogating nature" continually lessen, and finally cease.

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§ 2. Royal College of Surgeons of England; Extract from the Regulations respecting the Education and Examination of Candidates for the Diploma of Member of this College.

Section II.

Professional Education.

III. Candidates will be required to produce the following Certificates, viz.:—

1. Of being twenty-one years of age.
2. Of having been engaged, subsequently to the date of passing the preliminary examination, during four years, or during a period extending over not less than four Winter and four Summer Sessions, in the acquirement of professional knowledge.
3. Of having attended lectures on anatomy, during two Winter Sessions.
4. Of having performed dissections during not less than two Winter Sessions.
5. Of having attended lectures on general anatomy and physiology during one Winter Session.
6. Of having attended a practical course of general anatomy and physiology during another Winter or a Summer Session, consisting of not less than thirty meetings of the class.

Note A.—By the practical course referred to in Clause 6, it is meant that the learners themselves shall, individually, be engaged in the necessary experiments, manipulations, &c.; but it is not hereby intended that the learners shall perform vivisections.

&c., &c., &c., &c.

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§ 3. Documentary Evidence furnished by Mr. John Anthony, M.D., of Washwood Heath, near Birmingham, and referred to in his evidence.

Extracts from the Article "Innervation" in "Bowman's Physiology," published by Parker 1845, as descriptive of experiments performed by injuring or removing portions of the brain on living animals.

Spinal Cord.—Flourens found by experiment,—

If in an animal which breathes without a diaphragm, as in a bird, the spinal cord be gradually removed in successive portions, proceeding from below, the movements of respiration go on, and deglutition is performed. The higher senses are unimpaired. (Flourens.)

Medulla Oblongata.—Irritation in any part of the medulla oblongata excites convulsive movements in muscular parts which receive nerves from it.

Spasms of the glottis, difficulty of deglutition result from irritation of the medulla oblongata.

Restiform bodies.—Flourens injured the restiform bodies, and his experiments in that direction were of no value.

Cerebral Hemispheres.—When the cerebral hemispheres have been removed, as in Flourens' and Magendie's experiments, the bird can stand, walk, or fly.

Medulla Oblongata.—Experiments of Le Gallois and Flourens showed that the destruction of a portion of the medulla oblongata impairs or destroys the respiratory actions.

Corpora Striata.—(Experiments by Longet and Lafargue.) The animals remain immoveable after the removal of the corpora striata and only move after application of some external stimulus. Removal of the corpus striatum of one side caused weakness of the opposite side.

Optic Thalami.—The results of experiments add little to our knowledge of the functions of the optic thalami, the taking off successive slices or any cutting or pricking did not occasion muscular agitation, or induce contraction of the pupils.

Longet found the removal of the optic thalami in the rabbit was followed by paralysis in the opposite side of the body, but this experiment was done after the removal of the hemisphere, and the corpus striatum, and Longet also says that in his experiments on the optic thalami the paralysis was produced equally of the anterior and posterior extremities.

Destruction of Tubercula Quadrigemina.—Flourens found that the destruction of either of the tubercula quadrigemina was followed by loss of sight on the opposite side, and that the removal of both deprived the animal altogether of vision but did not affect the locomotion, or its intellectual power, or its sensibility,—except to light. So long as any portion of the roots of the optic nerves remained uninjured the iris continued to respond to the stimulus of light.

If the lobes of the brain and cerebellum were removed leaving the tubercles untouched the iricles would continue to contract.

When the injuries inflicted on the tubercula quadrigemina were deep, more or less general convulsive movements were produced. If one tubercle were injured, the opposite side only was so affected. The convulsions were due to the lesion of the central parts of the medulla oblongata.

Longet saw the same movements in pigeons in which he had *evacuated the humours of one eye.*

* *Cerebellum.*—The facility with which the cerebellum may be removed or injured, especially in birds, without involving the other segments of the brain, renders it a much more favorable object for direct experiment. A skilful operator may remove the greater part of the whole of the cerebellum without inflicting any injury on the hemispheres or other parts. Flourens removed the cerebellum from pigeons "*slice by slice.*" During the removal of the superficial layers there appeared only a slight feebleness; on reaching the middle layers, an almost universal agitation was manifested * * the animal performed rapid and ill-regulated movements, it could hear and see. After the removal of the deepest layer the animal had lost completely the power of standing, walking, leaping, or flying. The power had been injured by the previous mutilation, but now it was completely gone. When placed upon his back he was unable to rise * * he evinced an incessant restlessness * * he could see the instrument raised to threaten him with a blow, and would make a thousand contortions to avoid it but did not escape. Volition and sensation remained but that of co-ordinating these movements with combined actions was lost.

Animals deprived of the cerebellum are in a condition very similar to that of a drunken man * * * The fruitless attempts which they make to stand or walk is sufficient proof that a certain degree of intelligence remains.

Cerebellum.—After the *well known experiments of Magendie* of dividing the crus cerebelli, the animal was seen to roll over on its long axis towards the side on which the injury was inflicted.

M. Longet says, "Take two pigeons, from the one completely remove the cerebral lobes, and from the other only half the cerebellum; the next day the first will be seen firm upon his feet; the second will exhibit the unsteady and uncertain gait of drunkenness."

Corpus Callosum.—Direct experiments by Longet on the commissure of the corpus callosum yielded only negative results.

Bowman in the course of his Article on the Nervous System gives the following remarks:—"Vivisections upon "so complex an organ as the brain are ill-calculated to lead "to useful or satisfactory results; but we do not hesitate "to quote such as have been made."

[The foregoing descriptions embrace only a small portion of the experiments I saw made a matter of public exhibition at the College de France in Paris some few years prior to the publication of Bowman's standard work on physiology. I do not so much quarrel with the experiments themselves, made by the hands of such able men as Flourens, Magendie, and Longet, as I do with the series of sensational exhibitions made of them before a mixed audience in a large lecturing theatre.]

J. Anthony, M.D., Cantab.

Washwood Heath, near Birmingham.
October 31, 1875.

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* I saw a number of creatures subjected to this experiment.

App. III.

§ 4. Extract from "Archives de Physiology," volume II. of 1869, page 650. Translation read by George Hoggan, M.B. Minutes of Evidence, q. 4111.

Observations faites sur un chien curaré, par Paul Bert.

Sous le peau d'un chien vigoureux de moyenne taille, j'introduis 6 centigrammes de curaré à l'état solide et 6 centigrammes de la même substance dissous dans 6 grammes d'eau. Après quelques minutes, l'animal trébuche sur les pattes de devant marchant sur les ongles, puis il tombe et présente tous les symptômes habituels de cet empoisonnement; il défèque, urine un peu, émet beaucoup de salive visqueuse, et pleure abondamment.

J'ouvre la trachée et j'y introduis la buse d'un soufflet mis en mouvement intermittent par une chute d'eau agissant sous une roue à auges (appareil installé par M. Gréhant dans le laboratoire de M. Claude Bernard, au College de France). J'exécute ainsi une respiration artificielle très regulière.

Le nerf pneumogastrique du côté droit est mis a découvert et lié; on en fait autant pour le nerf sciatique du même côté.

Après quelques minutes, le nerf sciatique n'agit plus sur les muscles. Pendant dix heures on examine, d'heure en heure, l'action des nerfs, l'état des pupilles, &c.; et à chaque observation, on constate les faits suivants, qu'il suffira de décrire une fois.

La pupille du côté droit est contractée, celle du côté gauche très dilatée. La galvanization du pneumogastro-sympathique au cou, d'un côté ou de l'autre, améne la dilatation pupillaire et la projection de l'œil en avant; cet effet est naturellement plus marqué à droite, mais il est réel à gauche.

La galvanisation du nerf pneumogastrique d'un côté ou de l'autre arrête parfaitement le cœur.

L'iris du côté sain (côté gauche) se contracte quand on fait tomber sur lui la lumière.

Enfin, et c'est là le point sur lequel je désire appeler particulièrement l'attention, l'excitation galvanique du bout central du nerf sciatique lié, celle du nerf médian, celle du nerf sous orbitaire, amène une contraction vésicale qui, le sphincter urethral étant paralysé, se traduit par le rejet d'une petite quantité d'urine. Cet effet ne s'obtient que par l'intervention des nerfs de sensibilité appartenant à la vie animale. Ni le pneumogastrique, ni le sympathique au cou, ni les nerfs splanchniques ne donnent rien de semblable.

Après dix heures de respiration artificielle, l'animal était refroidi de 3 ou 4 degrés (température de l'air 12°).

Le lendemain matin il etait mort, et l'appareil à respiration artificielle marchait toujours; mais peut-être, pendant la nuit, y avait-il eu quelque irregularité.

L'urine contenue dans la vessie, joint à celle qu'on avait antérieurement recuillie, contenait beaucoup de sucre; la salive visqueuse, sécrétée en grande abondance, a donné avec le réactif bleu une précipité jaune peu considerable, mais manifeste. C'est la première fois, je crois, que l'on trouve du sucre dans la salive à la suite des diabétes artificiels.

Mais revenons à l'action des nerfs de sensibilité sur les contractions vesicales.

Ce fait est interressant en lui-même parcequ'il explique certaines relations connues entre la vessie et des nerfs sensitifs, tel que l'envie d'uriner consécutive à la sensation vive du froid extérieur, à l'entrée dans un bain froid ou très chaud, &c. De plus, il sera possible, en poursuivant son étude, de résoudre certaines questions très interressantes pour la physiologie. D'une part, l'origine intramédullaire, et le lieu de sortie des nerfs moteurs de la vessie pourront être par là facilement déterminés. On verra, ensuite, quelle voie suivent ces ébranlements centripètes venus, par exemple, du nerf sous-orbitaire, pour parcourir ainsi toute la longueur de la moelle épinière, s'ils s'empruntent exclusivement des cordons blancs, ou s'ils se répandent dans la substance grise. Les expériences de Budge et de M. Schiff pourront être ainsi utilement complétées.

Quoi qu'il en soit, voici un chien empoisonné par le curaré et chez qui, pendant dix heures, les actions du nerf pneumogastrique et des nerfs du grand sympathique ont persisté, soit qu'on excitât ces nerfs directement par l'électricite, soit qu'on les mit en jeu par l'intervention de leur excitant naturel, c'est à dire par voie réflexe.

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§ 5. Proposed Statute for the Limitation of Vivisection. Prepared and handed in by the Royal Society for the Prevention of Cruelty to Animals.

Whereas it is expedient to restrict the practice of vivisection and the performance of other experimental operations upon animals, and to prohibit the instruction of students, in classes and otherwise, by such means; Be it enacted:

1. The term "vivisection" in this Act shall include every experiment producing or of a nature to produce pain or disease in any animal, as well as the cutting or wounding of any living animal otherwise than for the purpose of curing or alleviating some disease with which such animal is affected, or of killing it; the term "anæsthetic" in this Act shall be taken to mean any agent which will produce complete insensibility to pain.

2. It shall not be lawful, after the first day of January one thousand eight hundred and seventy-six, to perform a vivisection save in a place which is registered in pursuance of this Act.

Notice of any place in which it is intended to perform vivisections shall be given in Great Britain to one of Her Majesty's Principal Secretaries of State (in this Act referred to as the Secretary of State), and in Ireland to the Chief Secretary to the Lord Lieutenant of Ireland (in this Act referred to as the Chief Secretary); such notice shall be signed by a member of some college of physicians or surgeons, and shall sufficiently describe the place in respect of which it is given; and the Secretary of State or Chief Secretary may cause any place with respect to which such notice has been given, to be entered on a register to be kept for the purposes of this Act in such form and under the management of such persons as the Secretary of State and Chief Secretary may respectively direct.

Subject to the consent of the Secretary of State and Chief Secretary respectively as aforesaid, an entry on the register in pursuance of this section may continue in force for twelve months from the date thereof and no longer; but such entry may be at any time renewed on a fresh notice being given as required by this section.

Any person who, after the first day of January one thousand eight hundred and seventy-six, performs a vivisection in a place which is not registered in conformity with this section, shall be deemed to have committed an offence against this Act.

3. From and after the passing of this Act no person shall perform, or cause to be performed, or take part in performing, except as herein-after provided, any vivisection without having previously obtained a license as herein-after is provided.

4. That it shall be lawful for Her Majesty's Principal Secretary of State and for the Chief Secretary for Ireland as aforesaid, immediately on the passing of this Act, or so soon thereafter as may be required, to grant a license to practise vivisection to any fellow or member of any college of physicians or surgeons, or to any graduate or licentiate of medicine, or to any person lawfully qualified to practice medicine in any part of the United Kingdom, or to any professor or teacher of anatomy, medicine, surgery, physiology, on application from such person for such purpose in the form set forth in the Schedule to this Act, accompanied by a certificate in the form set forth in the same Schedule, and signed by one at least of the following persons; viz., the President of the Royal Society, the Presidents of the Royal Colleges of Surgeons in London, Edinburgh, or Dublin, the Presidents of the Colleges of Physicians in London, Edinburgh, or Dublin, and also by a professor of physiology, medicine, or anatomy in some University in Great Britain, or recognized by the Colleges of Surgeons and Physicians aforesaid.

5. And whereas under and in pursuance of an Act passed in the second and third years of the reign of King William the Fourth, intituled "An Act for regulating schools of anatomy," certain inspectors of places where anatomy is carried on, are appointed for certain districts in Great Britain and Ireland, be it enacted that, immediately upon the registration of a place, or the granting of a license for the practice of vivisection, the said Principal Secretary of State or Chief Secretary as aforesaid respectively, shall give notice of such registration or license having been granted to the inspector so appointed for the district in which the applicant for such registration or license shall reside.

6. Every such license may at any time be revoked by such Secretary of State and Chief Secretary respectively.

7. No person shall perform, or cause to be performed, or take part in performing any vivisection while lecturing to,

or giving instruction to students in classes or otherwise, or for the illustration of lectures in schools, hospitals, or colleges, or in any other places.

8. No person shall perform or take part in performing any vivisection for the purpose of attaining manual skill.

9. No person shall perform, or cause to be performed, or take part in performing any vivisection upon any animal without having first subjected such animal to the influence of an anæsthetic so as to render it wholly insensible to pain.

10. No person who shall perform, or cause to be performed, or take part in performing any vivisection upon an animal so subjected as aforesaid, shall omit to destroy such animal before the effect of the anæsthetic ceases.

11. A license under this Act shall extend to any person assisting the holder of the license, provided the person assisting acts in the presence and under the direction of such holder.

12. A justice of the peace, on information on oath that there is reasonable ground to believe that vivisections are performed at any place not registered, or by any person not licensed in pursuance of this Act, may issue his warrant, authorising any officer of police to enter and search such place where it is alleged that such vivisections are being carried on, and to take the names and addresses of the persons found therein.

Any person who refuses admission on demand to a police officer so authorised, or who obstructs such officer in the execution of his duty under this section, or who refuses on demand to disclose his name and address, or who gives a false name or address, shall be liable to a penalty not exceeding five pounds, or to a term of imprisonment not exceeding two months.

13. A book or books shall be kept by every inspector of places where anatomy is carried on of all licenses granted by Her Majesty's Principal Secretary of State, or by the Chief Secretary as herein-before mentioned, together with the names, surnames, descriptions, and addresses of the persons to whom such registrations or licenses have been granted, together with the date of the granting or revocation of all such registrations or licenses, and such book or books shall be open at all reasonable times to inspection by any person on payment of two shillings.

14. Every person who shall be so licensed as herein-before is provided shall keep a book at the place registered as herein-before provided, in which he shall enter in plain words and figures, and with consecutive numbers, the particulars of each and every vivisection which he shall perform or take part in performing, the object and result of such vivisection, the kind of animal upon which such vivisection is performed, the kind of anæsthetic used, and the period of time which elapsed between the time at which the anæsthetic first took effect and the destruction of the animal, together with the name, surname, description, and address of the person from whom such animal was received. Such book shall be in the form set forth in the Schedule to this Act annexed, and such book, and the place registered as herein-before provided shall at all times be open to the inspection of such inspector of anatomy so appointed as aforesaid, or of Her Majesty's Principal Secretary of State or such Chief Secretary as aforesaid, or of such person as they respectively may appoint for the purpose.

15. Any person offending against any of the provisions of this Act shall forfeit and pay a penalty not exceeding ten pounds for the first offence, or imprisonment for a term not exceeding three months, and a penalty not exceeding fifty pounds for the second and every subsequent offence, or imprisonment for a term not exceeding six months, upon summary conviction before a justice of the peace.

16. The Act passed in the eleventh and twelfth years of the reign of Her Majesty, intituled "An Act to facilitate "the performance of the duties of justices of the peace out "of sessions, within England and Wales, with respect to "summary conviction and orders," shall be read with and as if it were part of this Act.

[Then follow the usual sections relating to jurisdiction in Ireland and Scotland, and to appeals, schedule, &c., &c.]

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§ 6. A Bill intituled An Act for regulating the Practice of Vivisection. (Presented by the Lord Hartismere ; ordered to be printed 4th May 1875.)

Be it enacted by the Queen's most Excellent Majesty, by and with the advice and consent of the Lords Spiritual and Temporal, and Commons, in this present Parliament assembled, and by the authority of the same, as follows:

1. This Act may be cited as the Vivisection Act, 1875.

2. It shall not be lawful, after the first day of January one thousand eight hundred and seventy-six, to perform a vivisection save in a place which is registered in pursuance of this Act.

Notice of any place in which it is intended to perform vivisections shall be given in Great Britain to one of Her Majesty's Principal Secretaries of State (in this Act referred to as the Secretary of State), and in Ireland to the Chief Secretary to the Lord Lieutenant of Ireland (in this Act referred to as the Chief Secretary); such notice shall be signed by a member of some college of physicians or surgeons, and shall sufficiently describe the place in respect of which it is given; and the Secretary of State or Chief Secretary shall cause any place with respect to which such notice has been given, to be entered on a register to be kept for the purposes of this Act in such form and under the management of such persons as the Secretary of State and Chief Secretary may respectively direct.

An entry on the register in pursuance of this section shall continue in force for twelve months from the date thereof and no longer; but such entry may be at any time renewed on a fresh notice being given as required by this section.

Any person who, after the first day of January one thousand eight hundred and seventy-six, performs a vivisection in a place which is not registered in conformity with this section, shall be deemed to have committed an offence against this Act.

3. Any inspector of anatomy may at any time visit and inspect any place which is for the time being registered in pursuance of this Act.

4. A vivisection shall not (save as herein-after mentioned), after the first day of January one thousand eight hundred and seventy-six, be performed on any animal until such animal has been placed completely under the influence of an anæsthetic; and for the purposes of this section the substance called urari or curare shall not be deemed to be an anæsthetic.

Any person may apply to the Secretary of State or Chief Secretary for a special license to perform vivisections without the use of anæsthetics, and the Secretary of State or Chief Secretary may, as he thinks fit, grant or refuse such license.

There shall be paid in respect of every such license the sum of ten pounds, and every such license shall continue in force for six months, and no longer.

Any person who performs a vivisection in contravention of this section shall be deemed to have committed an offence against this Act.

5. The Secretary of State or Chief Secretary may remove any registered place from the register on its being proved to his satisfaction that any provision of this Act has been contravened in such place.

6. A justice of the peace, on information on oath that there is reasonable ground to believe that vivisections are performed at any place not registered in pursuance of this Act, may issue his warrant authorising any officer of police to enter and search such place, and to take the names and addresses of the persons found therein.

Any person who refuses admission on demand to a police officer so authorised, or who obstructs such officer in the execution of his duty under this section, or who refuses on demand to disclose his name and address, or who gives a false name or address, shall be liable to a penalty not exceeding five pounds.

7. Any person who commits any offence against this Act for which no other penalty is imposed shall be liable to a penalty not exceeding twenty pounds.

All offences and penalties under this Act may be prosecuted and recovered in manner directed by the Summary Jurisdiction Acts before a court of summary jurisdiction.

The term "Summary Jurisdiction Acts" means as follows:

As to England, the Act of the session of the eleventh and twelfth years of the reign of Her present Majesty, chapter forty-three, intituled "An Act to facilitate the "performance of the duties of justices of the peace out "of sessions within England and Wales with respect "to summary convictions and orders," and any Acts amending the same:

As to Scotland, "The Summary Procedure Act, 1864:"

As to Ireland, within the police district of Dublin metropolis, the Acts regulating the powers and duties of justices of the peace for such district, or of the police of such district; and elsewhere in Ireland, "The Petty Sessions (Ireland) Act, 1851," and any Act amending the same:

The term "court of summary jurisdiction" means in England and Ireland any two justices of the peace or any metropolitan stipendiary or other magistrate empowered by law to do alone or with others any act authorised to be done by more than one justice of the peace; and in Scotland any justice or justices of the peace, sheriff, or other magistrate by whatever name called, proceedings before whom for the trial or prosecution of any offence or for the recovery of any penalty under any Act of Parliament, the provisions of the Summary Jurisdiction Act, 1864, may be applied:

In Scotland the following provisions shall have effect;

(1.) All jurisdiction, powers, and authorities necessary for the court of summary jurisdiction under this Act are hereby conferred on that court:

(2.) Every person found liable under this Act in any penalty shall be liable in default of immediate payment to imprisonment for a term not exceeding three months, and the conviction and warrant may be in the form of No. 3 of Schedule K. of the Summary Procedure Act, 1864.

8. In this Act, the term "vivisection" means the cutting or wounding, or treating with galvanism or other appliances, any living vertebrate animal for purposes of physiological research or demonstration, also the artificial production in any living vertebrate animal of painful disease for purposes of physiological research or demonstration.

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§ 7. A Bill to prevent Abuse in Experiments on Animals made for the purpose of Scientific Discovery. (Prepared and brought in by Mr. Lyon Playfair, Mr. Spencer Walpole, and Mr. Evelyn Ashley; ordered to be printed 12th May 1875.)

Whereas it is expedient to prevent cruelty and abuse in the experiments made on living animals for the purpose of promoting discoveries in the sciences of medicine, surgery, anatomy, and physiology:

Be it enacted by the Queen's most Excellent Majesty, by and with the advice and consent of the Lords Spiritual and Temporal, and Commons, in this present Parliament assembled, and by the authority of the same, as follows:

1. Save as herein-after mentioned, no person shall, for any purpose whatever, make an experiment causing pain, or of a nature to cause pain, on any live animal.

Any person acting in contravention of this section shall be guilty of a misdemeanor, or shall be liable, on prosecution before a court of summary jurisdiction, to a penalty not exceeding *fifty pounds*, or imprisonment for a term not exceeding *three months*.

2. Any person, for the purpose of new scientific discovery, but for no other purpose, shall be permitted to make an experiment on a live animal of a nature to cause pain notwithstanding this Act and the Prevention of Cruelty to Animals Act (passed in the twelfth and thirteenth years of Her present Majesty, chapter ninety-two), provided that the following conditions are complied with:

(1.) That the animal shall first have been made insensible by the administration of anæsthetics or otherwise, and shall continue to be insensible during the whole of such experiment; and

(2.) That, if the nature of the experiment be such as to seriously injure the animal, so as to cause to it after-suffering, the animal shall be killed immediately on the termination of the experiment.

3. Nothing in this or in the Prevention of Cruelty to Animals Act contained shall be taken to prohibit any person holding a license, as herein-after provided under this Act, from subjecting any live animal to an experiment without the use of anæsthetics, provided that in making such experiment the following conditions are all complied with:—

(1.) That the experiment is made for the purpose of new scientific discovery, and for no other purpose; and

(2.) That insensibility cannot be produced without necessarily frustrating the object of the experiment; and that the animal should not be subjected to any pain which is not necessary for the purpose of the experiment; and

(3.) That the experiment be brought to an end as soon as practicable; and

(4.) That if the nature of the experiment be such as to seriously injure the animal, so as to cause to it after-suffering, the animal shall be killed immediately on the termination of the experiment:

(5.) That a register of all experiments made without the use of anæsthetics shall be duly kept, and be returned in such form and at such times as one of Her Majesty's Principal Secretaries of State may direct.

4. Any person desirous to obtain a license under this Act may apply for the same to one of Her Majesty's Principal Secretaries of State. Such application shall be in the form set forth in the Schedule to this Act, and shall be signed by the applicant, and shall be accompanied by a certificate in form set forth in the same Schedule, and signed by one at least of the following persons; viz., the President of the Royal Society, the Presidents of the Royal Colleges of Surgeons in London, Edinburgh, or Dublin, the Presidents of the Colleges of Physicians in London, Edinburgh, or Dublin, and also by a professor of physiology, medicine, or anatomy in some university in Great Britain, or recognised by the Colleges of Surgeons and Physicians aforesaid.

Provided that if the applicant be a professor or lecturer of or in physiology, medicine, anatomy, or surgery in any university in Great Britain, or in any college incorporated by Royal charter, or a professor or lecturer in any one of such sciences employed by Government, such a certificate shall not be required; but, instead thereof, his application shall be countersigned by the registrar, president, principal, or secretary of such university or college.

5. Upon receiving such application the Secretary of State may, if he think fit, grant to the applicant a license under this Act.

6. A license under this Act shall, unless earlier revoked, be in force, in the case of the holder being a professor or lecturer as herein-before mentioned, during such time as he shall continue to hold such professorship or lectureship, and in any other case for the term of five years. Provided that from time to time, when a license expires a Secretary of State shall renew the same upon receiving a written application from the holder, without requiring a fresh certificate or any countersigning of the application.

7. A Secretary of State may at any time, on cause shown, revoke any license granted under this Act.

8. A license under this Act shall extend to any person assisting the holder of the license, provided the person assisting acts in the presence and under the directions of such holder.

9. All offences and penalties under this Act may be prosecuted and recovered in manner directed by the Summary Jurisdiction Acts before a court of summary jurisdiction.

The term "Summary Jurisdiction Acts" means as follows:

As to England, the Act of the session of the eleventh and twelfth years of the reign of Her present Majesty, chapter forty-three, intituled "An Act to facilitate the "performance of the duties of justices of the peace "out of sessions within England and Wales with "respect to summary convictions and orders," and any Acts amending the same:

As to Scotland, "The Summary Procedure Act, 1864:"

As to Ireland, within the police district of Dublin metropolis, the Acts regulating the powers and duties of justices of the peace for such district, or of the police of such district; and elsewhere in Ireland, "The Petty Sessions (Ireland) Act, 1851," and any Act amending the same:

The term "court of summary jurisdiction" means in England and Ireland any two justices of the peace or any metropolitan stipendiary or other magistrate empowered by law to do alone or with others any act authorised to be done by more than one justice of the peace; and in Scotland any justice or justices of the peace, sheriff, or other magistrate by whatever name called, proceedings before whom for the trial or prosecution of any offence or for the recovery of any penalty under any Act of Parliament the provisions of the Summary Jurisdiction Act, 1864, may be applied:

In Scotland the following provisions shall have effect:

(1.) All jurisdiction, powers, and authorities necessary for the court of summary jurisdiction under this Act are hereby conferred on that court:

(2.) Every person found liable under this Act in any penalty shall be liable in default of immediate payment to imprisonment for a term not exceeding three months, and the conviction and warrant may be in the form of No. 3 of Schedule K. of the Summary Procedure Act, 1864.

10. Any person who has been convicted of any offence punishable by this Act by any justices may appeal in England to the next general or quarter sessions of the peace which shall be held for the city, county, town, or place wherein such conviction shall have been made, in Scotland to the sheriff of the county, after the expiration of ten days from the day when such conviction shall take place, provided that such person enter into a recognizance within three days next after such conviction, with two sufficient sureties, conditioned to try such appeal, and to be forthcoming to abide the judgment and determination of the court at such general or quarter sessions, or of the sheriff, and to pay such costs as shall be by such court or sheriff awarded; and the justices or sheriff before whom such conviction shall be had are hereby empowered and required to take such recognizance; and the court at such general or quarter sessions and sheriff respectively are hereby required to hear and finally determine the matter of such appeal, and may award such costs to the party appealing or appealed against as they or he shall think proper.

In Ireland any person who has been convicted of any offence punishable by this Act may appeal to the next court of quarter sessions to be held in the same division of the county where the conviction shall be made by any justice or justices in any petty sessions district, or to the recorder at his next sessions where the conviction shall be made by the divisional justices in the police district of Dublin metropolis, or to the recorder of any corporate or borough town when the conviction shall be made by any justice or justices in such corporate or borough town (unless when any such sessions shall commence within ten days from the date of any such conviction, in which case, if the appellant sees fit, the appeal may be made to the next succeeding sessions to be held for such division or town), and it shall be lawful for such court of quarter sessions or recorder (as the case may be) to decide such appeal, if made in such form and manner and with such notices as are required by the said Petty Sessions Acts respectively herein-before mentioned as to appeals against orders made by justices at petty sessions, and all the provisions of the said Petty Sessions Acts respectively as to making appeals and as to executing the orders made on appeal, or the original orders where the appeals shall not be duly prosecuted, shall also apply to any appeal made under this Act.

Schedule.

Form of Application.

I, *M.N.*, of [*description and occupation, with particulars of any scientific, academical, or other like qualification*], do hereby declare that I am engaged in the study of the science of physiology, and especially in the making of researches, with the view of advancing physiological knowledge, and that I desire to obtain a license under the provisions of "The Experiments on Animals Act, 1875," empowering me to make experiments on living animals; and I hereby undertake that in making any experiments under such license, if granted, I will conform to the conditions and restrictions contained in sections 2 and 3 of the said Act.

Dated 187 . (Signed) *M.N.*

[*These sections to be printed on the back of the application.*]

Form of Certificate.

We, *A.B.*, President of the Royal Society, and *C.D.*, hereby certify, for the information of the Secretary of State for the Home Department, under the provisions of the Experiments on Animals Act, 1875, that the above-named *M.N.* is engaged in prosecuting such researches as are described in the above application, and that he is, in our opinion, a person properly qualified to carry on such investigations, and to have granted to him the license for which he applies.

(Signed)

Names.	Descriptions.
A.B.	
C.D.	

Dated this day of , 187 .

Form of License.

I, Her Majesty's Secretary of State for the Home Department, having received from *M.N.* residing at [*description and occupation*] an application, under the provisions of the Experiments on Animals Act, 1875, accompanied by certificate, such as is required by the same Act, of which certificate and application copies are appended hereto, do hereby, in exercise of the authority given to me by the said Act, license the said *M.N.* to make experiments on living animals, in the manner and subject to the conditions and restrictions set forth in the said Act, during the period of five years from the date hereof, [*or, in the case of a professor or lecturer, under section* , "*for so long as he may retain and fulfil the duties of his*" *office as such professor or lecturer aforesaid.*"]

Dated this day of 187 .

§ 8. Digest of Replies to Questions forwarded to the principal Medical Schools and Veterinary Colleges in Great Britain and Ireland.

Name of Medical School, &c.	1. Are any experiments performed on live animals (including frogs) for scientific or teaching purposes at —— or in the Hospitals, Schools, and Laboratories connected with it?	2. State as accurately as you can the number of such experiments performed in the course of the year.	3. State for what purposes such experiments are made, whether for original research or class demonstration, and give the relative numbers in each case.	4. State what animals (including frogs) are used, and give the numbers of each species.	5. State whether and in what proportion of cases the animals are in the state of anæsthesia, distinguish the case of frogs:—and also what anæsthetics are employed.	6. When not in that state, the reason why not.
ENGLAND.						
St. Bartholomew's - -	Yes.	See answer 4.	Experiments are made for both purposes. As to original research, the individual experimenters (always lecturers or members of the medical staff, in either case qualified medical men,) only can give information. Their experiments are made on their own responsibility, and form no part of the curriculum of the medical school. As to class demonstration, see answer 4.	Frogs, 20 or 30, and about half as many dog, cats, and rabbits (chiefly rabbits).	In all cases of class demonstration the animals are in a state of anæsthesia. The animals are killed at the end of the demonstrations. The anæsthetics employed are wourali, chloral, chloroform, ether.	See answer 5.
St. Thomas's - -	Physiology. The only living animals subjected to experiment in this department have been frogs.	Three or four.	All for class demonstration. In all cases for demonstrating the circulation of the blood in the smaller vessels of the body.	Frogs. Three or four in all.	In demonstrating the circulation in the web of the frog's foot no anæsthetics are used. In demonstrating the circulation in the mesentery, curare is used.	—
Guy's - - -	Yes.	These amount to from 40 to 50 a year for the purposes of class demonstration. Experiments for original research vary in number with the nature and extent of the inquiries that are being prosecuted by the physiologists, and usually average from 15 to 20 in the course of a year, taking an average of years.		Dogs, rabbits, guinea-pigs, frogs, and occasionally rats. The greater number consist of dogs, rabbits, and frogs in about equal proportion.	All experiments for class purposes, and almost invariably all those prosecuted for original research, are conducted while the animals are in an anæsthetic condition. The anæsthetics employed are chloroform, puff ball, chloral, and croton chloral. Frogs are invariably pithed prior to experiment.	Because in some instances it might be necessary to discover whether the result had been rendered nugatory or fallacious by the anæsthetic.
St. George's - -	On rare occasions frogs have been experimented upon for scientific but not for teaching purposes.	Five or six.	These experiments were all performed for private research on the part of one of the surgeons, and had no relation to the business of the school.	Frogs only.	These frogs were previously injected with curare, which recent experiments tend to prove has an anæsthetic action.	—
London Hospital and Medical College.	Yes.	Two or three.	To demonstrate circulation in frogs for class demonstration about once or twice a year. To obtain blood for examination the tails of	Frogs, newts, and human fingers, perhaps twice a year.	Frogs are curarized; newts and human beings not anæsthetised.	Because the pain is much less than the annoyance of chloroform narcosis.

			newts are snipped off and the fingers of the lecturer or class pricked with a needle.			
MIDDLESEX - -	Yes.	None last year; about a dozen the year before.	For class demonstration.	Perhaps half a dozen frogs (pithed) and the rest rabbits.	Always in the state of anæsthesia. The anæsthetics employed being chloroform, ether, or chloral hydrate. In the case of frogs the brain is destroyed before any experiment is performed.	—
UNIVERSITY COLLEGE - -	Yes, in the physiological laboratory.	If frogs are included, it is impossible to make this statement with accuracy.	For both purposes. It is possible to state with approximate accuracy, that during the year ending December 31, 1874, about seven animals were used for demonstration, and about 12 for research. In the previous year about the same number of animals was used for demonstration, and a somewhat larger number for research. In both cases frogs are not included.	Probably somewhere about 100 frogs, but it is impossible to state this accurately. About six dogs and six cats were used for research, and about six rabbits and one cat for demonstration. During 1873 a certain number of guinea-pigs were also used for research.	During the year 1874, all the mammalian animals employed were in a state of anæsthesia. This was also the case previously, but I am not able to make so positive a statement about it. The anæsthetics used were chloral, chloroform, and ether.	No statement can be made with reference to the anæsthesia of frogs,
KING'S COLLEGE - -	Yes.	Two to three hundred.	For demonstration, about 150; for original research, from 50 to 150.	Frogs, 150; newts, 20; tritons, 30; dityscus, 20; rabbits, 25; cats, 5; dogs, 10; guinea pigs, rarely; (as nearly as possible).	Generally in a state of anæsthesia; frogs, newts, &c. not so frequently, as some operations are painless. Chloral, chloroform, morphia.	Only in such cases as do not require it from the insignificance of the pain, or in those where the use of anæsthetics would interfere with the result of the experiment.
WESTMINSTER - -	Yes.	About 15.	All for class demonstration.	All frogs.	In the majority of cases anæsthesia was produced by woorara.	Because the anæsthesia would have vitiated the result.
CHARING CROSS - -	Yes.	No exact number can be given, no account having been kept; but it is small.	Almost entirely for class demonstration.	Frogs, newts, and water-beetles. Pigeons and rabbits have been used, but very rarely; not once a year. The number of frogs and newts cannot be exactly stated, for the reason previously given.	The frogs have been usually curarized; for the larger animals chloral has always been used. The newts and water-beetles have not been in a state of anæsthesia.	The only operation on these last has been the withdrawal of a drop of living blood or a minute portion of muscle, implying in such organisms no pain whatever.
ST. MARY'S - - -	As far as I know, no experiments are performed on live animals (including frogs) in St. Mary's Hospital, or in the schools and laboratories connected with it.					
QUEEN'S COLLEGE, BIRMINGHAM.	No return has been received.	- -	—	—	—	—
BRISTOL MEDICAL SCHOOL -	The experiments on live animals at the Bristol Medical School have been confined (of late years) to the drowning of two or three rabbits in each year for the purpose of class demonstration in medical jurisprudence, the use of anæsthetics in such cases being obviously superfluous.					

§ 8. Digest of Replies to Questions, &c.—*continued.*

Name of Medical School, &c.	1. Are any experiments performed on live animals (including frogs) for scientific or teaching purposes at —— or in the Hospitals, Schools, and Laboratories connected with it?	2. State as accurately as you can the number of such experiments performed in the course of the year.	3. State for what purposes such experiments are made, whether for original research or class demonstration, and give the relative numbers in each case.	4. State what animals (including frogs) are used, and give the numbers of each species.	5. State whether and in what proportion of cases the animals are in the state of anæsthesia, distinguish the case of frogs:—and also what anæsthetics are employed.	6. When not in that state, the reason why not.
University of Cambridge -	Such experiments are performed only at the physiological laboratory in the new museums under the direction of the Trinity Prælector of Physiology.	It is difficult to answer this question exactly. Probably 200, or rather more than 100 (excluding invertebrates), such experiments have been performed in each of the last two years.	Probably rather more than one half were for original research, and the remainder for class demonstration.	For research purposes, about 12 dogs, 3 or 4 cats, 30 or 40 rabbits, 4 or 5 tortoises, 1 snake, about 60 snails, 30 or 40 frogs and toads, about 12 fresh-water hydræ, during the past year. For class teaching, 4 or 5 dogs, 4 or 5 cats, 30 or 40 rabbits, and 20 or 30 frogs, during the past year.	In the case of dogs anæsthesia was always employed, being brought about by morphia or morphia and chloroform combined. In the case of cats and rabbits, anæsthesia was always used, being brought about by chloral. In the case of frogs used for research purposes chloroform was used for painful and short operations, and urari when the animal had to be observed for some considerable time. When frogs were used for class purposes chloroform or ether was used for short operations, and in other instances morphia with urari, and latterly urari alone.	In the case of the snails and the hydræ anæsthetics were not employed, because they would have interfered with the results of the experiments; indeed, it is more than doubtful whether such animals can be thrown into a state of anæsthesia. In all other cases, including frogs, anæsthetics (urari being included as such) were employed.
	It is presumed that by "experiments performed on live animals" it is intended to refer to experiments performed on animals in possession of their brain and spinal cord, where these form a part of their organization. Then, experiments on the muscles, nerves, heart, &c. of frogs previously deprived of their brain and spinal cord are not to be considered as experiments on living animals. This remark is made because by far the greater number of the experiments constituting part of the practical work of the class of physiology, and a great number of those carried on for the purposes of research, have been experiments performed on frogs and toads whose brains, and in many instances the spinal cords, had been destroyed. The returns here given are made on the supposition that such is the meaning attached to the words. If, on the contrary, "live animals" are intended to include animals whose brain and spinal cord have been destroyed, and therefore, logically, any living tissue or organ, the number of frogs given in this return would have to be increased to two or three hundred in each year, the increase belonging rather more to research than to class teaching. In these cases the brain and spinal cord was destroyed by the operation of "pithing," which causes almost instantaneous loss of consciousness, and may therefore be considered as almost painless.					
Leeds School of Medicine	No.	—	—	—	—	—
Liverpool Royal Infirmary School of Medicine.	Yes.	During the present year about 60. (The average of other years is less than this.)	About 50 of these vivisections were made for the purposes of original research. The remainder for class demonstration. No vivisections are ever allowed to be performed by students.	45 rabbits, 3 monkeys, 1 tortoise, about 10 frogs.	In every instance deep anæsthesia was produced; in warm-blooded animals by ether or chloroform, in the frog and tortoise by the same means, or by injecting chloral, or the spinal cord was divided in the neck.	—

OWENS COLLEGE.	Yes. Experiments on living animals are performed by the professor of physiology for the purpose of illustrating his lectures on practical physiology, and more rarely those on systematic physiology. Experiments on living animals are, moreover, frequently performed in the physiological laboratory of Owens College for the purposes of original research.	It is impossible to answer this question, even in an approximate manner, as no record of the actual number of experiments has hitherto been kept.	In so far as this question admits of being answered, it is so in the reply to 1.	The animals employed for the purpose of experiment are rabbits, frogs, rats, guinea-pigs, and (rarely) dogs and cats. No record has hitherto been kept of the number of animals used, and this naturally varies greatly, depending upon the nature of the inquiries which are being conducted. A considerable proportion of the animals used are killed for the purpose of anatomical investigation, and are not subjected to any experiment whilst alive.	In all cases of experiments performed for class demonstrations, *i.e.*, not performed for the purposes of original research, anæsthetics are used. When the body of an animal is to be used for anatomical purposes, and death can be caused rapidly, as by decapitation, no anæsthetic is employed. The anæsthetics and hypnotics used are the following: chloroform, administered by inhalation; morphia injected subcutaneously, followed by the inhalation of chloroform; chloral injected subcutaneously. Frogs are rendered insensible when required, by immersion of their bodies in a diluted watery solution of chloroform.	Anæsthetic and hypnotic agents are not made use of when their introduction into the system would vitiate or obscure the result of observations made to discover new scientific truths.
OXFORD: Anatomical Department Museum.	Yes, but none regularly which entail the sacrifice of life.	Not more than half a dozen per annum, upon frogs, for purpose and in manner below specified.	Experiments are sometimes made for purposes of original research, but not regularly nor every year. Experiments for purposes of class demonstration are made every year upon frogs.	Half a dozen frogs annually; and occasionally, but not regularly, small birds and mammals.	In all cases in which any pain can be supposed to be likely to be caused, anæsthetics are employed. For the purpose of showing the circulation in the web of frogs' feet, curare has been employed as it renders the animal motionless, and the experiment does not involve the infliction of pain. The anæsthetics employed in original research are chloroform or chloral hydrate.	In some lectures on respiration, live animals have been placed under a bell jar, with air drawn through it in a constant current, to demonstrate the effects of the process of respiration. Their situation is much that of a man in a well ventilated room, and no anæsthetics are needed or employed.
General Hospital (Radcliffe Infirmary) and Sanitary Laboratory, Medical Department, in the Museum.	No vivisection is ever practised in the General Hospital (Radcliffe Infirmary) in Oxford. Nor has vivisection ever been practised in the Sanitary Laboratory of the Medical Department in the Museum.					
Exeter College Laboratory.	Experiments on live animals (including frogs) are performed in my laboratory both for scientific investigation and for teaching purposes.	The laboratory has only been established for three years, and it would be impossible to give an estimate of what might be the number of experiments, supposing that certain questions were to occupy my attention, or that pupils capable of profiting by such experimental work were to present themselves. I should estimate the number of experiments on live animals, which *have been* performed during the past three years in the laboratory, as not more than 30 (ex-	About half of these experiments have been made for my own investigations solely; but an experiment made for exhibition to one or more pupils is not, in my opinion, at any time likely to be devoid of importance for a person engaged in research, or upon topics more or less illustrated by the experiment.	The animals used have been frogs in all the experiments above mentioned. It is perhaps only right that I should mention, inasmuch as I should include such organisms under the term "animals," that experiments have also been made from time to time on fish, on tadpoles, on worms, on crayfish, on insects, fresh-water mussels, and on various microscopic animals. I am not able to give an exact estimate of the number of these various animals used both in	I am not able to assert that in any case the animal employed has been in a state of anæsthesia. Many of the experiments have been such as to be unaccompanied by any of those movements which are by some persons considered to indicate pain. In other cases (frogs) the power of movement has been removed by injection of curari, which, it appears to be exceedingly probable, suspends those functions of the nervous system which are active in ourselves when	My reason for not employing such an anæsthetic as chloroform in the case of a frog is that it is easier to prevent movement either by curari or by wrapping in a cloth. It is desirable to prevent these movements both for the purposes of the experiment and on account of the injurious effect which in some cases they might produce in the mind of a student, who should look on at the experiment as at a gladiatorial spectacle. But considering what is known

§ 8. Digest of Replies to Questions, &c.—*continued.*

Name of Medical School, &c.	1. Are any experiments performed on live animals (including frogs) for scientific or teaching purposes at —— or in the Hospitals, Schools, and Laboratories connected with it?	2. State as accurately as you can the number of such experiments performed in the course of the year.	3. State for what purposes such experiments are made, whether for original research or class demonstration, and give the relative numbers in each case.	4. State what animals (including frogs) are used, and give the numbers of each species.	5. State whether and in what proportion of cases the animals are in the state of anæsthesia, distinguish the case of frogs:—and also what anæsthetics are employed.	6. When not in that state, the reason why not.
		clusive of some mentioned in Answer 4).		my class and by himself; but if the microscopic animals are excluded as being too numerous for computation, I should suppose that some 50 or 60 in the three years would be near the number employed.	we experience pain. In other cases the movements are the subject of the experiment.	with regard to the structure and the functions of the frog, I should say that the probability of its experiencing anything like that which we call, in the case of ourselves, pain, is so small, that I should not be anxious to apply to it those drugs which, in the case of man, remove the consciousness of pain. On the other hand, it is necessary for me to observe, that in the case of experiments with such animals as the rabbit, cat, dog, and monkey (but especially the three latter) I should in experiments not of the briefest duration, and such as would give *severe* pain to a man, employ chloroform or a similar anæsthetic on the grounds (1st) that in proportion as the structure and functions of an animal resemble those of man, there is probability that it is capable of experiencing a degree of that state of consciousness which we call pain, and (2nd) that it is undesirable for men to accustom themselves to evoke that state of consciousness when they can avoid doing so without sacrificing results which tend to the good of mankind.
Magdalen College Laboratory.	Yes, in the laboratory of Magdalen College such experiments are performed.	Exclusive of pithed frogs, from 10 to 20.	These experiments have been made for both the purposes of original research and class demonstration, but the majority for the latter purpose.	Rabbits, two dogs, a few fish, and the majority as against all the rest, frogs.	With a very few exceptions the animals were anæsthetized with chloroform; chloral, or urari.	The exceptions were a few experiments for the purposes of original research upon the influence of urari, which were instituted with the express purpose of discovering whether this drug was an anæsthetic, and which resulted in demonstrating that it possesses this power. (For details of this see "Nature," Aug. 19, 1875.)
SHEFFIELD SCHOOL OF MEDICINE.	Yes, in the School of Medicine.	Two.	Class demonstration.	One frog, one rabbit.	The rabbit in a state of anæsthesia. Not the frog. Chloroform.	In frog only; because the web is simply exposed.
UNIVERSITY OF DURHAM.	I regret that I am unable to give you the detailed information which you require, as our late Professor of Physiology Dr. Nicholson, did not perform any experiments on live animals for teaching purposes. A few experiments have been performed during the past year for scientific purposes. The animals have in all cases, frogs sometimes excepted, been in a state of anæsthesia. It is the intention of our present lecturer on physiology, Dr. Preoch, to perform experiments on live animals both for scientific and teaching purposes. As his course of lectures has only just commenced, I am unable to give you the number of such experiments. Frogs, rabbits, dogs, and cats will be used. The animals will in all cases, frogs sometimes excepted, be in a state of anæsthesia. Chloroform is the anæsthetic which will be most frequently employed.					

Royal College of Surgeons in Ireland.	Yes.	Circulation in frogs is shown three times; intestinal movements in a rabbit killed by blow on the occiput; circulation in the tail of a minnow (once).	For class demonstration only.	See reply to No. 2.	In the case of the frogs, wourara is employed.	The minnow is not put to any torture: the rabbit is quite insensible.
Trinity College, Dublin	There are no experiments performed on *living* animals in Trinity College of a kind that inflicts pain, and there are no experiments performed upon animals under the influence of anæsthetics.					
Catholic University, Dublin.	The circulation in the web of the frog's foot is annually demonstrated by Professor Cryan, for the instruction of his class, the animals (frogs) being under the influence of curara. They are subsequently killed by decapitation.	About eight.	Class demonstration; number as previously given.	Frogs only, number as previously stated.	No anæsthetic has been used.	Probable interference with the regularity and force of the circulation.
Carmichael School of Medicine, Dublin.	Yes.	Very few. Seldom more than six for purposes of demonstration; except for obtaining specimens of fresh blood for microscopic purposes.	I usually show a few fundamental experiments to the physiology class. As already stated seldom more than six, and often not so many. The number of animals used for original research varies from 0 upwards. I regret to say there are seldom any original researches being conducted, hence very few animals are ever used.	Frogs, newts, rabbits, occasionally dogs. For class demonstrations the six might be divided, two rabbits and four frogs, occasionally one dog. I cannot pretend to give any answer in the case of original research.	In nearly all. Chloral hydrate for rabbits; chloroform for dogs; frogs are generally pithed. Sometimes curara is used.	The only cases in which I have not used anæsthetics is in those where I conceive the pain to be but slight. I have never made experiments on sentient nerves which might require the absence of anæsthetics. I desire to add that all experiments are made either by myself or under my immediate supervision, and I have never known "a student" (in the ordinary restricted meaning of the term) make, or desire to make, any physiological experiment.
Petre Street Original School of Medicine, Dublin.	None at present, as the lecturers have some time ago come to the conclusion that they are quite unnecessary.	None.	None.	None.	None.	None.
Dr. Steevens's Hospital, Dublin.	Yes.	Two or three in the year.	Class demonstration.	Frogs only.	Anæsthetics always used—curara.	
Queen's College, Belfast	Yes.	But few each year for class demonstrations; occasionally larger numbers for original research.	Probably 6 to 10 frogs are used annually. Some are used alive and without injury to show the circulation of the blood in the web of the foot and in the tongue, in the classes of physiology and histology; others are decapitated in a private room, and their bodies used to show the electrical currents in muscle in the chemical class, and the textures in a fresh state in the class of histology.	For original research, rabbits, cats, dogs, mice, birds, and invertebrate animals have been used.	No anæsthetics are used.	Anæsthetics are highly dangerous to life both in men and animals, and consequently interfere with the results of physiological observations.

§ 8. Digest of Replies to Questions, &c.—*continued.*

Name of Medical School, &c.	1. Are any experiments performed on live animals (including frogs) for scientific or teaching purposes at —— or in the Hospitals, Schools, and Laboratories connected with it?	2. State as accurately a you can the number of such experiments performed in the course of the year.	3. State for what purposes such experiments are made, whether for original research or class demonstration, and give the relative number in each case.	4. State what animals (including frogs) are used, and give the numbers of each species.	5. State whether and in what proportion of cases the animals are in the state of anæsthesia, distinguish the case of frogs:—and also what anæsthetics are employed.	6. When not in that state, the reason why not.
Queen's College, Cork -	Yes.	The number depends upon the views of the lecturers for the time being of physiology, therapeutics, and medical jurisprudence, and could not therefore be accurately stated. The number is in any case very small, such experiments being resorted to only where the phenomena cannot be otherwise investigated or illustrated. Last year, for example, the total number did not exceed a dozen, including cases of poisoning.	For both purposes. Last years however, the experiments were chiefly for purposes of demonstration.	Dogs, rabbits, birds, and a few frogs. Last year the experiments made being for the purpose of determining the poisonous action of certain subsatnces, birds and rabbits were chiefly employed.	Unless in the relatively few cases where anæsthesia would interfere with the phenomena to be investigated the animals are always put into that state. The anæsthetics employed are chloroform and ethers.	The answer to this question is given under No. 5.
Queen's College, Galway	No physiological laboratory has yet been established by Government in the college. Experiments for teaching purposes are occasionally made in the classes of phy siology and medical jurisprudence.	So far as I can learn, not more than a dozen experiments involving vivisection have been made during the last year.	For class demonstration.	Frogs were the only animals used.	—	—
SCOTLAND.						
University of Edinburgh -	Experiments on live animals are performed for scientific and teaching purposes (*i.e.* for original research and for tuition) in the physiological department of the University of Edinburgh.	During the past year (the first year of office of the present professor of physiology) about 232 such experiments, for all purposes, were performed.	About 66 for the purpose of original research, and about 166 for the tuition of medical students in physiology.	A. For original research 44 dogs, about 8 rabbits, about 12 frogs, 2 cats; total 66. B. For purposes of tuition 2 dogs, 9 rabbits, about 155 frogs; total 166. P.S.—It may be stated that the professor of physiology considers the number of dogs used last year for original research to have been exceptionally high, and that of frogs exceptionally low. The animals used illustrated a course of 100 lectures to 162 medical students, and a course of practical physiology for 125 students.	A. Of the animals used for original research 4 dogs and 2 cats were anæsthetised by chloroform; 40 dogs got curara; the rabbits and frogs got nothing that could act on the sensory nerve apparatus. B. Of those used for teaching purposes all the dogs and rabbits were anæsthetised by chloroform, opium, or chloral; about 133 frogs got curara; about 23 frogs got no anæsthetic.	Animals were not anæsthetised when it was believed that an anæsthetic would render the result of the experiment equivocal.

GEONS' HALL, EDINBURGH.	formed in connexion with the class of Institutes of Medicine or Physiology taught by Dr. McKendrick.		trating the physiology of nerves and muscles, and certain points connected with the circulation.		as the animals are suddenly decapitated, and thus deprived of sensation.	
UNIVERSITY OF GLASGOW -	Yes.	Between twenty and thirty.	For both purposes. Eight or ten original experiments.	Frogs, say twelve; rabbits, six; dogs and cats, four.	All anæsthetised, except two or three. Anæsthetics:—chloroform, ether, chloral, and curari.	Two or three experiments upon sensation.
UNIVERSITY OF ABERDEEN -	No such experiments are practised here.					
VETERINARY COLLEGES.						
ROYAL VETERINARY COLLEGE	No experiments for teaching purposes are had recourse to in any department of the college. Experiments for the purpose of determining the pathology of infectious and allied diseases are occasionally adopted.	The number varies, but cannot be said to exceed 20 to 25 in a year.	For original pathological research.	Guinea-pigs and rabbits chiefly. Occasionally sheep, calves, and donkeys.	Anæsthesia is not produced in the animals which are subjected to the class of experiments named.	—
VETERINARY COLLEGE, EDINBURGH.	Yes.	About 50 or 60. The numbers vary according to the amount of original investigation done. The number of animals used for class demonstration is from year to year the same in physiological department. About 16 or 18 in pathological department.	The experiments were made chiefly for original research. Six or eight experiments are made in the course of the year for class demonstration upon frogs alone (in the physiological department). Majority of experiments made for class demonstration, two or three only being for original therapeutic research (in pathological department).	The following animals have been used on an average: 8 to 10 rabbits, 2 or 3 dogs, 2 or 3 cats, 8 to 10 white mice, 5 or 6 pigeons, and the remainder frogs and gold-fish (in the physiological department). About 10 or 12 horses, 1 cow, and 2 or 3 dogs (in the pathological department).	Nearly all the animals are in a state of anæsthesia. Some on which the actions of drugs were tried received no anæsthesia. Before class experiments on the frog it is instantaneously killed and pithed, except in the instance when the circulation of the blood is demonstrated in the foot of uninjured frogs. The anæsthetics employed have been chloroform and chloral, but latterly the hydrochlorate of chinoline has been in use, as it has distinct advantages for such animals as rabbits (in physiological department). All animals completely anæsthetised with chloroform, from the effects of which they are not allowed to recover, death being completed by opening the carotid artery. If minor operations are to be performed the ether spray is used, and animal killed immediately after (in pathological department).	In the instances where anæsthetics were not employed, it was for the reason that the use of an anæsthetic substance would interfere with the success of the experiment, as in testing the action of any drug or chemical substance on the animal, or, as above stated, in the case of the frog used for demonstrating the chief facts of muscular contractility and the action of nerve, where it was instantaneously deprived of sensibility by removal of the head and pithing.

Digest of Replies to Questions—*continued.*

Name of Medical School, &c.	1. Are any experiments performed on live animals (including frogs) for scientific or teaching purposes at —— or in the Hospitals, Schools, and Laboratories connected with it.	2. State as accurately as you can the number of such experiments performed in the course of the year.	3. State for what purposes such experiments are made, whether for original research or class demonstration, and give the relative numbers in each case.	4. State what animals (including frogs) are used, and give the numbers of each species.	5. State whether and in what proportion of cases the animals are in the state of anæsthesia, distinguish the case of frogs:—and also what anæsthetics are employed.	6. When not in that state, the reason why not.
New Veterinary College, Edinburgh.	Yes.	About half a dozen.	For class demonstration.	Frogs, horses, and donkeys.	They are always rendered unconscious by chloroform.	—
Veterinary College, Glasgow.	Yes, such experiments are performed in the physiology class at this college.	About 100 experiments altogether in the course of the year.	The great majority of the experiments made are for class demonstration; the others have been for original research or for the purpose of verifying the statements and results of other physiologists.	About 20 frogs, 2 or 3 rabbits, 3 dogs.	In all cases the animals have been anæsthetised with chloroform, or in the case of the frogs with curare, except when the experiments permitted of pithing being previously performed.	
Brown Institution - -	Yes; exclusively for the purposes of scientific research, in which I have been myself engaged.	A hundred and two during the year ending Michaelmas 1875.	All of the animals were used for experiments relating to infection, or to the action of disinfectant remedies.	Four donkeys, thirty dogs, three rabbits, sixty-five guinea-pigs.	None were in a state of anæsthesia.	The operation in every case was extremely trifling. It consisted either in opening a vessel for obtaining blood for analysis, or injecting infecting material either into the circulation, into a serous cavity, or under the skin.
	N.B.—I am informed that about half a dozen frogs were used by Dr. Klein for teaching purposes. I am unable to state the nature of his experiments.					

APPENDIX IV.

App. IV.

Documentary Evidence presented by the Secretary of the Royal Society for the Prevention of Cruelty to Animals.

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APPENDIX IV.

PREFACE.

It has been thought best to introduce this evidence by a description of apparatus required for operations written by Dr. Brunton, which corresponds with similar directions in the "Handbook for the Physiological Laboratory," edited by Dr. Burdon-Sanderson, and other handbooks.

References are added to all extracts.

If it be alleged that although the authors do not state it, the animals were nevertheless narcotised in some of the experiments, it may be said in reply that no rule exists for the production of anæsthesia in laboratories except the operator's conscience and his convenience in manipulation; and that vivisectors sometimes are of opinion that the presence of pain is preferable to the presence of a narcotic. It may also be added that when animals have to be narcotised the writers generally state such circumstance, and the omission, therefore, adds weight to the foregoing remarks. Even when used, a narcotic may be insufficient in quantity or ineffectual in quality to prevent pain, although effective to permit of easier experimentation. The object may, moreover, be to deaden the sensory nerves during the cutting process only; and in such cases the animal awakens from its enforced sleep to the terrible consciousness of torture. Curare does not destroy pain, but only the power to express it. All these points must be remembered in reading the evidence.

It is not expected that every one of the following quotations will pass without question; but it is assumed that the entire collection will present conclusive evidence that pain is very often inflicted by vivisectors, much of which is prolonged.

The extracts are made from English books, and where the experiment was performed on the Continent it will be found that an English sanction has been given to it.

Four divisions have been made, viz.:—(*a.*) Pain; (*b.*) Prolonged pain; (*c.*) Design to teach students vivisection; (*d.*) Opinions more or less against vivisection.

A.

EVIDENCE OF PAIN.

A few of the following experiments may be said to show prolonged pain, especially in those of snake bites; but to prevent the mutilation of a consecutive series of experiments they have been retained under this head.

Lectures on Experimental Investigation.

Mode of securing Animals.—In order to determine in an exact manner what organs or parts are affected, we are obliged to make use of apparatus of various kinds; and before these can be applied to an animal it must be prevented from moving. Frogs are fastened to a frog-board by a piece of cord with a noose at the end, slipped over each elbow and ankle. The frog-board may consist of a piece of mill-board about nine inches long by three inches broad, with four slits at the sides to keep the cords in position, or of a piece of wood the same size, and from a quarter to half an inch thick, with holes, through which the cords are passed. They may be fastened by simply tying them together, or by sticking a small wooden pin into each hole, or by four screws, such as are used by fastening the wires of galvanic batteries, placed in the edges of the board. The last way is, I think, the most convenient. Rabbits are best secured by Czermak's holder and board.

[Here follows drawing of Czermak's holder, with the following key:—]

(Czermak's rabbit-holder and board.—A, the board. B, a bent piece of iron forming the upper part of the board. C, an open space through which instruments can be introduced from below to divide the spinal cord. It is generally covered by an iron plate. D is an upright rod fixed by a screw into a slit in B. F, is a forked rod, which can be moved back or forward, up or down, by the nut E. The forks are hollow, so that the ends of the holder can be passed into them and fastened by the screw *j*. H, is a bar which passes behind the incisor teeth of the rabbit. G and G are two bent bars which pass under the chin and over the nose of the animal, and are brought together by the screw *l*. From the upper end of *g′* hangs a screw, which passes between two projections on *g*, and has a mother-screw K. The screw K works against the projections on *g*, and draws the ends of *g′* and *g* together. These press on the rabbit's nose and under jaw, and keep the teeth firmly locked over the rod *h*. M M are screws for fixing the cords which confine the legs. They are a remarkably convenient sort, consisting of an outer part with a horizontal hole, and an inner ring with a stalk, on which a milled screw plays when the milled head is at the top of the stalk; the inner ring and outer holes correspond, and the cord can be easily pushed through; but when the milled head is turned, the stalk and ring are drawn up and the cord nipped between it and the outer part. The cords may either be fastened directly in the screw, or passed first through one of the holes in the edge of the board. The board should be covered with a large pad of india-rubber stuffed with horse-hair, and there should be another round pillow to put under the animal's neck.)

The best cord is strong window cord. The one end should be flattened with a hammer, and turned over so as to make a small loop, whose two sides are then firmly bound together with waxed thread. Through this loop the other end is passed, and the noose thus made is ready to be drawn tight at any moment. The other end of the cord should be cut to a point, and also bound with waxed thread, to prevent the strands unravelling. The rabbit is placed on the board, the nooses slipped over the legs and drawn tight, and the ends of each cord passed through the screw which will be nearest it when the animal lies on its back. The rabbit is then turned over, and the cords drawn through the screws and fastened. The bar *h* is then put between its teeth, and the screw *l* turned till *g* and *g′* fit tightly over its muscle, and the projecting ends of *g* fixed into the ends of *f*. Dogs may be fastened by Bernard's holder,

[Here follows drawing of Bernard's holder, with the following key:—]

(A, is Bernard's dog-holder.—*a* is a metal ring, within which a bent piece of metal, *b*, is moved up and down by the screw *c*. *h* is a straight piece, which is fastened by a screw to *a*, and can be moved nearer to or farther from a corresponding piece at *b*. These two pieces lie under the lower jaw of the dog; the bent piece *b* is screwed down on its nose, and the strap *i* buckled behind its head, which is thus firmly fixed. It may be moved back or forward by sliding the rod *d* through the nut *e*, or up and down by moving *e* on *f*, which is a strong iron rod fastened to a table or board by the screw *g*.)

(B, Brunton's holder for dogs or rabbits.—A loop of cord is tied round the upper jaw, the bar *l* passed behind the canine teeth of the dog or cat or incisors of the rabbit, and the two jaws then tied together to prevent its slipping out. This mode of fastening animals has been long used, and my modification simply consists in the addition of the forked bar *k*. After *l* is fastened in the mouth, the forked ends of *k* are pushed through holes in *l*, and fastened by the screws *m*. *k* may then be fastened to an upright bar by means of a nut in the same way as Bernard's or Czermak's holder.)

or by a simple bar of iron put behind their canine teeth. A piece of cord is first tied round the upper jaw, the bar put into the mouth, and the two jaws tied firmly over it. A split strap may be used instead of the cord. I have had a bar made with a hole at each end into which a fork of steel passes and is secured by a screw. The fork may then be fastened by a nut to an upright rod, as in Czermak's holder (Fig. 2 B). Cats and guinea-pigs may be fastened by Czermak's holder. For guinea-pigs, a little padding must be placed between g and g', in order to make them catch the head. A simple bar and cord may also be used for rabbits, cats, and guinea-pigs as well as for dogs.

. . . .

Narcotising Animals.—Narcotics cannot be given in all cases to animals on which we experiment, as their action must to a certain extent complicate that of the drug which we wish to investigate. We cannot use them when we are observing what are the general symptoms which a medicine produces. But when we are investigating its action on particular organs we may often use them, not only with safety but with advantage, when they have no action on the particular organ which we are studying, or so little that its disturbing influence is more than compensated by the diminished muscular action and consequent ease in performing the experiment which narcotics produce. It is almost unnecessary to say that, in all cases which admit of it, narcotics should be used, as we have no right to inflict any unnecessary pain, although we may be justified in taking the lives of the lower animals in order to preserve the more valuable life of man, either by supplying him with food by means of those killed in the slaughterhouse or by obtaining the knowledge which shall enable us to cure disease by means of those killed in our experiments. The narcotics which we use are opium and chloral. Chloroform is inadmissible, as its administration generally seems to cause dogs more pain than the experiment itself, and rabbits are very easily killed by it. . . . . . .

When we wish to render the animal absolutely motionless, or to observe what effect any drug will produce after the motor nerves have been paralysed, we give curare. .

. . . .

Curare may be obtained from Messrs. Hopkin and Williams, New Cavendish Street, London, or from Bruckner and Lampe.—*Dr. Brunton, British Medical Journal,* No. 542, pp. 321 and 322.

An experiment, illustrated by a drawing, shows a living frog strapped down to a board, its sciatic nerve dissected out, bared, and brought out of its body from its thigh to its loins, and attached to a galvanometer, after which strychnia is introduced under its skin, and a result from the action of the tortured nerve is shown on the needle of the instrument.—*Dr. Radcliffe, Lancet,* No. 2,061, p. 227.

If the sciatic nerve of a rabbit be divided in the ham, and the end which is in connection with the brain be dissected out and laid across the poles of a galvanic cell, the animal screams with pain and strains with convulsive movements when the circuit is closed or opened. Before the time when the portion of the nerve which is included in the circuit is paralysed by the current, the screams and convulsions happen equally at the closing and opening of the circuit, and it is immaterial whether the positive or the negative pole be in the position next the brain. After the time when the portion of nerve which is included in the circuit is paralysed by the current the screams and convulsions are present at the closing of the circuit and absent at the opening when the negative pole is in position next to the brain, and absent at the closing of the circuit and present at the opening when the positive pole is in the position next the brain. Pain and convulsion, that is to say, come together and go together. In a word, there is reason to believe that the electrical changes which a sentient nerve experiences in the production of sensation are the exact equivalents of the electrical changes which a motor nerve experiences in the production of muscular contraction.—*Dr. Radcliffe, Lancet,* No. 2,067, p. 409.

Through a fistulous opening into the stomach of a dog Bernard introduced, whilst digestion was going on, the hind legs of a living frog. The legs were dissolved away, the animal continuing all the while alive, and living for some time even after the experiment was completed. I have repeated this experiment myself and obtained a similar result. It proves unquestionably that the stomach has the power of dissolving living substances. .

. . . .

I performed an experiment substituting the ear of a rabbit for the hind legs of a frog. Whilst my dog with a fistulous opening in its stomach was at a period of full digestion, I carefully introduced through the cannula the ear of a vigorous rabbit and held it in position with the hands so as to avoid mechanically injuring it or producing congestion by obstructing the flow of blood through its vessels. At the end of two hours the ear was withdrawn and several spots of erosion, some as large as a sixpenny-piece, were observed on its surface, but nowhere was it eaten completely through. On being replaced for another two hours and a half, the tip, to the extent of about a half or three quarters of an inch, was almost completely removed, a small remnant of it only being left attached by a narrow shred to the remainder of the ear. The gastric juice seemed to act like a strongly corrosive material, making first a number of ulcerated-looking spots through the skin and afterwards extending its action more rapidly through the central parts of the ear. A rather profuse hæmorrhage took place, especially towards the latter part of the experiment. My own fingers became moistened with gastric juice that escaped by the side of the ear, and afterwards felt sore or tender as if the skin had been slightly acted on.—*Dr. Pavy, Lancet,* No. 2,070, p. 492.

The Parisian Correspondent describes his attendance at a meeting of the Academy of Sciences, and says:—These phenomena occur even when a drop of ammonia is inserted into the eye of an animal, and whilst the organ is strongly closed during the paroxysm of pain which ensues.—*Lancet,* No. 2,037, p. 298.

Experiments were made on small animals. The spinal cord was artificially inflamed by having a thread passed through it. The appearances found were contraction of the axis-cylinders, subsequent fissiparious division of the contracted portions, and the formation of pus from these. The nerve cells were affected with granular or "œdematous" degeneration. The neuroglia was somewhat increased, but not to a great extent. The perivascular lymphatic spaces of the surrounding pia mater were filled with lymph-corpuscles. One or two cases were quoted in confirmation of the facts.—*The Doctor,* October 1st, 1875, p. 195.

Mr. Bert has been instituting a series of experiments in illustration of "the phenomena and causes of the death of fresh-water animals when plunged into sea-water."

. . . .

A frog, when immersed in sea-water, is much agitated, and exhibits signs of pain, unless he can keep his muzzle above the surface. When all signs of sensibility have disappeared, the nerves and muscles are still found to be excitable, and the heart, filled with dark blood, is still seen to beat spontaneously. . . . . .

. . . .

The animal is found to have lost from one-fifth to one-third of its weight, the loss being chiefly borne by the muscles, which present a continuous or durable contraction like a kind of cramp.

Frog lived one hour in sea-water.—*Edinburgh Medical Journal,* 1871–72, p. 473.

By a new method of operation, a description of which is given in the original, they were enabled to remove the first thoracic ganglion without injury to the pleura, and in consequence, never observed pleuritis, as is common in the other methods of operation, and as said by some to depend upon the extirpation of the said ganglion.

On the contrary neither increase of the temperature of the ear, nor of the fore foot, or the opposite side operated on were missed, also the oculo pupillary phenomena will be the vascularisation of the conjunctiva as occurs after section of the sympathetic in the neck. Rotatory movements were absent.—*Journal of Anatomy and Physiology,* November 1874, p. 213.

For the production of coughing the author employed mechanical stimulants, feathers, pinching, teasing, squeezing with forceps, chemical irritants (common salt and ammonia), thermal (ice) and electrical stimuli were employed. [For these experiments on cats and dogs, it is said, the animals "in no case" were narcotised.]—*Journal of Anatomy and Physiology,* November 1864, p. 218.

Blindness of the opposite eye and paralytic dilatation of the corresponding pupil can be produced, while stimulation of the same spot is followed by strong and continued contraction of the pupil.—*Journal of Anatomy and Physiology,* February 1875, p. 210.

App. IV.

Having found in a large number of experiments on the effects of various agents on the temperature of the body and the generation of heat where the effects of muscular exertion had been set aside by the injection of woorara there was a constant and nearly equable fall of temperature beginning from the period of narcosis after antecedent convulsive movements, he determined to follow out by careful experiment the action of the woorara alone. . . . .

Another series of experiments were made on animals (principally dogs) in which fever was excited by the injection of pus into the veins, and it was found that even in this state the temperature was lowered by the action of woorara to the normal average or even below it.—*Lancet*, No. 2,510, p. 516.

Nothnagel states that he found the method of injecting chromic acid which he originally applied in order to determine the function of the convolutional centres inappropriate for experiments on these deeper seated centres; and he has employed instead a little cannula and trocar, from the extremity of which by pressure on a lever in the handle, small diverging arms could be made to protrude, and by rotation the thalamus could readily be entirely broken down. The powers of motion and sensation in the animal were then carefully tested, whilst an examination after death revealed the precise nature of the injury inflicted. The trocar was always introduced at the side of the head, and passed through the cornu ammonis and a part of the hemispheres.

. . . . .

He states as his final results that destruction of the optic thalami causes neither motor paralysis nor cutaneous anæsthesia, but that his experiments support the view of Meynert, namely, that motor processes take place in the optic thalami which are caused or excited by sensory impressions.—*Lancet*, No. 2,682, p. 135.

His experiments were conducted on dogs and rabbits fully under the influence of curare. A bent glass tube was inserted into the ductus communis choledochus in a large curarised rabbit, in which artificial respiration was maintained. He obtained at the commencement of an experiment lasting about half an hour one drop of bile in every seventeen seconds . . . .

Compression of the vena portæ, or of the hepatic artery, or of both together, caused an immediate diminution of the quantity of the secretion; but after some time, though the compression was continued, the liver recovered its activity to some extent. A rabbit which yielded one drop of bile in about twenty-four seconds before compression only yielded one drop of bile per seventy seconds after; and a dog previously giving one drop in thirty seconds only gave one per eighty or ninety seconds after compression. Ligature of the thoracic aorta just above the diaphragm reduced the secretion in a dog from one drop in seven seconds to one in fifty. Ligature below the cœliac axis raised the rapidity of the secretion from one drop in eight seconds to one drop in three seconds in a rabbit.—*Lancet*, No. 2,602, p. 56.

In the section on the Formation of Sugar in the Liver, Mr. Dalton gives the results of his experiments performed in 1869. He then found that if a portion of the liver of a dog was excised whilst the animal is living and immediately cut into thin pieces and thrown into boiling water. . . . —*Lancet*, No. 2,532, p. 324.

This view has been supported by two experiments intended to show—(1.) that bile pigment can be produced artificially from the bile acids by the action of concentrated sulphuric acid; and (2.) that colourless biliary acids, when injected into the veins of dogs, are converted in the blood of these animals into bile pigment. These experimental results are still the subject of much discussion. They have been controverted by Kuhne, Hoppe, G. Harley, &c., but confirmed by Stædeler, Neukaunn, Folwarczny, Röhrig, &c. —*Dr. Murchison, in Lancet*, No. 2,642, p. 537.

The animals were the dog and the frog; and the mode of experimentation was that the mean rapidity of the blood in the carotid and crural arteries and in the crural vein was ascertained; the sciatic or crural nerve was then dissected out and divided where it leaves the pelvis, yet so that both extremities could be easily subjected to the action of electric currents. The animals experimented on were either narcotised by the injection of morphia, or poisoned with woorara till the voluntary muscles were completely paralysed, artificial respiration being maintained in the latter case. It was not forgotten that, under the full toxic influence of woorara, paralysis of the vaso-motor nerves takes place.—*Lancet*, No. 2,528, p. 192. [Observe, curare is not an anæsthetic.]

By the investigations of Bernard and Brown-Séquard it has been established that the contractile elements of the blood-vessels are presided over by motor nerves. These are in consequence named vaso-motor nerves. . . . . . .

The largest vaso-motor nerve in the body is the great splanchnic nerve. It presides over the vessels of most of the abdominal viscera. It is sometimes necessary in physiological experiments to paralyse the blood-vessels of the body generally. This is readily done by dividing the cervical portion of the spinal cord at some little distance below the medulla oblongata. If, after the section, we desire to stimulate the vaso-motor nerves generally, we require to place two electrodes in the cervical portion of the cord, one on a level with, say, the third, the other on a level with the fifth cervical vertebra. The contraction of vessels produced in this way may be readily seen in the frog. I have injected $\frac{1}{2000}$ gr. of curare under the skin to paralyse the voluntary motor nerves, and so get rid of spasm of the voluntary muscles when we stimulate the spinal cord. I stretch the web, tie threads round two neighbouring toes, and fix them in slits on either side of a triangular window in a piece of cardboard. The back is uppermost. Two common sewing needles fixed in a piece of gutta-percha are pushed between the scapulæ into the spinal cord.

. . . .

In performing this experiment it is necessary to beware of giving too large a dose of curare. A powerful dose paralyses not only the voluntary motor but also the vaso-motor nerves. Do not give more than $\frac{1}{2000}$ gr.—*Dr. Rutherford, Lancet*, No. 2,525, p. 69.

If intense myosis be effected by the instillation of calabar bean into one eye irritation of the central extremity of the divided sympathetic in the neck produces no effect, for the sympathetic is paralysed by the bean; but if a current of electricity be passed across the eye, the electrodes being placed on opposite sides of the cornea, immediate dilatation can be induced, the muscular fibres being then directly called into action. A similar proof of the existence of a dilatator was formerly furnished by Hirschmann after the pupil had been made to contract with nicotine.—*Lancet*, No. 2,421, p. 127.

Dr. B. W. Richardson read a paper "On some effects of extreme cold on nervous action." This was a continuation of his experiments in relation to the freezing the centres of the nervous system. He explained that frozen frogs do not respire during insensibility.

. . . .

Freezing the brain necessarily produced a gradual slowness of the circulation, and even entire cessation of the heart's action.

. . . .

Lastly, he referred to the effects of freezing and rapidly thawing the skin of certain regions of the body, especially in birds, which in them caused extreme irregularity of movement and other signs of nervous disturbance.—*Lancet*, No. 2,348, p. 292.

M. Cohnheim's first experiments were performed on the corneæ of frogs. Inflammation was excited by touching them in the centre with a point of nitrate of silver, the contact being maintained sufficiently long to destroy the epithelium completely and to affect the corneal tissue.

. . . .

On repeating these experiments on the rabbit similar results were obtained. . . . .

His next experiments were made on vascular parts, and he chose the mesentery of frogs poisoned with woorara. Sufficient inflammation was excited by merely exposing the membrane to the air, which produced first hyperæmia, and after the lapse of twenty-four hours the exudation of a thin dull-grey gummy layer or fibrinous pseudo-membrane.—*Lancet*, No. 2,329, p. 506.

Threads are passed behind the common innominate and left subclavian arteries of a rabbit and arrangements made by which these threads may be tied and untied in a moment. On tying the ligatures the animal was violently convulsed, on untying the ligatures about sixty seconds later the convulsions, which were then raging at their height, were instantly brought to an end . . . .

In this experiment the subclavian arteries of a rabbit are tied at their origin and a ligature is also placed around the arch of the aorta a little beyond the opening of the left subclavian artery. The ligatures, that is to say, are so placed as to bring about a result which is the very opposite of that which was secured in the last experiment. In that case the blood was cut off from the head and neck and the circulation confined to the trunk and limbs; in this case the blood is cut off from the body and limbs, and the circulation confined to the head. In this case, that is to say, the vessels of the brain medulla oblongata, upper part of the spinal cord, and the cervical ganglia of the sympathetic nerve, receive more than their proper share of blood, for all the blood of the body is diverted in this direction, while the rest of the spinal cord and the thoracic and abdominal ganglia of the sympathetic nerve receive no blood at all. And what is the result? The result is paralysis of the parts behind the ligatures without convulsion. In one or two instances the paralysis was preceded by trembling, but in no instance was there convulsion or anything approaching to convulsion. It was ascertained also that this absence of convulsion was not due to paralysis of the spinal cord from want of blood, for on compressing the carotids so as to prevent the flow of blood through these vessels the animal was instantly seized with violent convulsion.—*Dr. Radcliffe, Lancet*, No. 1,908, *p.* 288.

Solution of an alkaline sitnate (from one to ten per cent.) were injected under the skin of the back or thighs of frogs. One of the sciatic nerves was then cut through; and tetanus was induced by means of strychnia. After this had lasted from one to six hours, the thighs were separately broken up into small pieces and rubbed up with water. *Dr. Gscheidlen, British Medical Journal*, No. 682, p. 118.

Dr. Lauder Brunton has made some very interesting experiments as to the action of chloral. He finds that, after large doses, the temperature falls till it can no longer be measured by ordinary clinical thermometers. Having found out what dose would kill an animal when exposed to the air, he gave this quantity to two similar animals, wrapping one up in cotton wool. The one so wrapped up survived, while the other died. Then he found out the dose which was lethal after wrapping up in wool, and gave that to two similar animals, wrapping one up in wool, and putting the other in a warm chamber. The one in wool died, the other recovered. A larger dose still was fatal to the one in the warm chamber. These experiments throw much light upon the action of chloral hydrate.—*Dr. Fothergill, British Medical Journal*, No. 680, p. 46.

A young man who wore red socks, having been attacked by a very acute and painful vesicular eruption on both feet, M. Tardieu attributed his affection to the red colour of the coralline dye. The substance in question having been separated by M. Roussin, the chemist, and injected under the skin of a dog, a rabbit, and a frog, which died from its effects, it was concluded to be a violent poison, and subsequently fell out of use as a dyeing agent. Contradictions of this statement, however, were soon forthcoming. M. Laudrin, a veterinary surgeon, asserted that he had administered coralline to dogs and cats without observing any subsequent ill effect. He had had positive proof of the absorption of the coralline and of its purity, since he had been able to collect it in the lungs of the animals, and to dye silk with it. Dr. Guyot confirmed these experiments, and came to the conclusion that coralline was not poisonous, even in large doses, and that it may be safely used in dyeing, provided that it be not mixed with poisonous substances.—*British Medical Journal*, No. 719, pp. 467 and 468.

Hieberg finds that when the epithelium is scraped off the surface of the cornea in the frog, fowl, or rat, it is reproduced only at the edge of the denuded part and never from any isolated centre on its surface.—*Journal of Anatomy and Physiology*, 1871–72, p. 247.

Bernard investigates the action of temperature by placing birds and rabbits in cages heated by a lamp or surrounded by a double metallic case containing a hot solution of sulphate of soda.

When the animals are thus exposed to a dry heat of 150° F. the respirations become quick and tumultuous, the temperature of the body rises, the heart beats quickly, and after a little time stops suddenly if the temperature be high enough. The necessary temperature is sooner reached in birds than in rabbits. It rose in birds to 122°, in rabbits to 115° F., Rigor mortis came on quickly, and both arteries and veins contained black blood.—*Journal of Anatomy and Physiology*, 1871–72, p. 236.

A few days ago I had the pleasure of assisting at several experiments of the highest interest, which Dr. Brown Séquard conducted at his laboratory of the Ecole Pratique.

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The experiments which M. Bernard has performed on the circulation of the blood are of great interest. Amongst other facts and inferences the following may be briefly related. Circulation is accelerated or slackened according as such and such a nerve is operated upon. In one of the experiments the following facts were observed. A dog was submitted to the action of woorara. When quite motionless, respiration and circulation continuing, the extremity of one ear was cut off, and the quickness with which the drops of blood fell one after another served to note the speed of circulation. A glass tube was then introduced into the submaxillary gland so as to gather the secretion. The great sympathetic was then set bare in the neck, as also the sciatic nerve in the posterior region of the thigh. The cervical string of the sympathetic was then cut, and then the blood was seen to flow much more rapidly, the drops quickly succeeding each other. At the same time a flow of saliva from the submaxillary gland into the tube was observed. Just then, however, on galvanising the upper end of the great sympathetic, the blood diminished and finally ceased running, through the contraction of the small arteries of the ear, the contraction sometimes going so far as completely to obliterate the arteries. These phenomena changed as galvanisation of the sympathetic was suspended.—*Lancet*, No. 2,550, page 63.

Their experiments were performed upon for the most part 20 curarised animals.

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At the commencement they found not unfrequently that a stimulus which in one animal excited the most violent uterine contractions, in another was apparently wholly inoperative, and this not only in animals of different genera, but in those of the same species. In rabbits, for instance, it sometimes happened that the slightest mechanical stimulus called forth active contractions, whilst in another animal no response occurred to the strongest electrical currents. They find that the animals best adapted to exhibit the movements are young but sexually mature rabbits which have not yet been impregnated. In these the uterus, when the abdominal cavity is laid open, appears as a flat, band-like, pink-coloured organ, at perfect rest, and but rarely exhibiting spontaneous movements. Their first experiments were made to determine the effect of arrest of the respiration. Krause, Mayer, and Basch have all shown that asphyxia induces movements of the intestines; but it has not hitherto been shown that a similar effect is produced upon the uterus. Oser and Schlesinger's experiments, however, show that in from ten to thirty seconds after suspension of the respiration contractions are perceptible, commencing from the tubæ and cervix, which in a few seconds more extend over the whole uterus. This organ becomes pale, cylindrical, and rigid, and moves downwards and towards the middle line; the cornua raise themselves in an arched manner, and, intercoiling, almost form a ball. The contractions increase in vigour with continued arrest of the respiration for some minutes. In a second set of experiments the effects of compression of the aorta were investigated, and it was ascertained that general contractions of the uterus occurred in a period varying from eighty to one hundred and twenty seconds after.

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Küssmaul and Tenner long ago described a somewhat similar experiment, but gave a different explanation of the phenomena observed, In a pregnant rabbit they exposed and compressed the carotids and vertebrals. Extrusion of the fœtuses occurred, but it was accompanied by convulsions, and they attributed the delivery to these rather than to any contractions of the uterus. M.M. Oser and Schlesinger further found that, after section of the spinal cord in the cervical region, the contractions of the uterus no longer occurred more rapidly after arrest of the respiration than after compression of the aorta, in each case supervening in about the same space of time, that is to say, in about one hundred seconds. After section of the cord, neither general loss of blood nor arrest of the flow of arterial blood to the brain produced any perceptible contractions.

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The authors of the paper do not attempt to fix the situation of the centre for the uterine movements, but they point out that their experiments prove that it lies at a higher point than that at which they divided the spinal cord, namely, between the occiput and atlas; and they think it is probable that it occupies some part of the medulla oblongata. At all events, it is unlikely that it is situated in the spinal cord, since the experiments of Küssmaul and Tenner and Schiffer show very clearly that the circulation of dyspnœic blood (a term that implies no theory as to whether the phenomena produced are due to the absence of oxygen or the presence of carbonic acid) through the cord is not followed by any symptoms of irritation, as by convulsions, but by rapid paralysis of the posterior extremities, the paralysis appearing in the course of a minute or a minute and a half.—*Lancet*, No. 2,551, p. 87.

Professor Cyon gives the following results of experiments made upon the above point. Dogs and rabbits were used, some under the influence of curare during the experiments, and some not. 1. The uterine plexus is the most important, if not the only motor nerve which can produce effectual movements of the uterus by the irritation of its peripheral ends. Irritation of the central ends only gave rise to severe vomiting. 2. Irritation of the central ends of the first two sacral nerves produces in a reflex way powerful uterine movements which cease after the uterine plexus has been cut through. Irritation of the peripheral nerves gives rise to powerful contractions of the bladder and rectum. 3. Irritation of the brachial, crural, median, sciatic nerves, etc., give rise to no peristaltic movements of the uterus, but only causes a slight rigidity and paleness. 4. The effect of the irritation of these nerves disappears if the aorta has been previously compressed, but irritation of the central ends of the sacral nerves still causes, even after the closure of the aorta, peristaltic movements of the uterus. 5. Suffocation through continued interruption of respiration causes powerful peristaltic movements, probably through direct excitation of the involuntary muscular fibres by the accumulation of carbonic acid gas.—*British Medical Journal*, No. 717, p. 405.

These reasonings have been confirmed by certain experiments of M. Bernard, who found that, when an incision is made into a lobe of the liver in a living animal, the blood may be seen to jet from the mouths of the hepatic veins during the movements of expiration, but to return sucking in air with it at each deep inspiration, so that the animal soon dies from the passage of air into the heart.—*Dr. Charles Murchison, British Medical Journal*, No. 696, p. 567.

Dr. Hertzmann says that by continuous administration of lactic acid to dogs and cats, rickets, firstly, and then osteo-malakia may be caused, whilst in rabbits and guinea-pigs osteo-malakia may be caused without rickets occurring. Lactic acid, he says, causes rickets as long as the animal is young, and osteo-malakia when it is older.—*The Doctor*, May 1st 1874, p. 81.

He cut down upon the splanchnics in dogs and rabbits from behind, and divided them without opening the peritoneum. After section of both great splanchnics the blood pressure fell greatly, while the rapidity of the pulse increased.

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Strange to say, in some animals which survived the operation, when the blood pressure was again observed after some days interval, it was found to have attained a degree as high as it had been before division of the nerves.

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On irritating the peripheral end of the divided splanchnicus major, or the central ends of any of its roots, the blood pressure always rose, and with few exceptions slowing of the pulse resulted. The latter was much less marked when the vagi had been previously divided.—*Journal of Anatomy and Physiology*, 1869, p. 211.

He has found that after division of intestinal nerves in dogs a large secretion of watery fluid into the intestines results. In a large healthy dog, which had fasted for twenty-four hours, he exposed a knuckle of bowel, and put four ligatures round it; these were separated from each other, so that the three portions of intestine, each about fifteen centimetres in length, were included within the ligatures. He carefully isolated and divided the nerves supplying the middle ligatured portion, taking care to avoid injury to the vessels. He closed the wound in the abdominal wall, and allowed the animal to remain at rest.

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In one dog a hundred grammes of fluid were found in the intestine three hours after the operation; and in another dog, killed eighteen hours after, two hundred and twenty-five grammes were present.—*Journal of Anatomy and Physiology*, 1869, p. 214.

Mr. Joseph Michon records an experiment which produced results different from those heretofore recorded. He removed the superior cervical ganglion of the sympathetic in the common cock.—*Lancet*, No. 2,218, p. 238.

In the last number of the American Journal of Medical Sciences we find one more added to the many theories that have been advanced to explain the uses of the cerebellum. Dr. Mitchell, an able experimenter, who states he has been studying the subject for six years, ... obtains the following results in pigeons, on which he had practised both the ablation of the organ, and the application of intense cold by means of Richardson's spray apparatus. After ablation he found, in accordance with the observations of other experimenters, that if the wound were deep convulsions invariably occurred, together with a tendency to backward movements, and that immediately succeeding, or even accompanying these, was an indescribable confusion of movement, the animal staggering, &c., and exhibiting what is commonly described as an apparent want of co-ordination.—*Lancet*, 2,393, p. 57.

At a recent sitting of the Parisian Academy of Medicine, Mr. Colin sought to demonstrate with the aid of anatomical preparations, that in man and the higher animals the auricular systole precedes the ventricular in the action of the heart, just as it occurs in the lower animals. He asserted that conviction of this fact may be obtained by taking certain precautions in experimenting on the higher animals. If the animal be carefully bled to avoid disturbance of the cardiac circulation, and if artificial respiration be properly kept up, it will be clearly seen that the contraction of the heart begins at the auricles. The speaker undertook to perform and show the experiment whenever called upon to do so. [A readiness to repeat "whenever called upon" shows a bad tendency of experimentation.]—*London Medical Record*, 1874, p. 306.

Let us first take under review the action of ozone on living animals. If a warm blooded animal be placed in a glass chamber, and be subjected to a stream of ozonised air, the oxygen of that air having been ozonised to the twelfth part and the influence of carbonic acid being entirely excluded, special physiological phenomena are quickly displayed. The first sign or symptom is an irritability of the mucous surface of the nostrils and of the conjunctivæ; there is often free secretion of saliva, and even profuse sweating in those animals that exhibit sweating; there is also thirst, and dryness of the tongue and nostrils. These symptoms are succeeded by great rapidity of respiration, and soon by violent action of the heart. When the chest is auscultated in this stage there is always dry bronchial breathing, and a whistling sound, as in the first or preliminary stage of acute bronchitis in the human subject. The effect of the ozone being sustained, cough manifests itself, followed by secretion of frothy fluid from the bronchial surface; this is equivalent to the congestive stage of bronchitis. Finally, there is lividity of the skin of the nose, of the nostrils, and of the lips, great coldness of the surface, gasping respiration, jactitation, and death, the death being often sudden. This may be said to resemble most perfectly the exudative stage of bronchitis. This order of symptoms, or phenomena as they perhaps had better be called, has been recognised by all experimentalists; it has been pointed out with particular care by Dr. Richardson, and my own experiments have been attended with a corresponding result. There is, however, a remarkable difference in the periods of the phenomena noticed in different animals, their order, nevertheless, being maintained in each case in which they are manifested. Guinea-pigs are peculiarly susceptible of the influence of ozone; these animals die in an atmosphere saturated with ozone usually in about an hour and a quarter, presenting with great exactitude the order of symptoms I have described above. Rabbits live longer than guinea-pigs, exhaling water from the lungs much more freely, and also micturating with greater freedom. Rats die very rapidly. Mice exhibit a greater tolerance. Pigeons resist the effects of ozone much longer than guinea-pigs, and may readily be taken out of the chamber in which they have been confined with guinea-pigs, apparently but little inconvenienced, at the period when the guinea-pigs are dead. If when just removed the chest is auscultated, the breathing, however, is found to be particularly sharp, dry, and cooing, the action of the heart being amazingly rapid, reaching even 240 beats in the minute, and the respiration being from 90 to 100 in the same time. Pigeons removed altogether from the ozone, when in this condition will frequently exhibit the third stage of obstruction to respiration and die. Frogs seem to have the power of resisting the influence of ozone permanently, if they are allowed free access to water.—*Dr. Day, Lancet*, No. 2,317, pp. 124, 125.

In order to prove the truth of his theory, he immersed two frogs in solutions of nicotine, which caused complete prostration or paralysis of motion, and the animals died paralysed in twenty-three minutes. Others he placed in solutions of strychnia, which produced tetanic effects of that drug, and caused death in four minutes. He then placed two others in a compound solution of the two poisons; the first frog lived in the compound solution forty-seven minutes, at the end of which time it died; the second frog was kept in the solution ten minutes, and then taken out, but after its removal from the liquid, in thirty minutes it was seized with the tetanic spasms of strychnia in the form of emprosthotonos, but ultimately recovered.—*Dr. Part, Lancet, No.* 1,961, p. 310.

"An experimental inquiry into the mode of death produced by aconite," by Dr. E. R. Harvey, was next read. The author's experiments, which were performed on dogs, rabbits, and frogs, went to prove that aconite acts first on the nerves and then on the muscles, killing by its action on the heart.—*Dr. Michael Foster, Lancet, No.* 1,923, p. 20.

Frogs introduced into a weak solution containing aniline died in periods varying from a quarter of an hour to two hours and a half, and death was also caused by the introduction of aniline into the mouth or into a wound in the back. Rabbits were also poisoned by this substance, a small animal being killed by fifty drops in six hours and a quarter, and a larger one by a hundred drops in four hours. In all the animals experimented upon violent clonic and tonic spasms ensued after the application of the aniline, and continued almost uninterruptedly till death.—*Lancet*, No. 1,994, p. 470.

Busch finds that sensibility is so much impaired in frogs poisoned by strychnia that pinching the animal's toes, or burning the central end of the divided sciatic nerve, may be performed without being followed by a reflex action.—*Journal of Anatomy and Physiology*, 1873, p. 399.

The four experiments now to be described are, as regards their results, merely confirmatory of those recorded by other investigators; but in their mode of performance some variations have been made from the methods usually followed, so as to show as clearly as possible the total inability of strychnia to act through the nerves. The experiments were performed on frogs, animals which possess the two-fold advantage of being very sensitive to the action of strychnia, and of surviving for a considerable time arrestment of the circulation.

Experiment 1.—A ligature was placed round the heart of a frog at the auricles. A solution containing the one-fourth part of a grain of strychnia was then injected into the stomach by means of a tube passed down the œsophagus. Thirty-five minutes after the injection the animal was able to leap about vigorously; fifty minutes after the injection it was still able to move its limbs although feebly; in five minutes more all movements had ceased.

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Experiment 4.—A ligature was placed round the heart of a frog at the auricles. One and a half grains of extract of nux vomica, mixed with fifteen minims of water, were then injected into the stomach by means of a tube passed down the œsophagus. Half an hour afterwards the animal was leaping about quite unaffected by the poison. The cranium was now cut through immediately behind the eyes with a pair of scissors, and a piece of extract of nux vomica was applied to the exposed nervous matter.

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Experiment 5.—The apex of the ventricle of a frog's heart was cut away. An opening was then made in the anterior abdominal vein, and a strong solution of extract of nux vomica was injected into the vein in an upward direction, that is, towards the liver and heart. . . .
. . Twenty-six minutes after the injection the animal was able to leap about; forty-three minutes after the injection it was still able to move its limbs. In a few minutes more all movements ceased.

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Experiment 6.—The greater part of the ventricle of a frog's heart was cut away. After allowing the blood to be expelled to as great an extent as possible, the remains of the heart were excised. The articulation of the lower jaw on each side was then cut through in order to facilitate the next part of the operation, which consisted in cutting through the cranium with a pair of scissors immediately behind the eyes. To the part of the brain thus exposed a piece of extract of nux vomica was applied. In about three minutes afterwards the action of the poison began to manifest itself. The muscles of the throat were first affected with spasm. The anterior extremities then became violently tetanised and directed upwards on each side of the head.—*Dr. Spence, Edinburgh Medical Journal*, July 1866, p. 44.

The following inquiry formed part of a graduation thesis given in by me to the University of Edinburgh on the 5th July 1873, entitled, "Erythroxylon Coca, with an "experimental inquiry into the physiological actions of "cocaine, theine, caffeine, &c., &c." The experimental research was conducted in the Physiological Laboratory of the University, and I have gratefully to acknowledge much kind assistance from Dr. M'Kendrick.

Upwards of one hundred experiments were conducted on different living animals, chiefly frogs, mice, rabbits, and cats. . . . .

The $\frac{1}{16}$ gr. of theine dissolved in 20 minims of water was injected under the skin over the back of a healthy middle-sized frog; almost immediately afterwards the respirations, which normally had been 80, were increased to 120 per minute. Seven minutes afterwards the respiration had diminished to 80 per minute.

The frog was now distinctly sluggish in its movements. It made attempts to leap, but did so feebly. When placed on its back it recovered its normal position with difficulty. When its toes were pinched with a pair of forceps it drew up its leg. . . . . . .
Two minutes after these symptoms were increased, and in three minutes more the limbs were very weak, and the animal lay on its belly without their support. When placed on its back the frog was unable to recover its position, but lay there with its limbs drawn up, and when the skin or toes were pinched the limbs were moved but sluggishly. The respirations had diminished to 40 per minute. Five minutes later the frog still lay motionless on its back with its limbs extended. All four legs were completely paralysed, and they remained in whatever position they were placed . . . .
The frog remained in this prostrate condition for eleven minutes, when slight spasmodic movements were observed in the limbs. Four minutes later it made feeble attempts to move its legs, and when its toe was pinched it drew them up. Four minutes afterwards, the animal gave a very feeble leap, and tried to crawl along the table . .
Eighteen minutes afterwards the frog jumped readily, especially if it was irritated. It croaked vigorously when touched, and in half an hour it was apparently in its natural state, with the exception of looking feeble . .
If smaller doses be given similar symptoms ensue, but they are less violent and less rapid in proportion to the amount.

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The $\frac{1}{12}$ gr. of theine, dissolved in 10 minims of water, was injected under the skin over the back of a white mouse weighing 3 drachms. For fifteen minutes no effects were observed. When its tail was pinched, it uttered a cry, which it did before the drug was administered. . .
It could now only crawl along the table, but could not run.
. . Five minutes later the animal lay with its limbs occasionally kicking. . . . . .
The under surface of the body, the feet, legs, and mouth were much congested. [Duration of experiment about 30 minutes.] . . . . In many instances the animal has tetanic spasms and opisthotonos.

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The left femoral artery of a healthy middle-sized frog was tied, and $\frac{1}{12}$ gr. of theine dissolved in 35 minims of water was injected under the skin over the back. In ten minutes the animal was almost prostrate. It lay on its back, but was still able to contract its limbs when they were pinched, which both did with equal strength, the left leg being perhaps somewhat more sluggish than the right. Fifteen minutes later the frog was apparently dead. The head was amputated; on irritating the upper portion of the cord with the electrodes of a weak Faradic current, the two limbs contracted powerfully, and apparently with equal strength.

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The heart of a healthy middle-sized frog was exposed by carefully dividing the sternum with a pair of scissors, and a ligature passed round its back and tied so as to interrupt the circulation. The $\frac{1}{12}$ gr. of theine, dissolved in 10 minims of water, was then injected under the skin of the calf of the right leg. In four minutes both of the posterior extremities were partially paralysed.

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Six grains of theine, dissolved in 2 drachms of water, were injected under the skin over the back of a healthy white rabbit, weighing 2 pounds 3 ounces. Almost immediately afterwards the ears were observed to become paler than before, then suddenly they appeared of a bright red colour, all the vessels being enlarged and congested. After remaining in this condition for half a minute they again became paler and anœmic. These sudden changes from extreme pallor to intense congestion alternated for about five minutes, each stage being about a quarter of a minute in length, after which time the ears became permanently red, hot, and congested. The animal then became restless and somewhat excited, but not hyperæsthetic, and it trembled slightly. When its toe or ear was pinched, it struggled. The force of the heart's pulsation was stronger, and the rapidity of the beats, as well as the respiratory acts, quicker than before. Pupils were unaffected. Three minutes later the hind legs struggled slightly, and they seemed to have lost power. Two minutes after, all four extremities were considerably weaker, and the rabbit was unable to stand upright, but lay flat on its belly, with all its limbs stretched out on the table. When its toe was pinched it did not struggle so much as formerly, still it pulled away its leg and attempted to crawl along, which it did in a shaky and laboured manner. The breathing was laboured and slow, the heart's pulsations were feeble, and the animal trembled. The ears were still intensely hot to the touch and congested. . . . . .

For twenty minutes the animal lay in this prostrate condition, breathing in a laboured manner, when it suddenly took a tetanic spasm, with slight episthotonos, which lasted for about a quarter of a minute . . . .

. For the next seven minutes the animal took tetanic spasms at intervals, occurring spontaneously, and not brought on by pinching or other external irritations.

Evidence of sensibility had disappeared from all parts of the body except the head, where it seemed to be normal.

The eyelids winked when the eyeballs were touched and even when the hands were clapped before them. When any portion of the face was touched its muscles contracted. The animal, although completely paralysed in its limbs, looked intelligent, as if sensation was unaffected. Ten minutes later the pupils were considerably contracted, the breathing slow and irregular, and the heart-beats not palpable. It died after a tetanic spasm. . . .

The animal never seemed to lose its intelligence. It was watchful, followed every motion of the experimenter, although it was unable to make any movement.

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Six grains of theine, dissolved in 1½ drachms of water, were injected under the skin over the back of a healthy cat, weighing four pounds one ounce. In ten minutes the animal became very angry and irritable. Fifteen minutes later this excitement had increased; the animal had a watchful, anxious appearance, prowled about, and when touched with a stick, bit at it and growled. If any noise or motion was made, it put up its back and made a hissing noise. The legs appeared weakened, and although it could still walk about, it preferred sitting in a corner of the room. Its mouth and tongue were very red, and there was an abundant secretion of saliva, which constantly trickled out of its mouth. The cat defæcated and micturated several times. Forty minutes later it continued in much the same condition. Salivation was profuse. Animal suffered from tenesmus, and it had a constant straining from the bowel of a clear fluid like mucus. The limbs, especially the posterior ones, were much weakened, but the animal could still run with difficulty. It could not jump, it made attempts to do so over a bench about two feet high, but failed. The breathing was laboured and irregular. The redness of the tongue and mouth as well as the excessive irritability of the animal had disappeared. It was quiet, lay in a corner, stupid and drowsy. It drank freely of water. Twenty minutes later it was prostrate and lay on its side, its limbs quite helpless. It paid no attention to a pinch of the toe or a blow on the tail with a stick. It seemed however to be intelligent, as its eyes watched every movement of the observer, and when the hands were clapped before its face it growled. The salivation and discharge from the bowels were excessive. Pupils were contracted, and the breathing was laboured. Five minutes later the cat took a series of tetanic spasms, and shortly afterwards died.

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A healthy white rabbit, weighing 2 lbs. 2 ozs., was carefully fastened down on its belly, an incision was made through the skin along the upper part of the spine, about two inches in length, and the vertebral column exposed. By means of bone forceps and scissors, portions of the vertebræ were removed so as to expose a piece of the spinal cord about a quarter of an inch in length. On touching the posterior column with the point of a blunt needle, the animal *struggled violently* and uttered *loud cries.* Twelve grains of theine, dissolved in two drachms of water, were then injected under the skin of the belly. In ten minutes the symptoms, already described in preceding experiments, commenced,—congestion of the ears, &c. On pinching the toe, the animal did not appear to feel it. On touching with the point of a blunt needle the posterior columns of the cord, the animal struggled, but not nearly so violent as before, and it did not cry out. When the anterior columns were touched, there were violent convulsions of the body. Five minutes later the animal was completely paralysed in all its limbs, and presented all the usual symptoms of prostration. A fresh portion of the cord was exposed by cutting away some of the vertebræ below the original wound. On touching as before the posterior columns, the rabbit only quivered slightly. On touching the anterior columns, marked muscular contractions of the limbs followed. The animal was shortly afterwards killed, and similar phenomena were observed after death as have been already described. . . . . .

These and other experiments have frequently been repeated with various modifications. . . . .

The research, however, is in its infancy; and this contribution, I trust, may be looked upon as a stepping-stone to further inquiry. — *Dr. Alexander Bennett, Edinburgh Medical Journal*, 1873–4, pp. 323–34.

As regards the physiological action of caffeine he coincides with previous experiments in considering that it causes increased reflex excitability and tetanus, the action resembling that of strychnia. If, however, one sciatic nerve be divided before the poisoning, that limb is not convulsed; hence it acts on the nerve centres. A frog is tetanised by the subcutaneous injection of a quantity not exceeding 0·005 of a gramme. 0·120 of a gramme injected into the jugular of a rabbit tetanises it, and a cat or dog is tetanised by 0·2 of a gramme. It is remarkable that by maintaining artificial respiration for some time the symptoms of tetanus entirely disappear. Its action on the heart of mammals is that it causes acceleration of the beats, with diminution of the blood-pressure; this last effect he attributes to the poison paralysing the ganglionic nerves of the heart.—*Lancet*, No. 2,551, p. 92.

Dr. Foster, having received a small quantity of the alcoholic tincture and glycerine solution of the alcoholic extract of Jaborandi, placed the drug in my hands, and requested me to observe its physiological action. I have been able to make some observations, which are confessedly very incomplete; but I am induced to publish them, since they seem to have some interest, and because I shall be quite unable to resume them until after the close of the term.

1. Effects on the general nervous system.—Injected beneath the skin of the frog, Jaborandi causes tetanic convulsions not unlike those of strychnia. These continue when the brain has been removed, but do not appear after destruction of the spinal cord. [Several other experiments are described by Mr. Langley.] At page 543–4 of the same journal may be read a report of the effects of this drug on man, by a medical practitioner, who says, "We have carefully watched the effects of the Jaborandi on the circulation, and always find that it increases the frequency of the pulse, and, except when the pulse is frequent from fever, we have never seen this drug lessen the frequency of the beats. Mr. Langley finds that Jaborandi reduces the frequency of the heart's action in animals; hence we must conclude that, in this respect, it affects man differently from animals."—*British Medical Journal*, No. 738, p. 241; and No. 747, pp. 543–4. [*See also* Hurley, p. 20; Marcet, Thorowgood, and Lanat, p. 25; Yeo, p. 38; and Reynolds and Moore, p. 39.]

[The following was a public exhibition :—]

Dr. Sanderson then exhibited a dog, into the abdominal cavity of which six drops of a pyæmic transudation liquid had been injected three hours before. The animal was in a state of profund collapse, accompanied with vomiting, purging, and cramps of the extremities. Shortly afterwards the animal was killed and the abdominal cavity opened.

The peritoneum contained liquid slightly stained with blood, which, on microscopical examination, was found to be crowded with bacteria. The intestines were distended with a frothy liquid, which possessed none of the characters of the natural contents which had been found in other cases to be charged with shed epithelium. The internal surface of the whole of the alimentary canal from the stomach downwards was intensely injected and presented appearances which (as had been found by more careful investigation in previous cases) were due to the separation of the epithelium from the surface of the mucous membrane, and the infiltration of that tissue with liquid.

The material which produced these results was obtained as follows:—Pus from a pyæmic abscess of spontaneous, *i.e.*, accidental origin, was introduced into the peritoneal cavity of a guinea-pig and allowed to remain there for two days. It was then withdrawn from the guinea-pig, and some of it at once injected in the peritoneum of a dog. The dog was affected in exactly the same way as the animal exhibited to the Society. The remainder of the liquid was kept for five weeks in hermetically sealed tubes, after which six drops were injected into the peritoneum of a guinea-pig; this showed its action to have become relatively feeble. After two days (the day before the meeting) the transudation liquid produced was tested with a third guinea-pig and found to be extremely active. On the afternoon of the meeting it was injected into the peritoneum of the dog exhibited.

. . . .

After the experiment Dr. Crisp said he differed from Dr. Sanderson in several of his conclusions. There was a great difference between tubercle in man and that in the lower animals; he had shown long ago that in monkeys and other animals no bleeding occurred from the lungs, that cavities were comparatively rare, that the liver and spleen, rarely affected with tubercle in man, were frequently so in the lower animals, and that in other particulars there were important differences. He (Dr. Crisp), as shown in their "Transactions," had inoculated guinea-pigs with pure pus from a whitlow on his own finger, and had produced tubercle, but in many respects it differed from tubercle in the human subject, although microscopically it was the same. He had also performed numerous inoculations in birds and in other animals, the results of which led him to the same conclusion. It was also important to bear in mind that there was a wide distinction between diseases of the lower animals and those affecting the human subject.

. . . .

What we wanted were experiments to ascertain whether by the use of chemical agents we could render this poison innocuous, whether by any external application we could prevent the occurrence of pyæmia. Again as regards *bacteria* being the cause of pyæmia, he entirely differed from Dr. Sanderson. *Bacteria* were found in numerous diseases of the lower animals, as had been fully shown by the French pathologists, and they were probably the effects and not the cause of the disease, as he (Dr. Crisp) had long since endeavoured to show. He was the first in this country to describe splenic apoplexy, and to point out its deleterious effects upon man and other animals. Experiments had been made by French pathologists, who showed that after the inoculation of rabbits with the blood of animals dying of this disease, that *bacteria* were found in the blood after a certain time, and that death took place at a given period. Dr. Sanderson's experiments appeared to him to have no important bearing upon pyæmia, the irritating matter he used acted more like a poison, such as prussic acid or arsenic, and killed nearly in a definite term, as others had shown.

. . . .

Mr. Hulke asked Dr. Sanderson if he had not confounded pyæmia and septicæmia. It seemed to him that the dog suffered from the latter, and not from pyæmia. If perfectly filtered pus were injected into an animal, the ordinary symptoms of pyæmia were produced, and the animal recovered; but if the pus were unfiltered, these symptoms were produced plus others, such as multiple abscesses, constituting septicæmia. Any putrescent fluid, animal or vegetable, would produce the same result.

. . . .

Dr. Murchison had often had the opportunity of examining the bodies of patients who had died of pyæmia following typhus fever, in which there were no ulcerated surfaces, no bed-sores, and no open wounds whatever, and yet in these pus had been deposited in the joints, under the skin, and sometimes in the internal organs. Everyone who had observed epidemics of typhus fever must have seen that pyæmia followed many cases, and that when one case had occurred in an hospital there were many.—*The Doctor*, 1872, p 132–3. [*See also* pp. 24 and 39.]

The following experiments will show however that all these substances when introduced directly into the blood, give rise to analogous reactions, or are followed by the same series of physiological phenomena, the only difference in them being in the quantity of the different salts required to produce these reactions.

In the following experiments the tubes by which the substances were injected directly into the blood-vessels being connected either with the jugular vein or the axillary artery, hæmadynamometer, when used, was connected with the femoral artery. The pressure is given in inches of mercury. In order to ascertain the general symptoms the animal was left at liberty, the substance being injected through a tube inserted into the jugular vein.

Salts of Magnesia.

Experiment 1.—The animal was a strong healthy dog weighing about 18 lbs., six grains of sulphate of magnesia dissolved in half an ounce of warm water was introduced into the jugular vein.

In 10 seconds the pulsations of the heart were quickened.

In five minutes the action of the heart and the oscillations were the same as before the injection.

Injected 16 grains; in seven seconds the respiration was affected, being deeper, in 10 seconds the heart was affected as before.

In 45 seconds after the injection the animal appeared to be uneasy, respiration more laboured.

After five minutes injected 25 grains, in seven seconds the respiration deeper. In 30 seconds partial spasmodic contraction of muscles . . . . .
heart's action quick, oscillation slight. 60 grains in 3 oz. of water arrested the action of the heart in eight seconds.

. . . .

Only one or two respiratory movements took place after the arrest of the heart's action.

. . . .

Experiment 2.—The animal was a healthy dog weighing 16 pounds. A tube was inserted into the right axillary artery, the point directed towards the aorta, the pressure was taken in the femoral; 16 grains of sulphate of magnesia was injected into the axillary artery. Violent struggles commencing immediately after the injection prevented the direct effect on the pressure in the arteries being observed. One minute, animal quiet, pressure one inch lower, oscillation not so great. In five minutes all effects of injection appeared to have passed, except that the pressure in the arteries was rather lower.

Injected 30 grains, apparently *considerable pain*, and in 40 seconds a general spasmodic contraction of the muscles; this soon disappeared, and the pressure on the arteries was rather lower.

The animal after a few minutes appeared not affected.

Sixty grains injected into the artery arrested the respiratory movements, in seven seconds there was a state of general tonic spasm, and in a minute and a half the animal was dead.

. . . .

Experiment 3.—Dog weighed 10 lbs., not confined; eight grains of the salt was injected into the jugular; 10 seconds, slight dyspnœa; 12 seconds, animal fell down as if suddenly paralysed. It got up almost immediately and walked about two seconds vomiting. After this the animal seemed not at all affected.

Fourteen grains injected; 12 seconds, animal fell on its side, legs extended, no spasm; the legs remained in any position they were placed; no expression of pain, although the animal was perfectly sensible, respiration regular.

After remaining on its side almost motionless for 10 minutes, the animal rose and walked about, its movements were unsteady. 35 grains arrested the action of the heart. Animal fell in 12 seconds, in 30 seconds respiratory movements arrested, animal dead. . . .

Salts of Zinc.

Page 203. Experiment 4.—The animal was a strong healthy dog weighing 15 lbs. A solution containing three grains of sulphate of zinc was injected into the jugular; in 10 seconds the respiration was affected, and the action of the heart rendered slower; in 15 seconds the pressure in the arteries began to fall, and in 35 seconds it had diminished. . . . . . .
after five minutes the pressure was still down . . .
oscillation slight. . . . .

Injection six grains; no apparent immediate effect on the action of the heart, but in 15 seconds the pressure in the arteries began to fall, and in 30 seconds it was only equal to two inches, the respiration was irregular; one minute, efforts to vomit; two minutes, heart stopped, but respiratory movements and efforts to vomit continued one minute after the pulsations of the heart had apparently ceased, at least the pressure in the arteries had sunk to zero.

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Experiment 5.—A solution containing 6 grs. of sulphate of zinc was injected into the axillary artery of a dog weighing 10 lbs. There was immediate expression of pain and partial spasm; 45 seconds, the animal quiet, the pressure in the arteries was about one inch lower, oscillation not so great; two minutes, vomiting.

Injected 15 grains; five seconds, respiration suspended, spasm, retraction of head; animal died in two minutes. The heart continued beating three minutes after respiration was suspended, and after the thorax was opened.

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Experiment 6.—Dog weighing 8 lbs., it was not confined. Inject three grains of sulphate of zinc into the jugular; no marked effect. Inject six grains, 12 seconds after the injection the animal fell down and lay on its side perfectly powerless, no spasm; 45 seconds, efforts to vomit; urine and fæces passed; the animal lay for some minutes perfectly still, the respiratory movements became slow and weak, so that its pulsation could not be felt through the chest, but pulsation in the femoral; sensibility unimpaired; no expression of pain. After 10 seconds inject three grains; 15 seconds, some slight movements, as if the animal was uneasy; respiration slower, ceased at two minutes three seconds; no convulsions; on opening the thorax the heart was found pulsating slowly but rhythmically.

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Experiment 7.—A strong solution of the salt was injected into the jugular of a dog weighing 12 pounds; in 7″ the pressure in the arteries began to sink; in about 45″ was at zero; no pulsation of the heart after 7″. Animal dead 1′ 30.

. . . .

Page 204, Experiment 8.—Salts of Manga.—Dog weighed 11 pounds. Injected 5 grains of sulphate of manga.; 10′ heart's action affected, oscillation less; no sign of pain; pressure diminished 1 in.; 1′ 30″ vomiting.

Inject 10 grains; heart stopped 10″; animal died 1′ 45″. On opening the thorax heart still.

. . . .

Experiment 9.—Dog 9 pounds, not confined. Inject 3 grains into the jugular. 20″ animal fell on its side. Again rose and walked about, lay down again in 2′; lay perfectly still in the same position for 10 minutes, although it could walk when roused. It remained five minutes without moving, the head and thorax resting on the ground, the back part of the body supported by the hind legs; there appeared a total absence of volition; no expression of pain, no convulsions. Inject 4 grains; animal dead in 2′.

. . . .

Experiment 10.—Salts of Cobalt.—Dog weighed 13 pounds. Inject 5 grains sulphate cobalt. 12″ heart affected; 15″ respiration rather deeper; the pressure diminished; at 3′ it had fallen ·3 in.; efforts to vomit. After 10 minutes the animal appeared not to suffer. Inject 10 grains; blood in tubes coagulated, but in 3′ the pressure was 2·5′ in., it having been before the first injection 8·9 in.; respiration slow, heart's action weak.

Inject 10 grains; in 10″ heart stopped; respiration continued 2′ longer. On opening the thorax the heart was still irritable but did not contract rhythmically.

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Experiment 11.—Dog 15 pounds, unconfined; 4 grains sulphate of cobalt was injected into the jugular; 45″ the animal lay on its side; respiration rather slower; 3′ vomiting. At the end of half an hour the animal still disliked moving.

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Inject 10 grains; prostration complete; animal lies like a dead mass; respiration slow, regular action of heart weak. After 10′ it could stand. Inject 15 grains; 10′ the animal fell on his side, head drawn back, struggled a little; respiration stopped 2′. On opening the thorax the heart was found motionless.

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Experiment 12.—Salts of Nickel.—Dog, weight 20 pounds, pressure 5 to 7 in. Inject into jugular 3 grains of sulphate of nickel; 10″ heart affected, quicker, oscillations less; 45″ pressure 4·2 in.; 2′ efforts to vomit.

Inject 6 grains; 12′ heart quicker, respiration deeper. 2 pressure 3 in.; respiration quiet and slow; pulsations 58. Animal appears not to suffer; lies quiet.

8′ inject 9 grains; heart stopped in 14″; respiration continued 2·30. On opening the thorax heart moved slightly.

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Experiment 13.—Dog weighed 12 pounds, not confined; 3 grains of sulphate of nickel was injected into the jugular; no immediate effect; 3′ vomiting; 7′ inject 6 grains; animal fell down; 30″ no expression of pain, no convulsion; breathing regular, slow; sensibility unchanged; remained in this state for some time without the slightest voluntary movement.

Inject 10 grains; heart stopped; animal dead in 2′.

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Experiment 14.—Salts of Cadmium.—Dog weighed 30 pounds, unconfined; 1 grain of sulphate of cadmium in 2 oz. of water was injected into the jugular; no appreciable symptoms. Inject 2 grains; 30′ animal appeared dull; 1′ 30″ vomiting; 5′ inject 3 grains; 45″ vomiting renewed; 4′ animal fell down, lay like a dead mass; respiration regular but slow; no symptoms of pain. On being placed on its feet it would stand for a few seconds, but then gradually sunk down. Inject 6 grains; respiration stopped in 1′ 45″; on opening the thorax heart still.

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Experiment 15.—Salts of Copper.—Dog, weight 15 pounds; inject 3 grains sulphate of copper into the jugular; 12′ the action of the heart affected . .

2′ heart slower. Inject 6 grains, 10′ heart fluttering for a few seconds; 45″ pulsations slower, from 84 to 55; respiration slower; pressure in arteries diminished 2 in.; efforts to vomit. Inject 15 grains; 12″ heart stopped; respiration continued 30″, then arrested for 45″, then recommenced and continued slowly for 45″, the heart apparently beating feebly, although no effect was produced on the pressure in the arteries. 3′ animal dead; on opening the thorax the heart was still.

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Experiment 16.—Dog, weight 18 pounds; a solution containing 2 grains sulphate of copper was injected into the axillary artery; 10″ general tonic spasm, which lasted 40″; respiration then recommenced and continued about 1′; the pressure in the arteries fell gradually, the heart being stopped by asphyxia.

On making an incision into the parietes of the thorax 3′ after the animal had been to all appearance dead, a full inspiratory movement of the parietes of the thorax and of the diaphragm took place.

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Experiment 17.—Dog weighed 12 pounds, not confined; inject 3 grains sulphate of copper into the jugular; 35″ the animal seemed uneasy, but no expression of pain; respiration rather deeper; 2′ vomiting; 5′ inject 4 grains; 12″ respiration deeper, more laboured; 20″ animal lay down, stretched out its legs and cried; no convulsions, sensibility unimpaired; 2′ rolled over on its back; 3′ respiration short and quick; 4′ efforts to vomit, and the animal rose for a short time, but soon fell again; 6′ inject 4 grains; 12′ respiration deeper; 45″ respiration stopped; no convulsive movements, but perfectly still. 1′ 30″ respiration again began, and continued at intervals for 1′ 30″; eyes closed on irritating conjunctiva 2′ after injection. On opening the thorax the ventricles were found contracting slightly.—*Journal of Anatomy and Physiology*, 1870, p. 201 and following pages.

Rabbits.—The sub-cutaneous injection of three grains of bromal hydrate produces the following effects on a rabbit of three or four pounds weight. During the first three or four minutes nothing is observed. The animal then becomes restless, and moves about quickly, rubs its nose with its fore feet, the vessels in the ear become dilated and full of blood, the pupil contracts quickly, but at an equal rate, from its original diameter before injection of 6 M M, or 7 M M to 1 M M, that is, to its maximum X of contraction. In one or two minutes more, the blood vessels of the conjunctiva, more especially on the inner surface of the lids, are injected, and there is great excess of lachrymal secretion, the mucous membrane of the mouth and nostrils also becomes red, and a profuse secretion pours from the mouth. The amount of secretion is often so great as to endanger the life of the animal from suffocation, and it may be collected easily in a watch-glass.

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The respirations become gradually more rapid until they are nearly double their normal amount. The cardiac pulsations are in the first instance diminished in number, afterwards they become more rapid, and they may become double the normal number. Towards the period of death they are much reduced both in number and in force. The animal then lies flat on its abdomen and chest, with the fore and hind limbs in spread-eagle fashion. There is paralysis of both fore and hind extremities. There is no hyperæsthesia so far as can be determined by pinching, or by weak induced currents. In a few minutes more the head sinks on the table, the respirations become slower and more laboured, the heart beats less rapidly and more feebly, muscular twitchings are seen, and after a few clonic spasms the animal dies, frequently in a state of episthotonos. This is the general picture of the action of a minimum lethal dose which kills in from one to two hours. With a larger dose the symptoms are more intense. The animal becomes much excited, the pupil contracts with great rapidity to its smallest diameter, and after screaming for a few seconds, as if in great pain, it dies in convulsions.

With a smaller dose the phenomena just noticed follow consecutively, but the symptoms are less acute, and after a period of stupor the rabbit may recover.

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A dose of five grains produces great distress within four or five minutes. The animal is excited and appears to suffer acute pain. This condition is coincident in time with the contraction of the pupil and the dilatation of the blood-vessels. During the first four or five minutes the animal is quite able to run or leap, but at the end of that period there is staggering, and very soon the power of voluntary movement is lost. . . . . .

When the chord has been involved several convulsions ensue. During the paralytic condition the nerves are still sensitive to very weak induced currents. The sensibility and conducting power appear to be intact. The action is therefore on the cerebral and spinal centres, and not on the peripheral terminations of the nerves.—*Dr. McKendrick*, Physiological Laboratory, University of Edinburgh. —*Edinburgh Medical Journal*, July 1874–5, p. 2.

Note.—Since the above was written, I have experimented by injecting acetic acid into the pleural sac of the horse. The experiment was made more for the purpose of determining the rapidity with which false membranes may form in the chest in pleurisy, and the results are as follows:—

Before the experiment the temperature was 96·4°, pulse 40, and respirations 12. The acid was injected at 10. 35 a.m. At 10. 50 the animal was restless, with a pulse of 48. At 11. 15 there was observed twitching of the superficial muscles of the right side, with slight gastric tympany, and a pulse of 52. At 12 the temperature was 101·4, and now and there was a slight abdominal breathing, with a visible line of demarcation, while the pulse was 54 and hard. The animal remained in this condition for some time, and at 8 p.m. it was found that the pulse had risen to 105°.

On the following day (morning) those symptoms had passed off. The animal was slaughtered forty-eight hours after the injection. A post-mortem having been made, there were found bands of lymph, partly organised on the pleural surface, hydrops pericardii, the quantity of the serum taken from the pericardium being about a quart. The pericardium and endocardium were both highly injected—*"The Principles and Practice of Veterinary Medicine," by William Williams, M.R.C.V.S., F.R.S.E., &c.*, p. 336.

To ascertain this they tested the first material ejected in vomiting in a number of dogs into whose veins tartar-emetic had been injected, and found that it always contained some of the antimonial salt. The fact that efforts to vomit may still occur after the stomach has been excised they do not regard as at all opposed to their view, since the salt may act on the peripheric extremities of nerves supplying the œsophagus or intestine, which also may have the power of inducing vomiting reflectorially.—*Lancet*, No. 2,561, p. 463.

To prevent coagulation of the blood, Dr. Braxton Hicks has recommended the addition of a small quantity of phosphate of soda (Guy's Hospital Reports, 1868, p. 14), and Dr. Richardson minute quantities of ammonia. Even in very small proportions, the ammonia not only prevents or retards coagulation, but in transfusion it acts as a stimulant to the system. Great care is required in using it. Injected into the veins of a dog, insufficiently diluted, it produced convulsions.—*Dr. Madge, British Medical Journal*, No. 680, p. 43.

The experiments made at Norwich by Dr. Magnan were similar to the following, described by himself several years ago:—The dangers of prolonged indulgence in absinthe drinking have been pointed out by many writers, and recently experiments have been made to ascertain the nature of the poisonous action of this substance. Drs. Magnan and Bouchereau add some facts to what is already known (Comptes Rendus, 5 Avril 1869); they administered the poison to dogs, cats, rabbits, and guinea-pigs, and found that convulsions of an epileptic character were quickly produced. The convulsions, they further show, are caused by some component part of the Artemisia Absinthium, and not by the alcohol in which it is dissolved. —*Journal of Anatomy and Physiology*, 1870, p. 313.

In order to compare the capability of resistance in inflamed parts with that in the normal condition, v. W. produced inflammation of the mesentery of the frog by means of cantharides. The animal was injected next day, and always at the same time a sound one for comparison. The injected matter consisted of water, soluble Berlin blue, and gelatine; and the injection was made by means of Hering's apparatus with constant pressure.

The result of 70 experiments was, that as well in normal as in inflamed mesentery, the injected mass passed through the walls of the vessels. . . . .

Similar results were obtained by another series of experiments, where the frog pumped the mass into the vessels by the action of its own heart.—*Journal of Anatomy and Physiology*, February 1875, p. 228.

Vulpian exposed the heart of a curarised dog, and in order to diminish the rapidity of its movements, a quantity of infusion of digitalis was injected into the femoral vein.—*Journal of Anatomy and Physiology*, February 1875, p. 230.

The animals used for experiment were the dog, the rabbit, the sheep, and the frog. Into the dog were transfused the blood of the sheep, of the cat, of the guinea-pig, of man, the frog, the calf and the pigeon; into the rabbit, that of the hare, sheep, calf, and man; and the blood of man into the sheep. A special series of experiments was made with frogs, which were injected with the blood of the dog, rabbit, sheep, man, calf, guinea-pig, pigeon, and pike. The blood of the rana temporaria was also injected into the rana esculenta. The results of these painful experiments are given thus:—

"In many kinds of blood also, when mixed with the " blood of the serum of other species, the blood corpuscles, " are seen to run together into masses; and these masses, " when transfusion is made into the venous system, may " block up the pulmonary capillaries and give rise to very " formidable symptoms." Dr. Laudois remarks that numerous experiments with various modifications are required for the solution of the questions that await an answer.—*British Medical Journal*, No. 687, pp. 280 and 281.

Dr. Guérin has repeated an experiment which was performed by Blundell and others long ago, a sort of double transfusion. Two dogs are placed side by side, and the carotid artery of each animal connected by means of a tube with the jugular vein of the other. In this way they obtain a common circulation, after the fashion of the Siamese twins. Although a most interesting experiment, this can only be regarded as one of the curiosities of transfusion.—*British Medical Journal*, No. 680, p. 44.

The Poison of some Indian Venomous Snakes administered to Dogs, &c. by Drs. Brunton and Fayer.

The general symptoms are depression, faintness, hurried respiration and exhaustion, lethargy, nausea and vomiting. In guinea-pigs and rabbits peculiar twitching movements occur which seem to represent vomiting in them, and occasionally in fact, guinea-pigs do vomit. Dogs vomit, are salivated, and present an appearance as if the hair had all been rubbed the wrong way, "staring." As the poisoning proceeds paralysis appears, sometimes affecting the hind legs first and seeming to creep up the body, and sometimes affecting the whole animal nearly at the same time. There is a loss of co-ordinating power of the muscles of locomotion.

Hæmorrhage, relaxation of the sphincters, and involuntary evacuations, not unfrequently of a sanguineous or muco-sanguineous character, often precede death, and it is generally accompanied by convulsions.

In fowls the appearance is one of extreme drowsiness; the head falls forwards, rests on the beak, and gradually the bird, no longer able to support itself, rolls over on its side. There are frequent startings, as if of sudden awaking from the drowsy state. . . . .

Experiment 1.—1.30. Three drops of this, diluted with water, were injected into the flank of a small dog. Immediately after the injection the corresponding leg was drawn up partially paralyzed.

1.32. He walks less steadily. Tail rigidly held out.

1.35. Is restless and whining. Walks about and then sits down again. Walks unsteadily.

1.45. There are distinct muscular twitches in the shoulder. General tremor.

1.47. There are twitching movements of the back.

2.8. Has been standing perfectly still. Is now pawing and licking his lips. Vomits.

2.10. Vomits again, but licks up part of what he had ejected.

2.22. Has been continually vomiting. The ejection consisted at first of food, afterwards of tenacious mucus. He now lies down apparently exhausted. He is still trying to vomit but can bring nothing up. He tries to rise but cannot. Convulsive struggles occur.

2.25. Breathing has ceased, but the cornea is still sensitive. Convulsive attempts to vomit.

2.27. Cornea insensible. Heart is still beating strongly. Death soon followed.

Experiment 2. A young rabbit weighing 900 grammes was used. An incision had been previously made through the skin of the neck, and the wound again sewn up, but the animal was otherwise uninjured. Two drops of cobra poison weighing 12 centigrammes were diluted with 1 cubic centimetre of water.

At 4.6. The diluted poison was injected under the skin of the left hip.

4.7. Washed out the watch glass in which the poison had been placed with water, and injected it under the skin of the back. The animal sat quiet after the injection occasionally licking its fore paws.

8′ 30″. Respiration seems hurried. The rabbit occasionally makes a jerking motion with its hind feet.

10′. Has been restless, running about occasionally licking its fore feet.

13′ 30″. Still very restless and when held makes convulsive efforts to get away. Ears are much congested.

17′. The animal is now quiet, its ears are no longer congested.

About 20′. Quiet with occasional starts. Disinclined to move but can walk quite well.

25′. Movements seem difficult and hind legs seem weak when it tries to walk.

26′. Paralysis of hind feet is increasing.

26′ 15″. The rabbit lays its head down on the table.

28′. When laid on its side it merely makes a few slight movements with its fore paws, and then lies still. The eyes remain in a half closed condition and have done so for some time. When the cornea is touched the head gives a jerk, but the eyelids move very little. Respiration slow and laboured.

4.30. The chin is twitched inwards, the sternum once or twice, the hind feet at the same time being twitched backwards. The eyes open widely. Slight convulsive extension of limbs.

4.31. Respiration has stopped, cornea is insensible; thorax opened immediately. There were large extravasations of blood under the skin of abdomen and thorax, and under the skin of the left hip. Heart beating vigourously.

The muscles contracted on direct irritation. The foot twitched when the sciatic nerve was exposed and irritated by an interrupted current. The peristaltic movements of the intestine were active after the abdomen was opened.

Experiment 3. Dissolved 5 milligrammes of dried cobra poison which had collected round the stopper of the bottle containing it in 1½ cubic centimetre of water, and injected it under the skin of the left hip of a guinea-pig weighing 790 grammes.

In three-quarters of a minute after the injection the animal became restless and uneasy and began to cry.

1½′. It began to give little starts.

3¼′. The starting motions became greater, the hind quarters of the animal being jerked upwards and the chin drawn in towards the body; continues to cry.

4¼′. Passes water.

7′. Less restless.

15′. Washed out the watch glass in which the cobra poison had been placed with about half a cubic centimetre of water, and injected it as before. Immediately afterwards the restlessness increased.

24′. Seems to be trying to vomit.

27′. It cannot walk rightly.

28′. The hind legs are paralyzed and spread out laterally from beneath it.

29′. Respiration very slow and deep. The animal lies quiet, but convulsive twitches of the limb follow almost every respiration. Respiration 8 in half a minute.

30′. Cornea insensible. Respiration has ceased. Post mortem examination made immediately. The left venticle was much dilated, the right ventricle empty. There were two beats of the left auricle for every one of the ventricle, and the ventricular beat was weak and imperfect.

Experiment 4.—Dissolved 1 centigramme of a substance like gum, and labelled "alcoholic extract of cobra poison" in one cubic centimetre of water. It dissolved easily and formed a somewhat opalescent solution.

Injected about one-third of this (equal to 3½ milligrammes of the dried extract) under the skin of the thigh of a rabbit weighing about a kilogramme. Four minutes after the injection there was no apparent effect; so a similar quantity was again injected, making the total amount received by the rabbit 7 milligrammes of extract; five and a half minutes after the first injection the animal became very restless.

7′. Respiration rapid. The vessels of the ears were noticed to be much injected. On continuing to observe them the injection disappeared, and then returned again. The alternate filling and emptying of the vessels was much more perceptible than in the normal condition. The rabbit sits quietly, but every now and then gives a start.

22′. The condition of the ears has continued the same. The eyes are becoming half shut and the eyeballs turned up.

The animal now begins to tremble. The head is laid down on the table and then raised again; this is succeeded by a nodding motion of the head. The head is next laid down on the table. Respirations twenty-two in fifteen seconds.

24′. The animal has sunk down on its face and paws as if its fore legs would no longer support it. The hind legs, however, still support the posterior part of the body. Respirations eleven in ten seconds. It seems to be trying in vain to raise its head.

26′. Respirations eight in ten seconds. Convulsions. The cornea is sensitive. The rabbit is now lying on its side. Respirations five in fifteen seconds. Pulse twelve in eighteen seconds.

31′. Cornea is nearly but not quite insensible. The eyeball is protruding.

About 31½′. Respiration has stopped. The heart is still beating vigorously.

32′. Cornea insensible. The animal opened immediately. The heart was beating vigorously, twenty-one beats in ten seconds.

An attempt was made to insert electrodes into the spinal cord and pass uninterrupted current through them. No effect followed; but it is not certain that they were well in the cord. Irritation of the nerves going to the hind legs by uninterrupted current had but a slight effect. Direct irritation of the muscles caused them to contract. After the irritation was discontinued, a fibrillary twitching was observed in one of the extensions of the thigh.

42′. Heart still feebly pulsating. Irritation of the brachial, sciatic, and crural nerves has very little effect.

45′. Heart still feebly pulsating.

Experiment 5.—Two drops of cobra poison were injected under the skin of the thigh of a guinea-pig. One or two minutes after the injection the legs of the animal began to twitch. It was then covered with a glass bell jar.

6′. After injection. The legs are again twitching. This is a peculiar motion of the hind legs, in which they seem to make an abortive attempt to kick involuntarily.

7′. Respirations are deeper than usual.

9′. Legs again twitching.

10′. The animal is restless, and moves round and round inside the bell jar. Grunts occasionally, and grinds its teeth. The hind quarters are twitched upwards and the nose is drawn in towards the chin at the same time.

13′. Bites at the spot where the injection was made and passes water.

22′. It can no longer walk.

23′. It has sunk down and lies flat on the table, leaning rather to one side. Respirations are deep. There are occasional twitches of the legs.

25′. Cornea is sensitive. Occasional convulsive stretches.

27′. Cornea almost insensible. Respiratory movement of nostrils continues.

28′. Cornea completely insensible. Post mortem examination made immediately. The muscles of the abdomen were dark coloured. Peristaltic movements of the intestines occurred when the abdominal cavity was opened. The heart was dark and slightly dilated; all its cavities were contracting though feebly. There were three beats of the auricles to each one of the ventricles. Irritation of the nerves in the pelvis caused contractions of the legs.

35′. After injection. The heart is still feebly contracting.

Experiment 6.—October 28th. Injected about a grain and a half, or two grains, of the precipitate which was thrown down from cobra poison by alcohol, into the thigh of a guinea-pig.

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2.30. Injection made. A few minutes afterwards it passed some milky looking water and then remained perfectly quiet.

3.8½. Passed water which was quite clear.

3.33. Injected about two grains into the right femoral vein. It passed clear water almost at once.

3.35. Its nose gave a jerk inwards. Wounded leg drawn up.

3.38. Nose twitches frequently, and the animal emits a faint barking sound.

3.40. Slight tremors.

3.50. Begins to eat a piece of bread placed near it.

3.58. Still twitches.

4.8. Is still sluggish, but seems nearly well. Recovered.

Experiment 7.—October 29, 1872.—About ½ a grain of fresh but coagulated and cheese-like cobra-poison was suspended in distilled water and injected into the back of a guinea-pig weighing about a pound and a quarter.

2.23. Injection made.

2.26. The animal looks scared and is twitching. This guinea-pig is very active.

2.30. Another dose injected. The animal is twitching much. It jumped out of the deep box in which it had been placed for observation. Breathing is hurried.

2.36. It seems better. Another dose injected into the thigh.

2.45. Not much effect. Another dose injected.

2.46. Twitching continues; animal remains active. It recovered. . . . .

Snake poison probably produces its fatal or deleterious effects either by completely paralyzing the nerve centres or other portion of the nervous apparatus, and thus causing arrest of respiration, or by partially paralyzing them, and also poisoning the blood, thereby inducing pathological conditions of a secondary nature, which may according to circumstances cause the slightest or most dangerous symptoms.—*Proceedings of the Royal Society,* Vol. 21, No. 145, pp. 365–70.

An opportunity occurred to me in June last of performing some experiments with the poison of the cobra di capello or *Naja Tripudians.* A small quantity of the cobra poison in a dry state was given to me by Mr. Blackburn, formerly a pupil at Guy's Hospital. . . . . with the aid of my colleague, Dr. Pavy, the experiments were performed in his physiological laboratory. . . . . . .

Two grains thus dissolved, including the gelatinous portion, were introduced into a wound made in the side of a rabbit by Dr. Pavy, the wound penetrated into the cellular tissue beneath the skin, very little blood escaped. The edges of the wound were then sewn up. No particular symptoms manifested themselves for a quarter of an hour. Mr. K. Ashby undertook to watch the animal, and I subjoin a note of his observations.

Operation completed.

Began to show symptom of loss of power in limbs, particularly in hind legs, resting its body on the table without supporting itself on its limbs. It got up and moved when irritated. Respirations quick.

Cannot support itself on its legs

Lies over on its side. Respiration slow.

Eyelid shut lazily when the corner is touched.

Eyes insensible to the touch, six or eight slight convulsions.

Respirations, all movement ceased twenty minutes after the poison had begun to act. Chest opened five minutes afterwards. Heart still beating, and continued to beat for five minutes after the chest was opened, when it became engorged and stopped.

A middle-sized healthy dog was selected for the next experiment. The animal had been kept without food for many hours. Two grains of the poison finely powdered and mixed with a small quantity of powdered gum to give it uniform consistency, was brought to a fluid state with half an ounce of water. This was injected into the stomach of the animal by means of a catheter.

No symptoms of poisoning were at any time observed, and in the following day the dog was as well as usual, and took his food with appetite. — *Dr. Taylor, Guy's Hospital Reports,* 1873, p. 297.

About 280 experiments are recorded performed by Dr. Fayrer on dogs, cats, pigs, kids, birds, rats, horses, and several other animals, consisting of various snake bites. It is needless to add that the animals suffered much pain for periods of from one to several hours duration—in some instances 70 hours.

Nine experiments were performed on dogs, pigeons, and fowls.—*Edinburgh Medical Journal,* Vol. 14, p. 522.

Twenty-seven experiments were performed on fowls, dog, cat, frogs, and fish.—*Ibid.,* pp. 915–21.

Nineteen experiments were performed on dogs, cats, birds, pig, &c. &c.—*Ibid.,* p. 996.

Six experiments on dogs, pig, snake, and civet cat.—*Ibid.,* Vol. 15, p. 236.

Two experiments on two horses.—*Ibid.,* p. 242.

Fourteen experiments on fowls, cats, and snakes.—*Ibid.,* p. 245.

Thirteen experiments were performed on fowls, chicken, and snake.—*Ibid.,* p. 334.

Twenty-one experiments were performed on snails, fowls, dogs, chickens, and snakes.—*Ibid.,* p. 417.

Two experiments on snakes.—*Ibid.,* p. 423.

Four experiments on cock, rabbit, and dog.—*Ibid.,* p. 427.

Five experiments on dogs.—*Ibid.,* p. 428.

Two experiments on dogs.—*Ibid.,* p. 429.

Four experiments on dogs, kid, and pigeon.—*Ibid.,* p. 620.

Eleven experiments on dogs, fowls, and snake.—*Ibid.,* p. 807.

Nineteen experiments on dogs, cats, and fowl.—*Ibid.,* p. 813.

Eight experiments on dogs, kittens, and snakes.—*Ibid.,* p. 994.

Ten experiments on dog, fowl, and snake.—*Ibid.,* p. 998.

Three experiments on dog and fowls.—*Ibid.,* p. 1000.

Six experiments on dogs and fowls.—*Ibid.,* p. 1099.

Four experiments on dogs.—*Ibid.,* p. 1104.

Five experiments on dogs and fowls by the cobra poison. —*Ibid.,* p. 1106.

Six experiments on dogs and fowls by cobra poison.—*Ibid.,* Vol. 16, p. 53.

Five experiments on dogs and fowls by cobra poison.—*Ibid.,* p. 56.

Seven experiments on dogs, cats, and snake, by cobra poison and carbolic acid.—*Ibid.,* p. 57.

Four experiments on dogs and fowls by cobra poison.—*Ibid.,* p. 135.

Four experiments on dogs and fowls by cobra poison.—*Ibid.,* p. 137.

Three experiments on dogs and fowls. Cobra poison. Dr. Fayrer says:—"I have seen as much difference "between the effects produced by different daboias, or by "the same daboia on different animals of the same species, "as in those that had been bitten by the cobra; and on "the other hand similar differences in the effects of the "bites of different cobras, or of the same cobra on different "animals of the same species, as in those bitten by the "daboia."—*Ibid.,* p. 139.

Four experiments on dogs and fowls. Cobra poison.—*Ibid.,* p. 423.

Seven experiments on dogs and fowls. Cobra poison.—*Ibid.,* p. 426.

Five experiments on dogs and fowl by snake bites.—*Ibid.,* p. 430.

Six experiments on dogs and fowls by snake bites.—*Ibid.,* p. 431.

Two experiments on dogs by snake bites.—*Ibid.,* p. 434.

Five experiments. Dogs and fowls.—*Ibid.,* p. 435.

Five experiments performed on dogs and fowls by snake bites. [One of the dogs is described as "wildly excited, "whining, and licking the bitten part, which is bleeding "and swollen."]—*Ibid.,* p. 628.

Six experiments of much the same character as the above, the animals remaining in great suffering for from 2 to 70 hours.—*Ibid.,* p. 631.

The following will generally serve to describe the 280 experiments, as regards the sufferings of the animals.

No. 1. A full-grown pariah dog was bitten in the thigh by a full-grown and vigorous *cobra,* of the variety called by the snakemen kurees keauteah. Two tablespoonfulls of Mr. Otho Alexander's fluid antidote was poured down the dog's throat immediately after the bite, which was inflicted at 12.18 p.m., and the vegetable extract or paste, made into the consistency of honey with liquor ammonia, was well rubbed into the wound and over a large surface round it. . . . .

12.19 p.m., the dog limped on the bitten leg, and seemed restless and uneasy; 12.26, retching; 12.30, very restless, breathing hurried; 12.31, vomiting; 12.33, staggering, profuse defæcation; 12.34, convulsed; diarrhœa; rises, staggers, and falls over into convulsions; 12.35, violent convulsions; 12.37, perfectly paralysed, heart still beats, no respiration; 12.38, dead, in 20 minutes after the bite. . . . .

No. 5. A fowl had 20 drops of the blood of the dog, poisoned by a cobra in experiment No. 1, injected with the hypodermic syringe into each thigh at 12.42 p.m. 12.50, crouching; 12.55, crouching, feathers ruffled; 1.5, sluggish, eyes closed, drowsy; 1.30, stands with head depressed, feathers staring, eyes closed, very drowsy; 2, very drowsy, head drooping; after this the fowl slowly recovered, and on Monday 24th was quite well. The poison in this experiment must have been infinitesimal in quantity, only 40 drops of the blood of a full-grown dog poisoned by a cobra were injected. The symptoms of poison were well marked, though the bird ultimately recovered.

No. 6. A solution of one part of cobra poison to eight parts of liquor potassæ was prepared by Dr. Ewart, and of this nine drops were injected into a fowl's thigh at 12.57 p.m. There was a floculent-looking deposit caused by the mixture of the fluids. 1 a.m., drooping; 1.2, crouching; head falling over, nearly paralysed; 1.4, convulsed; 1.7, dead, in seven minutes, with all the symptoms of cobra poisoning. . . . . . .

No. 8. Twenty drops of the blood of the above fowl, removed immediately after death, injected into either thigh of a fowl at 1.10 p.m. 2.15, sluggish; 4.10, drowsy, head falls over; 4.20, no convulsions; 7.15, dead, in six hours and 15 minutes. . . . .

No. 10. A fowl was bitten in the thigh by a Bungarus cœruleus (Krait) at 1.22 p.m. 2.24, feathers staring, eyes have a fixed glaring stare; 1.25, stretches out the neck, falls over, point of beak resting on the ground; 1.26, convulsed, puncture in thigh ecchymosed and œdematous; 1.29, dead, in seven minutes. . . . .

No. 11. A cat was bitten in the thigh by a cobra (Tentuliah keauteah) at 1.46 p.m. Mr. O. Alexander's antidote and extract was administered, according to his instructions, immediately, 1.47 p.m. Pupils widely dilated; cat lies stretched out, hurried breathing; 1.51, convulsed; 1,52, paralysed, heart still beats, no respiration; 1.55, dead, in nine minutes.

This cat was on a former occasion bitten by a large Bungarus fasciatus, and showed no sign of poisoning. .

No. 12. A fowl was bitten in the thigh by a large Bungarus fasciatus at 1.44 p.m. 1.54, drooping head, falling forwards; 1,58, convulsed, cannot stand; 2 p.m., convulsive movements; there is a peculiar vocal sound as though the thorax was compressed; 2.5, convulsed; 2.10, dead, in 26 minutes. . . . .

No. 13. A young rat was bitten in the thigh by a Bungarus cœruleus at 2 p.m.; insensible immediately, dead in 30 seconds.—*Ibid*, 1870–71, p. 721.

Irritation of the roots of the pneumogastric, in Bernard's famous experiment of puncture of the fourth ventricle, has the same effect as similation of its trunk.

This experiment is performed by pushing an instrument like a bradawl through the skull and cerebellum till it reaches the olivary fasciculi in the medulla oblongata.—*Dr. Brunton, British Medical Journal*, No. 680, pp. 40 and 41.

For the experiments *non*-narcotised dogs were for the most part employed, the surface of the brain being stimulated by weak induced currents. In general, the facts already known were confirmed.

. . . .

After stimulation with quite weak currents, after movements, dependent on the stimulated centre, were manifested by the muscles, which movements often passed into general *convulsions*, but could only be produced from the points which were to be regarded as so-called centra.—*Journal of Anatomy and Physiology*, February 1875, p. 210.

The author's experiments upon rabbits confirm the statement of Riegel and Jolly that the vessels of the pia mater do not contract upon a stimulus being applied to peripheral sensory nerves. After an experiment upon a tracheotomised rabbit (to which no curare had been given) had shown that this poison had no effect on the vasto motor channels; it was tried to investigate the central circulation without trepanning, and with the exclusion of air, viz., by ophthalmoscopic examination of the retinal vessels.

The occurrence of a strong and continual flow of tears, rendered every attempt at examination fruitless.

Lastly the experiments with trepanning and the setting a piece of glass in the skull seemed to be useless, in that the action of the air caused inflammation.—*Journal of Anatomy and Physiology*, February 1875, p. 213.

Veyssiere has recently made some experiments on the localization of sensibility in the dog. The method he employed was to introduce a puncture in the cranium. The stylet was then withdrawn, and another bearing a concealed spring which could be thrust out at an angle from its extremity was pushed down the cannula, the spring projected and the requisite laceration made, the spring recovered, and the instrument withdrawn.

No. 1. The lesion was limited to the posterior part of the intraventricular portion of the corpus striatum and the anterior part of the optic thalamus on the left side. Imperfect hemiplegia and hemiancœsthesia of the right side. The hemiplegia passed off in twenty-four hours, the animal was well in three days.

. . . .

[The other four experiments are of the same character.]
—*Edinburgh Medical Journal*, January 1875, p. 659.

By means of a sharpened spoon the outer and upper portion of the anterior lobe of the hemisphere was so far removed that the parts lying immediately outside of the anterior portion of the lateral ventricle were exposed, without, however, opening the ventricle. (The anterior can be opened without injury.) The deepest part and that lying next the middle line in this prepared surface corresponds to the corpus striatum.

When the surface of the corpus striatum was stimulated electrically, it was shown —

1st. That movements of the muscles of the opposite side occurred upon the application of weak induced currents.

2nd. That the points where stimulation of the intact surface of the brain was followed by distinct groups of movements are also present on the surface of the corpus striatum.

3rd. That the opposite position of the active points is the same in the corpus striatum as in the surface of the brain.

If the deepest part of the corpus striatum is stimulated the animal opens its mouth, puts out its tongue, and draws it in again alternately.

These are the movements whose centres are pretended to be found on the convolutions of the under surface of the brain, *i.e.*, lower frontal and suprasylvian convolutions.—*Dr. Sanderson, Journal of Anatomy and Physiology*, November 1874, p. 209.

In his experiments on this subject, Afanasieff divided one or both peduncles through a hole in the temporal bone. In consequence of the transitory irritation which the section produced the animal immediately afterwards drew itself together, the head was inclined to that side on which the peduncle had been divided, the pupils became contracted, especially on that side, and the arteries of the ears also contracted, but their contraction was more marked in the ear of the opposite side. In six seconds after the operation all the above-mentioned effects were succeeded by their opposites. The irritation also produced increased flow of tears and saliva, and twitchings of the extremities on the side opposite the section, all of which lasted for half an hour. Section of one peduncle between the pons and the tuber cinereum produced paralysis of the muscles of the extremities on the opposite side, and of those of the back and neck on the same side. The amount of paralysis increased with time. . . . . .
In two or three weeks it was again able to run straight forward.—*Journal of Anatomy and Physiology*, 1871–2, p. 218.

Dr. Schüller (Berlin) performed experiments on rabbits by removing with a trephine a portion of the upper part of the parietal bone without injuring the dura mater, and generally at the same time taking away the cervical sympathetic with the superior cervical ganglion of the same side. He was thus able to observe the vessels of the pia mater, and to notice any changes in their filling. The substances with which he experimented were mustard, nitrite of amyl, ergotin, opium, and chloroform. Small sinapisms produced scarcely any effect on the vessels, the application of large ones, on the one hand, was first regularly followed by dilatation, which was followed by more or less rapidly alternating changes in the calibre of the vessels, and finally by contraction, which often continued an hour and a half after the removal of the sinapism.
—*British Medical Journal*, February 1875, p. 279.

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Injuring with a needle a certain spot on the surface of the brain of the rabbit peculiar disturbances occur, above all hæmorrhage in the lungs and in the tissues of the same, often so pronounced that almost the whole lung is traversed by the hæmorrhage.—*Journal of Anatomy and Physiology*, 1873, p. 397.

J. Schrieber has operated on a large number of rabbits with a view to determine the effect of injuries to the brain on the temperature of the body.

The injury was made by means of a lancet-shaped needle introduced through the skull. The temperature was measured in the rectum. From about seventy experiments the author concludes that after injury of the pons in all parts, of [the pedunculi cerebri, of the cerebellum and cerebrum, increase of the body temperature occurred when the animals were protected artificially from losing warmth, that the same results followed unconditionally and constantly on injury to the limit between the medulla and the pons.—*Journal of Anatomy and Physiology*, 1873, p. 398.

1st. Ablation of both spheno palatine ganglia does not affect, in dogs and cats, the sense of taste, in parts supplied by the linguals.

2nd. After section of the chorda tympani, in dogs and cats with cut glosso-pharyngei, the taste was little modified in some cases, notably diminished in others, and completely abolished in one.

. . . .

3rd. . . . After action of the chorda, in cats, dogs, rabbits, and guinea-pigs, degenerated nerve fibres were found in the terminal branches of the lingual as well as in the mucous layer of the tongue and submaxillary gland . . . . .

4th. . . . After section of the chorda in the ear, the central end of this nerve (on the side of its emergence facially) remains healthy.—*Journal of Anatomy and Physiology*, 1873, p. 398.

Curarised dogs in whom artificial respiration was maintained were used for experiment.

After having exposed the glosso-pharyngeal nerve below the base of the cranium, it was tied, and then cut above the ligature, so that its peripheral extremity could be excited by electricity. The effect of the faradisation lasted some time, and could be renewed at pleasure.—*The Doctor*, June 1st, 1875, p. 105.

No. 1. The first pigeon had had the anterior part of both hemispheres removed. The bird was able to fly and walk, and in every respect seemed to have its faculties intact, except that when it tried to peck up food it never succeeded in getting hold of a single particle. That was what he invariably observed when the anterior part of the cerebral lobes had been removed.

No. 2. Showed two pigeons with the posterior part of both hemispheres removed. These were not quite so active as the first bird. He had never seen them attempt to eat at all.

No. 3. There were here two pigeons, in which the right cerebral hemispheres had been removed. One could scarcely be able to detect any difference between them and ordinary pigeons. The removing of one cerebral hemisphere made almost no difference.

No. 4. In those classed No. 4 the left cerebral hemisphere had been removed, and the birds were both quite active. They saw on both sides, and were in the same condition as those in No. 3.

No. 5 was a bird, the right cerebral hemisphere of which he had removed, along with a considerable slice of the upper part of the right corpus striatum. He had shaved off the grey matter that represented the cerebral hemisphere, and had carried the knife deeply through the substance of the brain, so that he was quite sure that he had removed a considerable portion of the right corpus striatum. It would be observed that the bird was blind on the opposite side.

No. 6 showed the left cerebral hemisphere and the left half of the corpus striatum removed. The result was exactly the same (only on the other side) as in the other case. This was a point he was very glad to be able to demonstrate, as he was somewhat doubtful about it for a considerable time. He now knew that if he removed the upper part of the corpus striatum there was loss of vision, in the pigeon at all events, on the opposite side.

No. 7 was a bird from which he had intended to remove the cerebral hemispheres only, but in the operation he unfortunately injured the upper part of the corpus striatum. The forceps with which he removed the skull-cap slipped deeply into the corpus striatum, so that the bird was much more in the condition of pigeons in which the corpus striatum had been injured as well as the cerebral hemispheres. It could fly, it could walk; he did not think it saw or heard, or if it did do so it was only to a small extent. It did not manifest any sign of alarm, and if flung into the air could fly quite well. Birds in that condition very often attempted to take food, but they never pecked up anything, and birds in this condition required constantly to be fed, so that it became a serious matter to keep a number of them.

. . . .

He gave the birds water, but had to introduce the beak; they dipped their heads deeply into the water, and took one drink. Where the hemispheres and corpus striatum had been removed the birds seldom took a second drink.

No. 8. Showed two birds in which the cerebral hemispheres alone had been removed. By these he tried to illustrate that consciousness was apparently not entirely lost; one could still frighten the birds.

No. 9 was a bird exactly in the same condition as the previous two.

No. 10 was a bird on which he had performed the experiment he had described, of injuring the deeper parts of the corpora striata and the cerebral hemispheres by passing a long narrow knife into the side of the head and severing the connection. The corpora striata, though injured, had not been removed. The creature was in a state of deep stupor.

No. 11 was a bird in which both cerebral hemispheres had been injured, but not removed. The bird was still able to fly, but he had observed that the creature had not taken any food since the operation.

. . . .

No. 12. In the cases of No. 12 both birds had a large portion of the cerebellum removed, and he thought it would be found that the faculties of the birds were intact, and that they could fly and walk.

No. 13. Was a bird in great health. On the 5th March he removed from the animal considerably more than half of the cerebellum. At first the bird staggered slightly in its movements, but now it had completely recovered. Within the last six weeks it had been regaining its plumage, and was now as handsome a pigeon as one could see.

Dr. M'Kendrick concluded by stating that if any wished to see him operating, he would have much pleasure in receiving a visit from them at the Laboratory.—*Edinburgh Medical Journal*, 1873, pp. 652–3.

He experimented exclusively on the brain of the rabbit. On puncturing with a fine microscopic needle a spot lying in the interior of the posterior end of the cerebrum the animal sprung from the table and exhibited unusually violent spasmodic movements, which appeared either at the time of puncture or a second or so thereafter (at the latest two minutes), and lasted not longer than three minutes.

. . . .

Many of the animals so injured had meningitis with dippacu, but asph. and death without meningitis was also observed.

. . . .

More than 40 experiments in different ways and different directions were made in the thalamus opticus. Slight disturbance of the superficial layers were without effect. In a few cases the paralysis of the extensors of the finger observed by Schiff occurred. If punctured more deeply and towards the middle line, the limbs of the opposite side were directed towards the middle line. This was specially and almost exclusively observed of the fore limbs. The deviation was the most pronounced the more basal the direction of the puncture. The deviation in all cases was only temporary, disappearing sometimes after a few hours, in most cases after 24 hours. In other cases, immediately after puncture the head was turned to the opposite side, the fore limbs strongly divergent, the one directed outwards, the other (opposite side from injury) towards the middle line. No disturbance of sensibility. [The Editor states that these results are at variance with those alleged to be found by Fournier].—*Journal of Anatomy and Physiology*, 1873, p. 395.

Leyden has made a series of observations upon the movements of the brain, and the blood pressure within the cranium, by means of a manometer screwed into an artificial opening made in the skulls of dogs.—*Dr. Rutherford, Journal of Anatomy and Physiology*, 1867, pp. 358–9.

He gives the results of a number of experiments upon the crura cerebelli of rabbits. The author, working under the direction of Professor Eckhard, has taken great care to ascertain the exact position and extent of the lesion which he inflicted.

. . . .

He gives an exact description of his mode of operating, which will prove of not a little service to those who may wish to perform experiments, whether for the purpose of research or demonstration.

In his experiments on the "tract of the crus cerebelli, formed by the anterior and posterior crus," he found that very much the same effects followed a variety of injuries to the tract comprehending punctures to the depth of one, usually two, millimetres, some in its middle, some on its outer, others on its inner side. Irregular but not violent contractions followed these injuries. Voluntary movement was slightly disturbed. Some animals showed a tendency to lie upon the injured side.

. . . .

In a second series of researches he studied the effect of complete division of this tract. The section was followed by irregular convulsive contractions of varying intensity, extending throughout the entire body.

. . . .

Voluntary movement seemed to be entirely abolished. The animal lay upon the injured side and fell always into this position when it was placed in any other. Some animals were observed in this state for three or four hours, others died from the hæmorrhage, which the operation for exposure of the brain had occasioned.—*Journal of Anatomy and Physiology*, 1869, p. 208.

At the recent meeting of the British Medical Association Dr. Brunton read a paper, communicated by Dr. Ferrier, containing an "Abstract of Experiments on the Brains of "Monkeys, with special reference to the Localisation of "Sensory Centres in the Convolutions." The experiments, which were conducted by trephining and the destruction of the sensory centres by means of a red hot wire, led to the following results: These centres are bilateral, so that when one of the centres of touch was destroyed there was loss of tactile sensibility in the corresponding half of the body. Stimulation of the centre of hearing caused the animal to prick up its ears, as if it heard something, while destruction of the whole of this centre rendered the creature totally deaf. Destruction of the centre of vision corresponding to one eye (*e.g.*, the right) only rendered the animal temporarily blind in that eye, the function, after 24 hours, being carried on by the opposite centre. In the discussion that followed Dr. Nairne pointed out that other observers had arrived at conclusions different from those of Dr. Ferrier, and that the brain of a monkey could not be taken as exactly similar to that of a man; but Dr. Brunton thought the mistake made by German and other investigators who differed from Dr. Ferrier was, that they took the brains of animals lower even than the monkey to correspond with that of man. M. Dupuy had arrived at different results. He said that he had found that when the centres of motion on one side of the brain were removed paralysis followed for a short time throughout the corresponding part of the body, but that when the centres were removed from both sides of the brain there was no paralysis at all.—*Lancet*, No. 2,712, p. 289.

Dr. Brown Séquard, in a lecture before the Boston Society of Natural History, published in the Boston Medical and Surgical Journal, dissents altogether from the conclusions that have been drawn on this subject, and which have lately occupied so much attention.

Our readers will remember that Fritsch and Hitzig, followed by Charcot, Ferrier, and others, concluded that the fronto-parietal convolutions of the brain are the centres for the voluntary movements of definite groups of muscles; but Brown Séquard explains the facts in quite another way.—*The Doctor*, October 1st, 1875, p. 181.

Prof. Hitzig refers to Burdon Sanderson's experiments, and remarks "that the localised points on the surface of "the brain given by B. Sanderson do not correspond with "those described by himself."—*Journal of Anatomy and Physiology*, November 1874, p. 210. [*See also* p. 39.]

On the Suffocation of Animals.

About the year 1860 the Royal Humane Society received several suggestions from Dr. Silvester for restoring suspended animation in persons apparently drowned. Other methods were shortly afterwards placed before the society, on the merits of which that body felt itself unable to decide, and consequently its committee desired the Royal Medical and Chirurgical Society to investigate the several proposals, without indicating the tests which that learned body should apply, or the means they should adopt. It is important to bear in mind that the Royal Humane Society is in no way answerable for the course taken by the Royal Medical and Chirurgical Society, and that they were not even aware, until the delivery of the report from which the following extracts are made, that experiments on animals had been performed by the Committee of Investigation.

Seventy-six experiments were made on animals, in only a few of which anæsthesia was present; and after the terrible sufferings caused by plugging their windpipes to suffocate them, holding them under water, and in some cases restoring them to life for further experimentation, burying their heads in liquid plaster of Paris or mercury, cauterizing their bodies with an iron heated to a white heat, &c. &c., the committee, it will be seen, report that they were unable to recommend any material improvement in the plan adopted by the society.

Report of the Committee appointed by the Royal Medical and Chirurgical Society to investigate the subject of Suspended Animation.

Members of the committee.—C. J. B. Williams, M.D., F.R.S.; C. E. Brown-Séquard, M.D., F.R.S.; George Harley, M.D.; W. S. Kirkes, M.D.; H. Hyde Salter, M.D., F.R.S.; J. B. Sanderson, M.D.; W. S. Savory, F.R.S.; E. H. Sieveking, M.D. (ex officio).

At the first meeting of the committee it was resolved to pursue the inquiry—

By means of experiments upon living animals.

By means of experiments upon the human body.

Two sub-committees were forthwith appointed for these purposes.

. . . .

Report of the Sub-Committee appointed to investigate the subject of Suspended Animation by means of Experiments upon living Animals.

In investigating anew the subject of apnœa by means of experiments on the lower animals, it seemed expedient to observe, in the first place, the principal phenomena of apnœa in its least complicated form, viz., when produced by simply depriving the animal of air.

The following plan of effecting this was adopted:—The animal was secured on its back, and the trachea was exposed by a single incision in the mesial line of the neck. A ligature having been passed round it, it was opened by a vertical cut, and a glass tube, as large as could be conveniently inserted, was passed into it for a short distance downwards, and firmly secured by the ligature. Through this tube, while patent, the animal breathed freely, but the supply of air could be at once completely cut off by inserting a tightly fitting cork into the upper end of the tube. It was ascertained by separate experiments that the tube thus plugged with the cork was perfectly air-tight. . . . . . .

The duration of the heart's action was conveniently ascertained by means of a long pin inserted through the thoracic walls into some part of the ventricles. So long as the heart continued to beat the pin moved, and its motions were thus recorded for some time after the cardiac sounds had ceased to be audible:—

Experiment 1.—A full-grown healthy dog was suddenly deprived of air by plugging the tube placed in the trachea in the manner already described. Its first struggle occurred in 25 seconds; its first respiratory effort was not recorded, its last took place at 4 minutes 40 seconds, and its last heart's beat at 6 minutes 40 seconds, or exactly 2 minutes after the last respiratory effort.

[Eight similar experiments were performed on dogs, rabbits, and one cat. In three of these the plug was withdrawn at about 4½ minutes, but the animals were found to be dead.]

From nine experiments it is seen, that in the dog the average duration of the respiratory movements after the animal had been deprived of air, is 4 minutes 5 seconds, the extremes being 3 minutes 30 seconds and 4 minutes 40 seconds. The average duration of the heart's action on the other hand is 7 minutes 11 seconds, the extremes being 6 minutes 40 seconds and 7 minutes 45 seconds. Lastly, these experiments lead to the belief that on an average the heart's action continues for 3 minutes 15 seconds after the animal has ceased to make respiratory efforts. The extremes being 2 and 4 minutes respectively.

In the case of the three rabbits experimented upon, we find that on an average they ceased to make respiratory efforts in 3 minutes 25 seconds; that their heart's action

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stopped in 7 minutes 10 seconds; and, consequently, that the interval between the last respiratory effort and the cessation of the heart's action was 3 minutes 45 seconds.

[Then follows a series of experiments in which the plug was withdrawn at different intervals, and the dogs delivered from the pains of death, and were subsequently operated on again. The committee say :—]

These results lead to the conclusion, 1st, that a dog may be deprived of air during a period of 3 minutes 50 seconds, and afterwards recover without the application of artificial means; and 2ndly, that a dog is not likely to recover if left to itself after having been deprived of air during a period of 4 minutes 10 seconds.

. . . .

For some time after the occlusion of the tube in the trachea the force of the respiratory efforts was so remarkable that it was determined to adopt some means of measuring it. . . . . . . The following were the results :—

Experiment 15.—A medium-sized dog was treated in the above way. The respiratory efforts commenced at 2 minutes 5 seconds. As apnœa advanced they became more powerful, and from 3 minutes 20 seconds and onwards they were very violent till 4 minutes 45 seconds, when they ceased. The needle showed the heart to be moving up to 8 minutes. This dog drew the mercury up the tube, by its violent inspiratory efforts, a height of four inches, and that height was attained in almost the last attempts at respiration, 4 minutes 45 seconds after the establishment of the apnœa. On examination the lungs were found to be congested, but there were no ecchymosed spots or blood in the tubes.

[Then follow similar experiments which produced similar congestion of the lungs.]

. . . .

In the following experiments the great force of the inspiratory efforts was demonstrated in another way.

. . . .

Experiment 18.—A guinea-pig was held so that its nose was immersed in mercury, the animal being upside down, and the nose inserted sufficiently deep in the mercury to prevent the possibility of getting any air. The respiratory efforts commenced at 35 seconds, and ceased at 1 minute 37 seconds. On examining the lungs they were found full of globules of mercury, which had thus been drawn up by this weak animal a distance of an inch or two, and that in spite of gravitation. . . . . . .

Experiment 19.—A terrier was deprived of air by plunging its head into liquid plaster of Paris, the object being, to see, through the whiteness of the plaster, whether any of the fluid obtained access to the lungs. Respiratory efforts commenced at 1 minute 35 seconds, and ceased at 4 minutes, the heart beating till 5 minutes. On examining the lungs the white plaster was found throughout the bronchial tubes.

. . . .

From these preliminary experiments on the effects produced by simply depriving an animal of air, the Committee passed on to the consideration of drowning. The first point to be ascertained was, "for what period can an "animal be submerged and yet recover without the aid "of artificial means?"

Experiment 20.—A medium-sized dog was fastened to a board and submerged in a large bath. It was removed in 4 minutes, but although the heart went on acting for 4½ minutes longer, it neither gasped nor moved.

[Two similar experiments follow.]

. . . .

It having been thus ascertained that 4 minutes drowning kills, it was determined gradually to shorten the time, in order to find out what was the limit of time at which immersion proved fatal.

Experiment 23.—A dog was bound as before to the board and immersed for 3 minutes 15 seconds. On being taken out of the water no respiratory efforts were made; the dog was dead. Bloody froth escaped from its mouth, and its lungs were full of the same material.

Experiment 24.—The same as above, but the dog's head was kept under water 2 minutes only. The dog gasped once or twice, and then died. Lungs full of blood and watery froth."

[Five other dogs were then submerged, and removed at graduated intervals of time.]

. . . .

Having seen that a dog lives after being submerged 1 minute, and dies after being submerged 1 minute 30 seconds, another experiment was performed.

Experiment 30.—A large dog was submerged 1 minute 15 seconds. On being removed, it perfectly and almost immediately recovered.

Thus, then, the remarkable fact appeared that, whereas in simple apnœa recovery may be possible after the deprivation of air for 3 minutes 50 seconds (Experiment 13), and subsequent experiments showed that a dog simply deprived of air almost certainly recovered after 4 minutes, 1½ minutes immersion in water suffices to destroy life.

Now, to what is this striking difference due? With reference to this question the following experiments were performed :—

It was resolved in the first place to eliminate the element of exhaustion produced by struggling; it was thought that possibly the violent struggles of the animal to gain breath when its limbs were confined might exhaust it and hasten the catastrophe.

Experiment 31.—A cat was placed in a cage, and the cage plunged under water. The animal's limbs were at perfect liberty, and there were no violent struggles. After 2 minutes the cage with the cat in it was taken out, and the cat was dead.

Experiment 32.—A dog was treated in the same way, but the cage was kept submerged in the water only 1½ minutes. The dog died. There had been no struggle.

Thus it was seen that struggling had nothing to do with the early fatal result, as it happened equally soon when there were no struggles.

It was next determined to eliminate the element of cold, and for that purpose the following five experiments were performed, in which cold was applied to no part of the surface except the animal's head.

[All the dogs died.]

. . . .

Still further to clear up this question it was determined to place two dogs under precisely similar circumstances, with the single exception that in the one case the free access of water to the lungs should be permitted, and in the other case prevented. The following were the experiments :—

Experiment 38.—Two dogs of the same size were fastened to the same plank and submerged at the same moment, but one of them had previously had its windpipe plugged in the usual way and the other had not. At 2 minutes they were taken out together; the one that had been plugged at once recovered, the other died."

[This was repeated, and the Committee add]—

These experiments satisfactorily show that the difference between apnœa produced by plugging and that by drowning is not due to submersion, to depression of temperature, or to struggles. . . . . . .

. . . .

The fact that animals do not recover after so short a period of immersion is mainly due to the entrance of water.

[Three other experiments were tried with windpipes plugged, after which chloroform was used.]

Experiment 43.—A medium sized dog was rendered insensible by chloroform and drowned. It was kept in in the water for 2½ minutes. Its respiratory efforts were by no means violent, and were in this respect in strong contrast with those of the unchloroformed dogs.

[Two other experiments of a similar kind followed, which proved]—

That by simply depriving the animal of the power of making violent respiratory efforts the period during which submersion may be continued, and yet recovery follow, is at once prolonged. The value and conclusiveness of these chloroform experiments, as showing the essential connexion between the early fatal result in drowned animals and the violent inspiratory efforts that fill the lungs with water, need not be pointed out. . . . . .

Various means of resuscitation were employed in many of the experiments performed by the Committee, and with variable results.

[Then follow seven experiments, in all of which, save one, the animals died.]

Experiment 51.—The same dog as had been previously used in Experiment 13 was again deprived of air, as before, and on the use of Dr. Marcet's instrument 1 minute after respiration had ceased it again recovered.

[This poor dog, therefore, recovered from death twice. The Committee then say]—

No definite conclusion concerning the relative value of the various methods of artificial respiration can be drawn from these experiments.

For this subject the Committee would refer to the "Report of the Experiments upon the Dead Human Body." . . . . . .

. . . .

Many other methods of resuscitation which have been recommended were practised, including actual cautery, venesection, cold splash, alternate application of hot and cold water, galvanism, puncture of the diaphragm.

[Twenty-six experiments under this head were tried, in which the animals were "suffocated in the usual way by plugging their windpipes," and the Committee add]—

Although some of the above means were occasionally of manifest advantage, no one was of such unequivocal efficacy as to warrant the Committee in specially recommending its adoption.—*Report of the Royal Humane Society*, 1865, pp. 31–66.

[Numerous experiments were also made on the bodies of deceased human beings which appear to have yielded excellent results.

Reference to the Reports of the Royal Humane Society will show that with immaterial modifications the method originally introduced to the Society by Dr. Silvester is still in use; such modifications having arisen out of experiments on dead human bodies alluded to, and were not derived from the foregoing painful experiments on animals.]

On the occasion of presenting the above Report to the Royal Medical and Chirurgical Society, the chairman of the investigating committee, Dr. Williams, closed his own summary of proceedings by saying :—" So far then as these " experiments go, they show a great superiority of Dr. Sil- " vester's over Dr. Marshall Hall's ' ready method.' "

Dr. Edward Smith remarked upon the great importance of the present discussion from the interest of the subject, and the fact this being the first occasion on which the society had appointed a committee to make scientific investigation, it might be a precedent for future action. He thought it most desirable that the society should endeavour to accurately estimate the true value of the results which such committees could produce. On the present occasion they had a committee amongst whom were men of world-wide reputation, and a subject of inquiry of the highest interest, and not of greater complexity than would be found in all practical questions in medicine. The Report must be regarded in two aspects : one that of the scientific facts which had been elicited ; the other in their exact application to the purposes for which the committee was appointed—to determine the best methods of restoring the drowned. As to the facts, no one could doubt their extent and interest, the care with which they had been ascertained, and the pains taken to estimate the influence of disturbing causes . . . . . .

It was in reference to the practical object in the appointment of the committee that the Report failed. The committee had not proved that any one of their inquiries was applicable to the drowned human subject. The time during which a man could be immersed in water and recover could not be proved by experiments on dogs, and the committee themselves had shown that all their plans for the restoration of drowned dogs had failed. The committee had in one part of the Report disclaimed any intention to say how far the Silvester method was fitted for the restoration of the drowned ; and yet in their recommendations they advise the use of this method almost exclusively, without having in any experiment tried it, under these conditions. The recommendation to place the body prone and allow fluid to run out of the mouth, was an old recommendation ; but they had inferred and not proved its value, and that only from experiments on drowned dogs which they could not resuscitate. The experiments on dogs had shown that neither hot nor cold water alone had any value as restorative agents, but that the alternative of the two was somewhat useful ; but this alternative had not been recommended for man .

. . . .

Dr. Webster said that he thought the Silvester method was the best, and that the recommendation was very important. He was sorry to hear that the lives of so many dogs had been sacrificed in the experiments. He hoped that in future, if possible, experiments on living animals would be avoided . . . . .

Mr. Charles Hunter said that as he was one of those gentlemen who six years ago conducted the experiments upon the dead body for Dr. Marshall Hall, and upon those experiments the "ready method" was established, he felt called upon for a few words in its defence. He regretted that the committee thought fit to condemn it, and observed that if the Marshall Hall method after all was a failure, the long series of experiments carefully made by him (Mr. Hunter) with others must go for nothing ; and yet the original experiments were much more numerous than those made by this committee, and perfectly conclusive in their general results to those who made and saw them.

. . . .

Dr. Williams would remark in reply to some objections made by Dr. Webster as to the destruction of animal life involved in these experiments that no one experiment had been undertaken without a definite and useful object ; that animal suffering and life had been spared as much as was possible in pursuing the inquiry ; and he did not think that, when it was considered how animal life was hourly and unsparingly sacrificed for the gratification of appetite, there could be any objection to the dedication of a few lives to the elucidation of a subject of real importance to the interests of humanity.—*Lancet*, 2,028, p. 39–40.

[Other similar experiments were made by Dr. Waters, and the results communicated by Dr. Sharpey to the Royal Medical and Chirurgical Society, May 14th, 1861.]

The subjects of experiments were dogs, cats, and rabbits. They were drowned in water varying in temperature from 40° to 50° Fahrenheit, and in one instance 56°. On being removed from the water after every external symptom of life had disappeared, they were opened by the removal of the anterior part of the chest, so that the movement of the heart could be observed . . . .
Thirteen experiments were performed, twelve on rabbits, one on a cat. Of the thirteen, seven were put into the hot bath ; of these six died at periods varying from two to twenty hours after submersion. Six animals were left to themselves ; of these four recovered and two died, both between the eighth and twentieth hour after submersion.

The author believes that the best method of performing artificial respiration we are acquainted with is that recommended by Dr. Marshall Hall.

Dr. Babington was not sure whether experiments upon dogs respecting warm baths were applicable to human beings. The warm bath would probably be more injurious to asphyxiated dogs from the fact that the skin of the dog was remarkably thick, and it was known that he did not perspire. He (Dr. Babington) did not know whether cats were subject to perspiration.

Dr. Waters in reply said that of the experiments he had detailed some were performed about four years ago. The attention of the profession was at that period directed to the subject by the late Dr. Marshall Hall. He (the author) at that time brought the results of his experiments in reference to the hot bath before the committee of the Liverpool Royal Humane Society. This society had previously adopted the rules of the Royal Humane Society of London. The committee of the Liverpool Royal Humane Society referred his plan to the Liverpool Medical Society for their opinion. The result was that the plan was recommended, and it was therefore adopted. Unfortunately no record whatever had been kept of the cases thus treated, so that no practical test of the working of the method was attainable. . . . . The experiments of Mr. Erichsen and those performed by himself tended to prove that the heart contracted for some time after complete asphyxia.—*Dr. Waters, Lancet*, No. 1,969, p. 513 and 514.

[After all these valuable experiments the learned Doctor comes to the following honest, sensible, conclusion.] .

. . . .

In considering the question of the deviation of the heart's beat in asphyxia, and the possibility of restoring animation in the affection, it is very desirable that if we err we should err on the right side. It is better that we should make fifty ineffectual attempts to save life, acting on the supposition of the prolonged duration of the heart's beat, than that we should suffer one life to be lost by allowing the opposite assumption to paralyse our efforts.—*Dr. Waters, Lancet*, No. 1,977, p. 60.

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B.

EVIDENCE OF PROLONGED PAIN.

[Some of the following show pain only for a short time, but they could not well be eliminated from the series in which they appear.]

This pamphlet contains the results obtained by burning and scalding about thirty dogs, in regard to, 1st, the local temperature produced by burning certain substances upon the surface of the body; 2nd, the manner in which this local increase of temperature extends over neighbouring parts, and the mode in which this increased temperature dies off; 3rd, the histological alterations produced locally and generally by burns and scalds. Medium sized dogs alone were made use of. Some of these were narcotised by the injection of half a drachm of tincture of opium into the crural vein, and others by chloroform inhalations. The latter method was employed when the blood was to be examined, the former in other cases. The burns were produced by sponging the chest and bellies of the dogs with oil of turpentine five or ten times in quick succession, setting fire to it each time, the scalds by pouring over similar parts eight ounces of boiling water nine times in quick succession.

The results obtained were:—1st, all the dogs died either in a few hours or at the latest after five days.

2nd. Excision of a portion of skin corresponding in position and extent to that burned, had no injurious effect on three dogs on which it was performed. For the first few days the wound was covered with sponge, no attention was subsequently paid them, and the wounds healed most kindly.

3rd. In three cases the burned portion of skin was *excised* two, five, and fourteen hours after the burning. All the three dogs died 24 hours after the burning.—*Edinburgh Medical Journal,* 1868–9, p. 1026.

To a third I gave 193 drops of spirits of wine within the hour, but warded off immediate death by means of strychnine. The animal died in six weeks. On making the post-mortem examination, I found slight congestion of the dura mater, but severe congestion of the liver, which was black, and literally rotten. The intestine was also severely congested, doubtless in a great measure from the action of the strychnine. In this case a large amount of alcohol was kept in the blood of the rabbit by artificial means (equal to seven to eight pints in an ordinary sized man). The result was, that there was not oxygen available to the extent required for its rapid combustion and elimination; hence the severe mischief set up by its presence.—*Mr. Lucas, British Medical Journal,* No. 724, p. 612.

Good results are yielded more easily by a feverish than by a healthy animal. For these experiments strong guinea-pigs, rabbits, or dogs of the same origin and of the same quality have been used. Under their skin some cubic centimeter of ichor or putrifying blood was injected. After thus proceeding, the temperature of the animal rises several degrees, and all the symptoms appear which are to be observed in human beings suffering from putrid fever. If the quality of the poisonous substance be right the animal expires in a few days.—*Nature,* 216, p. 132.

The author, Dr. Moritz Roth Greipwold, could not succeed in producing ulcers by tying or occluding vessels, but in a number of rabbits he produced ulcers, closely resembling the ordinary perforating ulcers, by administering a small fragment of lunar caustic, the ulcers were mostly along the lesser curvature and the back wall. When examined within a few days of the administration of the dose the surrounding mucous membrane was found swollen and inflamed.—*Edinburgh Medical Journal,* Vol. 18. p. 951.

Physiologists have been accustomed to examine the action of the heart in cold-blooded animals to determine the laws that regulate its movements. Dr. Hope performed a number of experiments on frogs and on turtles, to perceive the manner in which the auricle and ventricle contract and dilate; and Professor Muller, of Berlin, from the observations he made on the movement of the heart in frogs, states a general law respecting the rhythm of the heart. Oesterreicher placed a body on the heart of a frog heavy enough to press it flat, but sufficiently small to allow the heart to be observed, and it was seen that the body was lifted during the contraction of the heart, but that during its extension it remained flat; . . and Dr. John Reid endorses the opinion as a legitimate deduction applicable to warm-blooded animals, and physiologists still continue to pursue their investigations in the action of the heart in cold-blooded animals. . . .

Page 412. We consider then that we have proceeded on sound physiological principles in the series of experiments that we instituted on the denuded heart of turtles during the highest temperature of the season, to determine the cause of the sounds of the heart; and we are decidedly of opinion that these animals afford a much better field for investigating the action of the heart and arriving at a correct knowledge of the cause of the sounds than is obtained from the denuded heart of warm-blooded animals. In the warm-blooded, as in the dog and ass, the operation of laying open the thorax and denuding the heart produces a great shock on the system, and the fact that you require to maintain artificial respiration to continue your investigations for any length of time, interferes materially with the action of the heart, rendering it weak and irregular, and the sounds indistinctly heard. But in turtles the effect of the operation on the action of the denuded heart appears but slight.

If the temperature of the body be high the heart continues to pulsate with energy and in a normal and regular manner after being exposed, and the *animal* will *survive* for *several days,* affording an ample field to observe true action and investigate the cause of the sounds.—*Dr. Paton, Edinburgh Medical Journal,* 1873–4, pp. 407–12.

Who would have supposed that a rat's tail after removal of the skin might be kept in a glass tube for 62 hours at 15–17° Fahr.; or kept for a still longer period in moist air at 121° Fahr.; or after being subjected to a temperature 31° below the freezing point; or, finally, after being dried in an air-pump over sulphuric acid, and enclosed in a glass tube for three days, then exposed to a temperature of 175° Fahr. in a hot air chamber, and again enclosed in a glass tube for four days,—who would have supposed that the unfortunate tail might be subjected to such a treatment and yet live on its being placed below the skin of the back of a rat. These remarkable facts regarding the vitality of the tissues have been ascertained.

. . . .

I had an opportunity of witnessing the results of two such experiments. [Amputating tails of animals and afterwards engrafting them on other animals.] In one the revived tail had been frozen; in the other it had been kept in moist air for three days at 121° Fahr.; on the animals being injected it was found that there was free vascular communication between the engrafted tail and the surrounding tissues. He moreover finds that the tissues which have been subjected to such modifying influences are liable to fall into certain diseased conditions, the progress of which may be traced by killing the animal at different stages. In the prosecution of this research he is still engaged. He has also succeeded in joining together animals, not only of the same, but of different species, not not only rats to rats, but actually a rat to a cat. He effected this by denuding corresponding parts of their sides, and then uniting by means of sutures the skin of the one animal to that of the other, and tying the two animals together so as to prevent their tearing themselves apart. The practical importance of such researches does not require to be dwelt upon.—*Dr. Rutherford, Journal of Anatomy and Physiology,* 1867, p. 163.

His investigations have extended over many years; and he has experimented upon cats, rabbits, crows, fowls, pigeons, and various small birds. Of all these he finds young cats the best adapted for research, as the nerve trunks in those animals contain very little connective tissue in their interior. The mode of operating consisted either in dividing the nerve with the knife, or in applying a fine silk ligature for a few moments, which is almost equally efficacious in cutting through the axis cylinders. The changes induced were examined at various intervals of time ranging from two hours to six months, and both in the fresh state and after maceration in different preserving fluids.—*Lancet*, No. 2,560, p. 421.

Vulpian removed completely this ganglion in dogs, on the left side. At the end of from ten to fifteen days the animals were curarised and artificial respiration kept up. The skin and subjacent tissues of different parts of the body were stimulated by strong induced currents. Each time the pupil on the left side dilated a little from the quarter to a third of its radius.—*Journal of Anatomy and Physiology*, 1873, p. 398.

After extirpating the superior cervical ganglion in a dog, it was curarised after the lapse of from 10 to 15 days. Artificial respiration was kept up, and the skin of the abdomen and hinder extremities were stimulated with strong induced currents, each time both the pupils became dilated, even that corresponding to the side operated on, and which is inervated from the upper cervical ganglion (a part of the sympathetic was destroyed at the time of extirpation).—*Journal of Anatomy and Physiology*, November 1874, p. 214.

A. Billder excised a piece (1·5 c. m. in length) of the left cervical sympathetic from a half-grown rabbit.

. . . .

In about a month the rabbit had grown just as the other rabbits, and appeared quite sound. The left pupil was only half as large as the right, and the left eyeball projected much less from the orbital cavity than the right. The left ear was distinctly broader and larger than the right, and was more hyperæmic and warmer.

In a fortnight later the difference in size was more striking.—*Journal of Anatomy and Physiology*, November 1874, p. 214.

Dr. Hjalmar Heiberg's (of Christiania) paper on the regeneration of the corneal epithelium, which leads off the serial, contains several points. . . . .

His mode of procedure was to scratch the surface of the cornea with a cataract needle in animals (frogs, birds, rats,) and after the lapse of from 18 to 40 hours to remove the eye.—*Lancet*, No. 2,501, p. 197.

If one continues to administer a daily dose of alcohol sufficient to bring on intoxication, one remarks in the dog from about the fifteenth day a nervous excitability of quite peculiar character. The animal is melancholy and uneasy; he listens, the least noise makes him start; when the door is opened, seized with fright he runs and crouches in the darkest corner of the room; he no longer responds when patted, he runs away and tries to bite when one attempts to take hold of him, and utters sharp cries at the mere threat of blows. This irritable and timid condition increases each day, and from the end of the first month, illusions and hallucinations becoming added to it, it is transformed into a veritable delirium. In the middle of the night he utters plaintive moans, or even whilst all is quiet he begins to bark, the cries becoming louder and more frequent as if an enemy were approaching; speaking or calling does not reassure him, one must interfere with a light. At last, during the day he growls without cause; then, thinking that he is pursued, he cries out, runs scared hither and thither, with his head turned back and snapping in the air.—*Dr. Magnan, The Lancet*, No. 2,664, p. 411.

What we see in the dog, in some cases, after intravenous, subcutaneous, or stomachal injections of essence of absinthe is as follows:—In the interval between two epileptic attacks, and sometimes before the convulsive symptoms, or even without convulsions, the animal is seized with an attack of delirium. All of a sudden he erects himself on his paws, the hair bristles, the look becomes wild, the eyes injected and brilliant, staring at some particular spot where there is nothing apparent to draw his attention; he barks furiously, advances and retires as before an enemy, with open mouth he throws his head suddenly forwards, and immediately shuts his jaws and shakes them from side to side, as if he wished to tear his prey in pieces. This attack of delirium may recur several times; then the effects pass off, and the animal becomes quite calm.—*Dr. Magnan, Lancet*, No. 2,664, p. 411.

At a recent séance of the Académie des Sciences, however, Mr. Ranvier adduced certain experiments, which, if they do not absolutely disprove the ordinarily received views, at least are strongly suggestive of the suspicion with which we should regard all traditional dogmas, however high the authority by which they are supported.

The views above mentioned seem to date from the experiments made by our countryman Richard Lower, who, in his "Essay on the Heart and on the Colour and Movement of the Blood," first showed that tying the vena cava was followed by ascites, and ligature of the jugular veins by œdema of the head, with copious flow of saliva and tears, resembling, as he says, the salivation produced by mercury, terminating in two days in suffocation. . . .

Mr. Ranvier, however, appears to have been dissatisfied with the accepted views on the subject, and proceeded to repeat the second experiment of Lower. He tied the two jugular veins at the inferior part of the neck in a dog and in a rabbit. To his surprise, however, these animals presented no discharge of tears, no salivation, nor any œdema of the head. In other experiments he ligatured the femoral vein immediately below the crural ring in the dog; but here again no œdema occurred either on the day of operation or at any subsequent period. These results consequently were in accordance with those observed by Hodgson in man. Lastly, he applied the ligature to the inferior vena cava, but still no œdema occurred. He then conceived the idea of favouring the production of dropsy by paralysing the vaso-motor nerves, and recalling the experiments and observations of M. Claude Bernard he divided the sciatic nerve on one side in a dog whose vena cava inferior had previously been tied. On this side a considerable degree of œdema immediately supervened, whilst the opposite hind limb remained in its ordinary condition.

This remarkable experiment was performed three times, and on each occasion with the same results. From these experiments M. Ranvier believes that he is justified in concluding that mere ligature of the veins does not in the dog at least produce œdema, but that after obliteration of the veins, dropsy may be caused by section of the vaso-motor nerves.—*Lancet*, No. 2,428, page 383.

Dr. Tschaussow has lately made some observations of interest on the inflammatory process as observed after ligature of the artery supplying the inflamed part, and his results are recorded in a recent number of the Centralblatt. He remarks that, under ordinary circumstances, the several stages of an acute inflammation proceed with such rapidity that it is difficult, if not impossible, to define their limits. It was therefore suggested by Dr. Samuel that means should be adopted by which the progress of the inflammation might be retarded, and to this end it was practicable either to apply cold or to ligature the artery supplying the part with blood. These suggestions were carried out by Dr. Samuel himself to a certain extent. The part investigated was, for the sake of convenience, the ear of the rabbit, and in this inflammation was established, after ligature of the common carotid or auricular arteries, by the rubbing in of croton oil. Dr. Samuel found that in the first stage (within twenty-four hours) of acute inflammation there first occurred retardation, and subsequently arrest, of the venous circulation; white "vesicle" appeared in the veins. Then followed a second stage, characterised by congestion of the arteries, which previously presented no visible change whatever. This was followed by intense arterial congestion with exudation, cloudiness, and swelling of the tissue. By means of this secondary congestion the arrest of the circulation was overcome, or, if not overcome, the death of the part was the result.—*Lancet*, 1869 No. 2,416, p. 849.

Ablation of the cerebellum is therefore equivalent, for a time, to extensive irritation of the organ, but this gradually subsides, and in those that long survive the effects of the operation may wholly disappear. The only permanent change Mr. Mitchell thinks he has seen is, that in all the birds there is an incapability of prolonged exertion, the animals tiring much sooner than their uninjured fellows, but beyond this, no locomotor defect, no alteration either in sensibility or in the sphere of emotional activities is perceptible. Vomiting is an occasional but transitory symptom. Diarrhœa commonly follows ablation, and persists for a week or more.

After having concluded these investigations, Dr. Mitchell examined the effects of freezing the various parts of the nervous system, and found that complete refrigeration of the cervical region of birds induced asphyxia by paralysis of the respiratory nerves, but mere chilling produced gasping respiration, convulsions, backward movements, and other symptoms which strongly reminded him of the effects of cerebellar lesions. Freezing the cerebellum induced precisely similar symptoms, and this he attributes to the congestion which cold secondarily occasions acting as an irritant along the plane of junction of the frozen with the unaffected parts, and this view is supported by the fact that direct irritation of the cervical spinal cord with capsicum occasions the backward movements after some hours, the effect being also rather persistent. Dr. Mitchell made the remarkable discovery, that cold applied suddenly to definite tracts of skins in pigeons gives rise to exactly the same retrogressive movements as when the correspondent spinal regions are frozen, and when the right or left side of the crop was frozen, the pigeon walked to the side opposite to that frozen. But a still more curious point remains to be noticed, namely, that in animals from whom the cerebellum had been removed the effects of congelation and of irritants were still the same, the birds being yet capable of exhibiting in perfection the retrogressive acts, convulsions, and lateral walking.—*Lancet*, 1869, No. 2,393, p. 57.

In a memoir recently published by Herr Dr. Zalesky, this physiologist records some experiments which seem to prove that urea is found in the kidneys, and not in the blood. He removed the kidneys from various animals, and examined the blood subsequently for urea, but without finding larger traces than usual. In animals, however, in which (without removing the kidneys) he placed a ligature upon the ureters, he found the proportion of urea in the blood largely increased, as the consequence of several experiments upon birds, reptiles, and mammals.—*Lancet*, No. 2,218, p. 239.

Experiment 6.—A very large and powerful dog, in admirable condition, was subject to a rigid fast for seventy-two hours, three full days.—*Dr. Harley, Lancet, No.* 1,938, p. 388.

Dr. Harley briefly related several of the experiments he had performed, some of which lasted but twenty-five minutes, while others had extended over a period of eighty days. In the first case, one grain of common arsenic was injected into the jugular vein of a cat; in three minutes convulsions commenced, and in the short space of twenty-five minutes the animal was dead. In the case referred to where the animal lived eighty days, the dose was gradually increased from a quarter of a grain to one grain; in all forty-nine grains of the poison were taken, and at death the animal presented in a marked degree the effects of chronic poisoning.

In reference to experiments on animals the author said that he did not pretend that poison could produce exactly the same effect as in man. There was, however, probably greater similarity than was generally supposed. He thought that as a rule the mineral poisons acted on animals in the same way as on man.—*Dr. Harley, Lancet*, No. 1,995, p. 499. [*See* Langley, p., 7, Marcet, Thorowgood, and Lancet, p. 25, Yeo, p. 38, and Moore and Reynolds, p. 39.]

I have also ascertained that in frogs rendered blind these experiments give the same results.—*Dr. Brown Séquard, Lancet*, No. 1,818, p. 4.

Dr. Soutougnine (physician to the Cesarewitch) also injected the blood of a rabbit into the veins of a dog with the effect of causing bloody urine, wasting and death.—*Dr. Madge, British Medical Journal*, No. 680, p. 42.

In the first eleven cases blood of various kinds of animals was transfused, in all cases defibrinated. The blood of dogs was transfused in two cats, and in seven dogs the blood of calves was transfused, cat's blood was thrown into one dog, and dog's blood into a sheep. In two cases in dogs, death took place from twenty to twenty-two hours after transfusion of calf's blood; and the author in one case attributed the death to paralysis of the heart, whilst in the second there was œdema of the brain and lungs. In all the rest of the cases the transfusion of defibrinated blood by different species of animals was not only well borne, but it gave the animals strength, like the blood of their own species. The author found the foreign blood to be assimilated in from twenty-four to sixty hours.

In eight cases, again, the leg of a dog was amputated and great anæmia was caused. In the space of from ten to thirty minutes after the amputation defibrinised blood from calves or sheep was injected. In all cases death was the result from hæmorrhage from the stump. Of two dogs which had blood transfused into them twenty-four hours after amputation, the first outlived the operation, whilst the second died on the eleventh day of acute purulent œdema and of septicæmia. In one case transfusion of defibrinated calf's blood was made two and a half days before the operation, and it got well in twelve days.—*The Doctor*, May 1st 1874, p. 81.

From his paper we learn that Dr. G. R. Cutter has informed him of some experiments made in Arnold's laboratory, Heidelberg, on dogs, rabbits, and frogs, the retinal vessels being measured before, during, and after the administration of quinine. A gradual but marked diminution of the arteries took place. Before death they became mere threads; the veins were also diminished in diameter; the animals often became blind; and the retina was anæmic in the highest degree. Inflammatory dilatation was lessened, and the wandering of the corpuscles retarded.

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Chirone's experiments further show that quinine exercises a powerful influence over the heart. A frog poisoned with the drug is able to leap or jump a considerable time after the heart has been arrested in full diastole.—*The Doctor*, October 1st 1875, pp. 189–90.

In order to test the statement of Reitz and Oertel that true croupous inflammation can be excited by dropping ammonia into the air passages of animals, H. Meyer introduced into the air tubes of dogs and rabbits a solution containing in each 100 cubic centimetres 8·96 grammes of ammonia, eight to ten drops being used for dogs, and from three to six for rabbits. He then investigated the changes found either on killing the animals, or on their spontaneous death, which took place generally within a period varying from forty-eight to seventy-two hours. From two to five hours after the injection, the mucous membrane became swollen and red, and studded with numerous ecchymoses; colourless blood corpuscles lay around the vessels. From the surface, a fine membrane could be raised, consisting essentially of epithelial cells; under it, the basement membrane lay exposed. The epithelial cells had undergone very various changes of form, being partly cup-shaped, with an opaque granular protoplasm, without cilia; most of them, however, were reduced to fragments. Along with them were a few pus corpuscles and a few fungous growths of the most various forms. The whole was imbedded in a mass of mucus which, under the action of alcohol, formed a fine network or ran into nuclei. The trachea was filled with bloody mucus as far as the larger bronchial tubes; the mucous membrane of the larynx was pale and œdematous. After ten hours the frothy secretion was replaced by a muco-purulent one, becoming purely purulent in the deeper part; the false membrane was yellower, and consisted almost exclusively of round cells, nucleated adipose cells, free nuclei, fat nuclei, cryptogamic growths, and mucus. Under the action of hardening materials, the latter formed a delicate trellis-work, which, with its enclosed cells, closely resembled the network present in croup. The mucous membrane was enormously hypertrophied, and presented an increased infiltration of proliferating cells. The mucous membrane of the larynx was pale and remarkably swollen; the epithelium was in part fatty. If the animals died or were killed when the disease was at its highest point, the air tubes were found to be lined with a nearly tubular membrane, from half a centimetre to a millimetre in thickness, extending nearly as far as the bifurcation, and ending here in a dirty yellow puriform mass.—*British Medical Journal*, No. 707, p. 82.

Dr. Chrzonzewski details some experiments in the Wiener Med. Wochenschrift. The anus and urethra being covered up, and in thick-haired animals the skin shaved, in a two per cent. solution of muriate of morphia, the animal died in eighteen to twenty hours; in one per cent. of strychnia, in two and a half to four hours; in one per cent. of nicotine, in one to one and a half hours; in two per cent. of cyanide of potassium at 2° C., in a half to one third of an hour; more quickly at a higher temperature . . . . .

In order to test the method of absorption such experiments as the following were instituted:—Ferro-cyanide of potassium was injected into the vein of a dog, and the animal placed in a bath containing an iron salt, in three to five hours the veins and capillaries of the skin were stained an intense blue, the cellular tissue remaining colourless.—*The Doctor*, April 1st, 1872, p. 70.

The cause of death after the skin of animals has been covered with varnish has been the subject of much discussion. It has been ascribed to asphyxia, reduction of temperature, retention of perspiration, &c. Dr. Feinberg has repeated the experiments, and he considers that the symptoms are due to a general dilatation of the entire vessels of the body.—*The Doctor*, February 1st, 1872, p. 38.

Dr. P[illegible] experimented on rabbits by suppressing the function of the skin by painting them partly over either with oil, gum, varnish, &c., or producing inflammation of the skin by croton oil and turpentine. He experimented on thirteen rabbits. Two of them died after forty hours, four in the first twenty-four hours.—*Mr. Purdon, Doctor*, February 1st, 1872, p. 39.

The Lancet, No. 2,713, contains an article approving experiments made by Dr. Sokoloff on rabbits and dogs by painting their skins, after these had been denuded of hair, with substances to prevent transudation of moisture.

Various varnishes were employed, but it was found that the most satisfactory application was a thick oil. A comparison of a considerable number of experiments showed that the effect on the internal temperature varied according to the area of the skin on which the application was made. If the application was a very partial one, during the first days there was either a slight elevation of temperature (in the dog) or a slight fall (in the rabbit) but the deviation from the normal was never considerable. Subsequently, there was a slight rise, followed by a fall, and simultaneously a series of symptoms indicated the commencement of serious organic mischief, general weakness, loss of appetite, evidence of weakened heart, dyspnæa, &c., and these symptoms continued till death.—*Lancet*, No. 2,713, p. 318.

He has thus removed from rats and dogs, after opening the abdomen, a part of the liver by means of a galvano-caustic knife. By proceeding slowly there was no bleeding, and the animals completely recovered.

The autopsy of these animals was made three weeks after the operation. We found in a rat from which a notable part of the liver had been removed, and which had not presented any symptom of jaundice, the liver quite healthy and in the part cut numerous and strong adhesions to the stomach and a part of the intestines.

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We also removed a portion of the kidney from two dogs. In one we cut the kidney through almost its entire length, opening up the pelves. It was impossible quite to obliterate these, and as the urine dropped into the peritoneum the animal died in forty-eight hours. In the other dog we made a very deep cut in the cortical part, without opening the pelvis. This dog lived ten days without presenting symptoms of gravity, on the tenth day it fell sick and died rapidly.

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With other ideas, but always in order to show the innocuity of galvano-cautery, we pierced right through the thorax of a guinea pig with a large needle, and at once cauterised this wound in the lung by means of a platinum thread made red hot by the galvanic current.—*Dr. Onimus' Lectures, The Doctor*, June 1st 1874, p. 104.

When rabbits are starved, glycogen disappears from the liver. In such rabbits puncture of the fourth ventricle does not produce diabetes. After a few injections of cane sugar into the stomach of starved rabbits, glycogen reappears in the liver. Injections of water, albumen, or fat, have not this effect. If the fourth ventricle be punctured before the injection, no glycogen appears in the liver, and no sugar is found in the urine. Poisoning by woorara produces diabetes in starved rabbits, although puncture of the fourth ventricle does not. After poisoning by woorara, injections of sugar into the stomach do not produce glycogen in the liver, but sugar is abundantly found in the urine.—*Dr. T. L. Brunton, British Medical Journal*, No. 686, p. 222.

Dr. Pavy, in his lectures on diabetes, mentioned that, when he divided the superior cervical ganglion of the sympathetic, he could, at will, produce diabetes in the lower animals, but that he found it impossible to keep the subjects of the operation under observation for any length of time, because they all died in a few days with pleurisy and pneumonia. The same lesion which produced the diabetes, caused also the inflammatory affections of the lung.—*Mr. Moon, British Medical Journal*, No. 691, p. 428.

Dr. Hughes Bennett (of Edinburgh) contributed some "New Investigations to determine the amount of the Bile secreted by the Liver, and how far this is influenced by Mecurials." He stated that although much had been written regarding the functions of the liver, and the action of mercurials upon it, very little exact information existed on the subject. Last winter a committee was formed in Edinburgh to re-investigate the amount of bile secreted in health, and after the administration of mercurials. This committee was composed of Professors Christison and Maclagan, Drs. Rogers, Rutherford, Gamgee, Frazer, and Professor Bennett, the chairman and reporter. After studying all that had been previously published by authors, the committee made further experiments on dogs, animals best suited for the purpose. The results of four series of these experiments were given as to the amount of bile secreted, with and without mercury. In each case the weight of the animal was taken, a biliary fistula formed, the amount of food taken and analysed, and the secretion of bile for twenty-four hours measured.—*The Lancet*, No. 2,299, p. 375.

Of these, the mercury report of Dr. Hughes Bennett, which had also been brought forward at Oxford, and was now again submitted to medical criticism, was perhaps the most interesting. . . . .
And we are informed that there was an indisputably superior weight of opinion in favour of the conclusions expressed in the report, namely, that in dogs mercury is entirely incapable of increasing the secretion of bile, and that the analogy is sufficiently good to allow of our concluding the same of its operation in man. Now, in commenting on this conclusion, and the various arguments which have been urged against it at Norwich and elsewhere, we must in the first place express our entire conviction that the experiments themselves were performed with an amount of care and labour which renders them perfectly reliable, and for which Drs. A. Gamgee and Fraser deserve the warm thanks of the profession. It seems to us established beyond a doubt that mercury never increases, and that in large doses it seriously diminishes, the flow of bile in healthy dogs. And in spite of the objections which have been made about the infeasibility of arguments drawn from experiments on dogs (objections which are chiefly put forward by persons who are not familiar with experimentation on animals, as Dr. Gamgee rightly observed), we consider that the inference of a similar ineffectiveness of mercury as a cholagogue to healthy human beings is quite legitimate.—*Lancet*, No. 2,348, p. 285.

Professor Bennett read a "Report on the action of Mercury on the secretion of bile," in which he reiterated the statements made at the meeting of the British Medical Association at Oxford, viz., that in whatever doses mercury may be administered it fails to influence the secretion of the bile; in fact it diminishes it. An animated discussion followed, in which Dr. Crisp called in question the accuracy of the Committee's facts, because the dog was not at all like man in the general formation of his alimentary canal. In their wild state dogs are carnivorous, in domestication they become omnivorous. He agreed with Mr. Flower that the pig would be a better animal for experiment than the dog. He thought we were bound to analyse with great care the nature of these experiments, and should not hurriedly abandon our long entertained views regarding mercury.—*Lancet*, No. 2,348, p. 292.

Professor Gross, of Philadelphia, said that whilst acknowledging the accuracy of the facts, he was yet not disposed to ignore the action of mercury. An experiment on a dog was one thing, but a careful observation on the human frame performed at the bedside was another and wholly different one.—*Lancet*, No. 2,348 p. 292. [*See also* p. 38.]

I have found that, in cats, the glycogen disappears from the liver within a few days after the bile-ducts have been tied; and also that, on the fifth or sixth day after ligature of these ducts, irritation of the fourth ventricle is not followed by the appearance of sugar in the urine. It would appear, therefore, that within a few days after complete obstruction to the gall-ducts, the liver ceases to secrete glycogen.—*Dr. Wickham Legg, British Medical Journal*, No. 698, p. 646.

App. IV.

Schiff found that diabetes could be produced by division of the anterior columns of the spinal cord between the medulla and the fourth cervical vertebra. This lasted for days or weeks, in fact till the animal died.—*Dr. Brunton, British Medical Journal*, No. 680, p. 40.

During the past winter I have made several observations upon the changes which follow ligature of the bile ducts in animals. The animals used were cats; these seem to survive the operation better than dogs. Most observers find that dogs live only five to ten days after. The way in which the ligature was applied was as follows:—The animal was first secured in a Czermak's holder, and chloroform given largely so as to secure a deep narcosis. This latter point is worthy of attention, for, unless the animal be completely under the influence of chloroform, the bowels are apt to prolapse, and interfere with the success of the operation. Cats, it is well known, require a large amount of chloroform. A cut is then made through the linea alba, from the xiphoid cartilage downwards, for about two inches. Unless the animal be very large and fat a longer cut is unnecessary. I have never made any longer than two and a half inches. Pushing aside to the left the stomach and duodenum, and raising the free edge of the liver, the bile ducts are seen coming from the liver and gall bladder. They much resemble a vein in appearance, but they will be known by their insertion into the duodenum, bifurcation, and connexion with the gall bladder. A ligature is then put around the common duct and tied close to the duodenum; another is tied tightly on the duct, about half an inch nearer the liver, and the duct between the two ligatures divided by a pair of scissors, the vessel being held out from the portal vein for that purpose. In two of the cats the bile found its way again into the intestines; in the latter operations, therefore, I removed altogether about half an inch of the common duct, as is done in making biliary fistulæ. The belly walls were then brought together with ordinary sutures. It is well to place these close together, as I lost three of the cats from the giving way of the sutures and consequent prolapse of the bowels.

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Had I to repeat these experiments I should choose only young, not fully grown, animals, and a warm time of year. Though the cats were kept in a warm place, and the January of this year was mild, yet three of them were found dead one morning after a slight frost in the night.

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Experiment I., January 24th, 1873.—Large tabby cat weighing $8\frac{1}{2}$ lbs. (3,855 grm.), very fat; bile ducts tied double and cut. The cat died probably on January 26th.

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Experiment II., January 24th, 1873.—Black she cat weighing 6 lbs. $6\frac{3}{4}$ ozs. (2,757 grm.) Bile duct tied double, but not cut. Animal pregnant. The cat died on January 26th, examined January 28th.

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Experiment III., February 3rd, 1873.—Large tabby cat weighing, immediately after operation, 7 lbs. $3\frac{1}{2}$ oz. (3,265 grm.) Animal very fat; bile duct tied, but not cut. February 6th.—Cat seems to be dying; it is unable to stand, but lies on its side, mewing. February 7th.—Found dead at 11 a.m. in same place where left yesterday.

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Experiment IV., January 3rd, 1873.—A young cat. The common bile duct tied twice and divided. Cat found dead on morning of 7th. Cause of death prolapse of bowels.

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Experiment V., January 7th, 1873.—White cat with yellow brown markings, very old, only one canine tooth, weighing $6\frac{1}{4}$ lbs. Bile duct tied double and cut. January 9th, cat very feeble; no suppuration of wound. Cat found dead at 10.30 a.m. on January 11th.

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Experiment VI., February 3rd.—A cat not fully grown; very wild; scarcely any fat on body; weighing, immediately after the operation, 3 lbs. 5 oz. (1,501 grm.) Bile ducts tied double, but not cut. The animal nearly died under the chloroform, but recovered with artificial respiration. Cat last seen alive on February 7th; no jaundice of conjunctiva. Found dead on morning of February 12th, and already much decomposed. Cause of death, prolapse of bowels.

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Experiment VII., January 6th.—A whitish cat, with grey spots, weighing $5\frac{3}{4}$ lbs. (2,598 grm.) Bile duct tied double and cut. January 8th.—Cat feeble, belly wound suppurating. January 10th.—Less suppuration; cat livelier. January 13th.—Cat found dead in morning, with bowels prolapsed. Examined immediately; weight $4\frac{1}{4}$ lbs. (1,917 grm.)

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Experiment VIII., February 12th.—A tabby cat, weighing 6 lbs. $11\frac{1}{2}$ ozs. (3,060 grm.) The bile duct tied close to the duodenum and above, so that about seven or eight millimeters were cut out. The cat was found dying at 10.30 a.m. on February 19th. Examined immediately; weight 5 lbs. $13\frac{1}{2}$ oz. (2,620 grm.)

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Experiment IX., February 12th.—A very large tabby cat, grey and black, weighing 7 lbs. (3,175 grm.) Very little fat on body. The bile ducts tied double, and about four or five millimeters of the duct between the ligatures cut out. In opening the belly the liver was pricked; there was free bleeding, which presently stopped of itself. The animal is said to have died on February 21st. It was examined on February 22nd, weight 5 lbs. $2\frac{1}{2}$ oz. (2,325 grm.)

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Experiment X., June 10th.—Black and white cat. Common duct tied double, and about five millimeters of the duct cut away. Cat found dead on morning of June 23rd. No jaundice of lips and mouth.

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Experiment XI., January 9th.—Black tom cat, weighing 6 lbs. $1\frac{1}{4}$ oz. (2,725 grm.) Bile duct tied double and cut. January 22nd.—The conjunctivæ are now noticed for the first time to be yellow. The cat is growing thinner every day, but has eaten very little since the operation. The wound looks well, and there is no suppuration. January 25th.—This day all the remaining stitches taken out of wound; it is firmly united; weight of cat 3 lbs. $7\frac{1}{2}$ oz. (1,040 grm.) The cat died at 10 p.m.

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Experiment XII., January 24th.—Black cat, weighing immediately after the operation 7lbs. $5\frac{1}{2}$ oz. (3,348 grm.) Bile duct tied double and cut. February 3rd.—A brown yellow tinge now visible on conjunctivæ; most of the stitches taken out of the wound; the cat eats well. February 6th.—Jaundice increasing; wound nearly firmly united; weight 6 lbs. 10 oz.; appetite keeps good. February 7th.—Jaundice still increasing; cat is now very feeble, and when tumbled over has great difficulty in regaining its feet. February 10th.—Jaundice extremely intense. February 12th.—Cat dying; weighs 4 lbs. 7 oz. (1,995 grm.); was left alive at 5 o'clock in the evening. February 13th.—Found dead this morning at 10.

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Experiment XIII., January 11th, 1873.—A black and white cat; the bile ducts tied double and cut; this cat never became jaundiced; it was killed on February 6th, by cutting off the head.

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Experiment XIV., January 7th.—A brown yellow cat; weight 4 lbs. $15\frac{3}{4}$ oz.; bile duct tied double, but not cut. January 9th.—No jaundice, but cat very feeble. January 18th.—The conjunctivæ are now slightly jaundiced. January 22nd.—Yellowness of conjunctivæ still more marked; edges of wound in belly firmly grown together; all the stitches taken out to-day. February 3rd.—Yellowness growing less; belly wound quite healed. February 5th.—No yellowness can any longer be seen in the conjunctivæ; the belly wound can scarcely be made out, it is so firmly cicatrised; weight 4 lbs. 12 oz.; fæces dark coloured, and urine gives no green re-action with nitric acid. It was determined to kill the animal; the head was therefore cut off.

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Experiment XV., June 10th.—A tortoiseshell cat, not fully grown. The left branch of the hepatis tied once only. The cat never became jaundiced; it ate immensely, and yet it lost flesh daily. It died in the night, between June 27th and 28th.

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Experiment XVI., June 27th.—Black and white cat, well nourished, full grown. Bile duct tied double, and piece cut out. July 6th.—As the cat was now very weak, and seemed about to die, it was determined to make the diabetic puncture. The cat was therefore laid prone, a cut made through the skin, over the occipital protuberance, and the chisel applied immediately underneath this. After dividing the occipital bone the chisel was passed in a direction downwards and forwards, so as to cut the line made by joining the two auditory meatus. The chisel was pushed on until it met with the basilar bone, and was then withdrawn. Operation was over at 12.30. Before the operation the cat had languidly taken a little milk; urine passed during the operation, though highly jaundiced, gave no re-action with Trommer's test. At 2:15 urine pressed out of bladder, likewise gave no re-action with Trommer's test. July 4th.—Cat still alive; urine gave no re-action with Trommer's or Moore's test. July 7th.—The cat died in the night, between July 5th and 6th. Examined to-day at 2 o'clock. Much more peritonitis than in any other of the experiments.

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In all these cases it may be remarked that the jaundice of conjunctivæ was very close to declare itself. In none was it noticed until the tenth day after the operation; in one it did not show itself until the fourteenth. The cats were daily examined for this appearance. On the other hand, Frerichs asserts that a yellowish colour of the conjunctivæ could be noticed in sixty or seventy hours after the operation. Tiedemann, Gamelin, Leyden, and Golowin also found their dogs jaundiced on the second or third day. In Heinrich Mayer's experiments the jaundice seems to have come on later, about the same time as in my own; and it may be further noticed that he used cats, not dogs, upon which circumstances the difference between observers perhaps depends. In an old experiment by Jaunders the hepatic ducts of a dog were tied. Two hours after the dog was killed. The absorbents were found distended with a fluid of a bilious colour, and white paper dipped into the serum of blood taken from the hepatic vein gave a deeper tinge than from the jugular. I have repeated this experiment in the dog without success.

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Although the appetite in these animals remained good in most cases, yet they wasted; in one case the weight fell from 7¼lbs. to 4½ lbs. in nineteen days. The cats appeared to become weaker daily without any marked symptoms of disease.

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The cause of death in these creatures is obscure. Blondlot and many others attribute it to peritonitis. Blondlot gives a distinct cause. He says that the ligature eats through the bile duct; the bile is thus poured into the peritoneum.

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Leyden seems to think that it is the addition of the jaundice to the peritonitis which kills the animals.

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I should be far more inclined to think the cause of death to the changes which take place in the liver. The liver in these cases, as tested by iodine, contained little or no glycogen.

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Of all the functions of the liver known to us the most important is the preparation of glycogen, and this seems to pass into complete abeyance soon after ligature of the bile ducts. Glycogen is one of the most important elements of nutrition; and it is not surprising that the animals should have wasted so rapidly when the system was deprived of it. And it is to this defect in nutrition, even while the animal was taking nourishment freely, that I am inclined to attribute the fatal end.

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In [human] disease the state which most closely imitates ligature of the bile ducts is congenital obstruction of the bile ducts outside the liver. Of these cases, of which there are but few on record, there were found, in one of the more carefully examined cases, appearances which the writer calls hepatitis interstitialis; in other words, an overgrowth of the capsule of glisson. Here the same chain of events seem to take place. The change of the bile ducts into a fibrous cord influences all the connective tissue in the portal canals, and an overgrowth takes place. The nature of these changes, whether of a so-called inflammatory origin or otherwise, it is unnecessary and would be unprofitable here to discuss.—*Dr. Legg, St. Bartholomew's Hospital Reports*, Vol. IX., pp. 161–81.

Goltz ascribes the disturbance of movement produced by section of those organs, to the loss of the feeling of equilibrium . . . .

The first point investigated by Solucha was, how far is an abnormal position of the head able to disturb the feeling of equilibrium of the animal, and so to produce the abnormal movements? The author confirms the experiment of Longet that mere section of the recti capitis postici majores et minores in the dog renders the movements of the animal uncertain and insecure, the dog was unsteady on its feet, moved from side to side, kept the fore feet widely apart from each other, running was rendered difficult, &c. After five or six days the head generally assumed the normal position, and at the same time the walking became normal. In a second series of experiments the author sought to give pigeons a peculiar position of the head, without wounding important parts, a position such as occurs in section of the canals, with the beak directed upwards and the occiput towards the ground. On fixing the head to the breast in this position with a thread, the animals conducted themselves partly like those in which the horizontal as well as the vertical semicircular canals were destroyed.

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On section of one horizontal canal the animal made several lateral movements of the head, beginning from the injured side, which soon ceased. In section of the corresponding canal on the opposite side pendulum movements of the head occurred, and persisted very long. The violence of the movements increased from the beginning onwards until they reached a maximum, when the animal lost its equilibrium, fell over, executed movements de manège, &c. In a few cases the animals recovered completely, but generally after four or five days the animal was found in a corner with the peculiar position of the head above described and quite quiet, but when disturbed it resumed the pendulum movements, &c.; most of the animals died in from ten to twenty days.

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On section of all four canals, violent movement of the head, resembling a screw motion, occurred immediately, accompanied by general swinging movement of the whole body.—*Journal of Anatomy and Physiology*, 1873, p. 400.

L. Perl, in his first series of experiments (on dogs), performed seldom and large bleedings (every five or seven days, three to three and a half per cent. of the body weight at each time), in a second series more frequent and smaller bleedings (every three or four days, one to one and a half per cent.) were practised. The animals endured the operative proceedings well. The wounds healed well without fever, and only in one case did embolus of the lungs occur.

Whilst the animals of the second series, on which ten as the minimum and seventeen as the maximum of bleedings were practised, remained quite cheerful and well, and when killed, from the thirty-sixth to the thirty-seventh day, showed no signs of change in the muscles of the heart, the seven dogs of the first series, on the contrary, on which five to seven bleedings were practised, became lean, lost appetite, became sad, had partial œdema of the extremities, and died (6) with the phenomena of marasmus, within eleven weeks. With a single exception, all the animals dying after four weeks showed a very flabby heart, with a yellowish colour, and under the microscope the muscular fibres were found to have undergone extensive fatty degeneration.—*Journal of Anatomy and Physiology*, 1873, p. 407.

He ligatured the bile duct of a dog. The animal lived nineteen days, and though it continued to have a voracious appetite it emaciated visibly. The colouring matter of the bile was found four hours after the operation in large quantity in the urine, and on the second day the fæces were quite discoloured.—*Journal of Anatomy and Physiology*, 1873, p. 420.

J. W. Legg (St. Bartholomew's Hospital) operated upon cats. The animals survived the operation for varying times up to twenty days, and peritonitis when present remained local.

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The cats became emaciated and died without convulsive phenomena, and only became comatose shortly before death. With regard to the cause of death the author lays stress upon the decided diminution or absence of glycogen of the liver (tested with iodine solution).—*Journal of Anatomy and Physiology*, 1873, p. 420.

L. Seelig experimented on rabbits which had (p. 422) been allowed to hunger; diabetes was produced by Eckhard's method. The author then convinced himself that in the starving animals, after the diabetic sugar had disappeared from the urine, or occurred only in traces, corresponding to the results of Dock (the hunger period lasted three to five days, the collected urine was evacuated by pressure after it had been collected for six hours) . . . A solution of sugar (generally 20 ccm. of a 10 per cent. solution = 2 grms. sugar) was then injected into the jugular vein, in the one case into the starved animals, and in the other into the starved diabetic ones. In the former case only traces of sugar appeared in the urine when the animals had starved for

App. IV.

five, six, and seven days, somewhat more when the hunger period was shorter.—*Journal of Anatomy and Physiology*, 1873, p. 421.

A. Bidder operated on the superior epiphysial cartilage of young rabbits. The cartilage was either exposed and transfixed with needles or destroyed by section, growth of the bone was arrested either on one side or over the whole extent of the terminal surface according to the part irritated, and this effect was marked throughout the whole length of the bone as far as the distal epiphysis. Destruction of the cartilage on the fibular side was followed by growth of the opposite side, causing curvature of the bone with the convexity inwards.—*Journal of Anatomy and Physiology*, 1873, p. 425.

Dr. Burdon Sanderson delivered an address on this subject relating his first experiments as to the effect of inoculating animals with pyæmic liquids. In the autumn of 1867, he injected the purulent liquid contained in the ankle joint of a patient, who had died a few hours before with metastatic abscesses, general suppurative arthritis, and intense septicæmia, under the skin in a dog and two guinea-pigs. The two guinea-pigs died within fifteen and twenty days. Both had metastatic abscesses; in one the lungs were beset with minute nodules resembling miliary tubercles. The dog lived seven weeks, there were no secondary abscesses, but miliary tubercles of the liver and spleen. From one of the guinea-pigs, two others were inoculated; one died of pyæmic subcutaneous abscesses, without visceral disease; the other lived longer, had no abscesses, but tuberculous disease of the lungs. During the same winter other experiments were made, which seemed to show that, by the inoculation of pyæmic products, two sets of lesions might be produced; as an immediate result, metastatic abscesses, accompanied by a general typhoid state, which was often fatal; as an ulterior result, either disseminated nodules, at first hard, but afterwards becoming caseous at their centres, or interstitial in duration, both forms of lesion having their seat chiefly in the lungs, spleen, and liver, but also occurring in other viscera.—*The Doctor*, June 1872, p. 132.

Take pus—you do not want to go to decomposing vegetable or other animal fluids, you may do it with them —but take pus, and with it I could make a case of pyæmia or septicæmia according to order, by the length of time which I kept the pus before I injected it; and I know very well, in experimenting upon this subject, you may produce all degrees of the disease, and you may say that the chance of getting secondary abscesses are in direct ratio to the length of time an animal lives after it has become inoculated with the poison. When the poison is thoroughly septic, when you have that terrible substance which Dr. Burdon Sanderson has shown us how to get in the peritoneal cavity of an animal, the blood becomes so poisoned and spoilt, that it kills outright, and there is no time for the secondary effects to supervene.—*Dr. Savory, British Medical Journal*, No. 686, p, 240.

If, for example, you take perfectly fresh pus, not putrid in the least degree, pus that contains no flocculi, perfectly limpid and pure pus, and inject it into the venous system of a dog, you will get a great rise of temperature. We get shivering and malaise, and, after a certain time, the dog's recovery. You may inject a considerable quantity of pus, and you may repeat the experiment again and again, and the dog may recover. That I take to be simply pyæmia. If, on the other hand, you take pus which is no longer perfectly limpid, but a little flocculent—it need not be putrescent—which contains particles of such a size that they may stick in the vessels; and, if you inject that pus, then you get the same train of symptoms, you get abscesses, you get purulent deposits in different parts of the body, the internal viscera, and so on. In fact, you may imitate simple pyæmia and the multiple pyæmia which we get in a human subject. On the other hand, if you take putrid matter—whether vegetable or animal—for instance, if you take cabbage leaves and let them rot thoroughly in a solution, and then get this foul stinking cabbage water, if you clear away all the solid particles as far as you can, you will by these means get a train of symptoms which may be free from metastatic abscesses. You get a high temperature, you get shivering, not always, but very frequently, you get vomiting, purging, collapse, and the rapid death of the dog.—*Mr. Hulke, British Medical Journal*, No. 686, p. 238. [*See also* pp. 8 and 39.]

In the *British Medical Journal*, No. 718, and succeeding numbers, a series of 619 experiments are reported, performed by Dr. Bennett, extending over a period of four years, to show—

1. *Antagonism between Strychnia and Chloral Hydrate.*

To eighty-three rabbits and thirty-one rats were administered doses of strychnia, with the usual result of horrible torture. One of these is thus described.

Experiment 36.—On the tenth day after the former experiment one-ninetieth of a grain of strychnia was again injected into the same animal under the skin over the left loin. The animal remained quiet for five minutes, when it became restless, and moved about with a staggering gait. In two minutes more it leaped from the table, fell on its side, and had severe convulsions, with an extreme degree of opisthotonos. These convulsive attacks occurred three times, when death ensued twenty-six minutes after the administration of the poison.

2. *Antagonism between Sulphate of Atropia and Calabar Bean.*

Before commencing in this class Dr. Fraser indicated his results in the same field of experiments. Nevertheless, Dr. Bennett proceeded to operate on 112 animals, with the following shocking results.

Experiment 126.—Male rabbit, weighing 3 lbs. 8 oz. Three-fourths of a grain of extract of calabar bean dissolved in ten minims of water were injected under the skin of the back. In a minute and a half there were slight twitchings of the skin. In three minutes the breathing became very hurried, and the animal seemed to be very distressed. Saliva now accumulated profusely in the mouth. In two minutes more the animal rested on its abdomen and chest, and spread out its legs, which were stiff. It attempted to regain its natural position, but in vain. The pupils were now contracted from 7–25ths (their diameter before the experiment) to 3–25ths of an inch. Soft diffluent fæces were passed. At the end of eighteen minutes from the time of the introduction of the poison the animal was lying on its side. The respirations were much laboured. It remained in this condition for thirteen minutes more, with severe occasional muscular tremors, when it died; that is, 31 minutes after receiving the three-fourths of a grain of the extract.

3. *Antagonism between Hydrate of Chloral and Calabar Bean.*

Sixty animals in this class were tortured, their expressions being "profuse salivation," "severe tremors," "asphyxia," "very ill," "much affected," "very ill for eight hours," &c.

4. *Antagonism between Sulphate of Atropia and Meconate of Morphia.*

Notwithstanding the numerous experiments and observations which have been made on this subject, a careful investigation into the evidence which existed previously to the Committee's inquiry could not but demonstrate that nothing positive or certain had been arrived at.

One hundred and eighty-one rabbits and dogs were tortured as follows:—

Experiment 299.—Rabbit weighing 3½ lbs. Cardiac impulses 45 in ten seconds. Respirations 12 in ten seconds. Pupils measured in their transverse diameter 12–50ths of an inch. Six grains of meconate of morphia in 30 minims of water were injected subcutaneously under the skin of the back. In three minutes the animal lay on its abdomen and chest, with the hind legs extended and stiffened. The transverse diameter of the pupil was now about 11–50ths of an inch. Respirations 10 in ten seconds; cardiac impulses 42 in ten seconds. In two minutes more the animal attempted to walk, and it progressed forwards with evident difficulty, owing to the weakness of the posterior extremities. When moving a slight push was sufficient to turn the animal over on its side. In eight minutes more there were slight convulsive twitches of the muscles of the back, and the animal was now quite narcotized.

Experiment 300.—Thirty minutes after receiving the dose the respirations were reduced to six in ten seconds; the cardiac impulses fell to 38 in ten seconds, and the pupil now measured 6–50ths of an inch. The muscular twitchings had increased. After a little time there were severe spasms coming on with great suddeness, accompanied by bending backwards of the spine and pawing movements of the fore limbs. These spasms continued for nearly thirty minutes.

Experiment 307.—Twenty minutes after a dose a dog was much excited. It sat on its haunches and swayed its head from side to side. Occasionally it tried to walk, but there was evident weakness of all the limbs, more especially of the hind extremities. On placing the hand over the wall of the chest the pulsations of the heart could be distinctly seen and felt. The animal continued in this state for nearly four hours, when it began slowly to recover. It appeared to be out of health, and frequently refused food for four days after this experiment. . . . . .

A week after the preceding experiment the same dog weighed 15 lbs., and was made to take a second dose of poison. . . . .

6. *Antagonism between Tea, Coffee, Cocaine, Theine, Caffeine, and Guaranine on the one hand, and Morphia on the other.*

One hundred and seventy-six dogs, cats, rabbits, mice, and frogs were under treatment, the expressions of whose sufferings are too horrible to peruse. The following extracts are only two out of many detailed descriptions.

Experiment 430.—Six grains of theine. Cat. Death. Post-mortem examination.—Six grains of theine, dissolved in a drachm and a half of water, were injected under the skin over the back of a healthy cat weighing 4 lbs. 1 oz. In ten minutes the animal became very irritable and angry. Fifteen minutes later this excitement had increased; the animal had a watchful anxious appearance, prowled about, and when touched with a stick bit at it and growled. If any noise or motion were made it arched the back, and made a hissing noise. The legs appeared weakened, and although it could walk about it preferred sitting in a corner of the room. Its mouth and tongue were very red, and there was an abundant secretion of saliva, which constantly trickled out of its mouth. The cat defæcated and micturated several times. Forty minutes later it continued in much the same condition. Salivation was profuse. Animal suffered from tenesmus, and it had a constant straining from the bowel of a clear fluid like mucus. The limbs, especially the posterior ones, were much weakened, but the animal could still run with difficulty. It could not jump; it made attempts to do so over a bench about two feet high, but failed. The breathing was laboured and irregular. The redness of the tongue and mouth, as well as the excessive irritability of the animal, had disappeared. It was quiet, lay in a corner, stupid and drowsy. It drank freely of water. Twenty minutes later it was prostrate and lay on its side, its limbs quite helpless. It paid no attention to a pinch of the toe or a blow on the tail with a stick. It seemed, however, to be intelligent, as its eyes watched every movement of the observer, and when the hands were clapped before its face it growled. The salivation and discharge from the bowel were excessive. Pupils were contracted and the breathing was laboured. Five minutes later the cat took a series of tetanic spasms, and shortly afterwards died. . . . .

Experiment 433.—Twelve grains of theine. Rabbit. Spinal cord exposed during life.—A healthy white rabbit, weighing 2 lbs. 2 oz., was carefully fastened down on its belly. An incision was made through the skin along the upper part of the spine about two inches in length, and the vertebral column exposed. By means of bone forceps and scissors portions of the vertebræ were removed so as to expose a piece of the spinal cord about a quarter of an inch in length. On touching the posterior columns with the point of a blunt needle the animal struggled violently and uttered loud cries. . . . .

With regard to the dogs, Dr. Bennett says, "These "experiments were considered to be so unsatisfactory that "they were abandoned."

7. *The antagonism between the Extract of Calabar Bean and Strychnine.*

Twenty-four rabbits were tortured under this head.

8. *The antagonism between Bromal Hydrate and Atrophina.*

Forty rabbits were tortured under this division, Dr. Bennett closing it by saying, "The experiments were not encouraging, as all the animals died." The conclusion shows torture to 619 animals; and this is not enough, Dr. Crichton Browne says, (No. 743, *British Medical Journal*, p. 409), and he proceeds, therefore, to give picrotxine with chloral hydrate to another large number of animals, which produces in them spasms in all the muscles of the body, and causes biting of the tongue, foaming at the mouth, &c., &c.

A concise answer to the question, what is poison? really seems more difficult than ever. Men gradually habituate themselves to the use of opium, tobacco, &c., till their daily dose is sufficient to kill from two to ten of their own species. Sheep have been known to consume unwholesome plants till their flesh becomes uneatable. Goats will feed on hemlock; hedgehogs swallow almost anything; and the common toad cares little for hydrocyanic acid. Ultimately we come to the acari, which appear to enjoy a perfect immunity from the usual effects of a so-called poison; for here strychnine is only a poison in the same sense that starch would be poison to man, namely, in that it does not contain every element necessary for the reproduction of tissue.—*Lancet*, 2,015, p. 389.

Experiments on animals, already extensive and numerous, cannot be said to have advanced therapeutics much. I have seen Dr. Richardson give a pigeon enough opium to kill a strong man, and yet the bird was in no way affected; and I have heard of goats feeding on shag tobacco and rabbits on belladonna leaves without taking any harm; yet from these experiments to infer that belladonna and tobacco were innocuous to man would be a grave error. Probably calomel given to a healthy dog might cause a temporary irritation and congestive obstruction of the animal's biliary apparatus, thus showing that calomel has an action over the liver; but I cannot see my way clear to infer the action of mercury on a sick man from what we see of its action on a healthy dog.—*Dr. Thorowgood, Medical Times and Gazette*, October 5, 1872.

Dr. Marcet said if he understood rightly Dr. Harley's meaning, his interesting paper showed that small doses of arsenic continued for a long time do produce poisonous effects. A question, however, was still open to discussion, viz., how are we to account for the reported innocuous and even protective effects from the practice of arsenic-eating, which it is stated is carried on extensively in Styria? Indeed we are given to understand that in Styrian arsenic works the workmen take arsenic with a view of escaping the poisonous effects of its fumes.

. . . .

He (Dr. Marcet) observed finally that men and animals were not equally affected by poisons, a fact which it was important to bear in mind when experimenting with poisons on animals with the view of applying the results to mankind.—*Lancet*, No. 1,995, p. 499. [*See also* Langley, p. 7, Harley, p. 20, Yeo, p. 38, and Moore and Reynolds, p. 39.]

1st. If I divide the posterior column and almost the whole of the lateral column of the spinal cord, with the posterior and central parts of the grey matter in the dorsal region in a guinea-pig, I find, when the animal has become epileptic, that the irritation of the part of the face and neck which I have called epileptogenous determines reflex convulsive movements everywhere, except in the posterior limb on the side of the injury. This lack of reflex movements is not due to a paralysis of the nerves serving to voluntary movements, as, if that limb is at first a little paralysed after the operation, it soon recovers power, and has no trace of weakness by the time epileptic fits can be provoked. . . . . .

2nd. If only the posterior column and a very slight part of the grey matter, with a still slighter part of the lateral column of the spinal cord, is divided in a guinea-pig in the dorsal region, the four limbs are attacked with reflective convulsive movements when the epileptogenous zone is irritated. In this case the encephalon communicates with the posterior limb on the side of the injury for both voluntary and reflected convulsive movements.

3rd. If in another animal the lesions mentioned in the preceding experiments have been made at the same level of the cord, one on the right side the other on the left side, I find that the two sides of the face and neck acquire the epileptogenous power, and that fits can therefore be produced by the irritation of either side, but whether the right or left side be irritated, there are reflected convulsive movements only in three limbs, the posterior one on the side where the lateral column of the cord is divided remaining without the least convulsion, while the three other limbs are violently convulsed. Both lower limbs, however, remain endowed with strong voluntary movements.

4th. In animals having had a complete section of a lateral half of the spinal cord at the level of the vertebra, and having become epileptic, I have ascertained that the voluntary movements after a period of very great diminution in the posterior limb on the side of the lesion, return gradually almost to the normal condition in a variable number of months. In many guinea-pigs having recovered voluntary movements, even for a year or eighteen months, I have seen but very slight convulsions in the posterior limb on the side of the injury. Re-union of nerve-fibres serving to voluntary motor fibres can therefore take place to a very great extent in the spinal cord after having been divided, but there is hardly any re-union for the nerve fibres which in attacks of epilepsy give rise to reflected convulsive movements.—*C. E. Brown-Séquard, Lancet,* No. 2,418, p. 2.

Fortunately animals may not have any apparent diminution of either voluntary movements or sensibility after the exposure of the spinal cord to the air.—*Idem, Lancet,* No. 1,819, p. 28.

Before the operation in rabbits the most energetic pinching of the skin produces agitation but no shrieking; after the operation, on the contrary, the least pinching produces shrieking, and a much greater agitation. Sometimes the hyperæsthesia is so considerable that the least pressure upon the skin makes the animal shriek. Whether the operation is performed on the lumbar, the dorsal, or the cervical region the phenomena are always the same; that is, there is a manifest hyperæsthesia in the various parts of the body which receive their nerves from the part of the spinal cord which is behind the section. It has been so in all the animals I have operated upon, and I have already made this experiment upon animals belonging to more than 20 species.

As long as the animals live after the section of the posterior columns, hyperæsthesia continues to exist, except in the cases where re-union takes place between the two surfaces of the section; but hyperæsthesia is greater during the first week after the operation than it is after a month or many months.—*Idem, Lancet,* No. 1,819, p. 29.

In a mammal the spinal cord is laid bare at the level of the two or three last dorsal vertebræ, and a lateral half of this organ (including the posterior, the lateral, and the anterior columns, and all the grey matter, on one side) is divided transversely. The animal is left at rest for a little while, and then it is ascertained that sensibility seems to be much increased in the posterior limb on the side of the section.—*Idem, Lancet,* No. 1,820, p. 53.

To obtain a very striking result from the experiment which consists in only one section of a lateral half of the spinal cord, it is better to make it after the posterior columns have been divided. We know that after this division there is hyperæsthesia in the parts of the body which are behind the section; if, after having ascertained this fact, the section of a lateral half is completed where the posterior columns have been divided, we find that the hyperæsthesia seems to increase in the side of the second operation, while in the opposite side, not only the hyperæsthesia, but sensibility entirely disappears.

. . . .

A longitudinal section is made on the cervico-brachial enlargement of the spinal cord, so as to separate it in two lateral halves—I ascertain then that sensibility is lost in the two anterior limbs, while it remains, and even seems to be increased, in the two posterior limbs.—*Idem, Lancet,* No. 1,820, p. 54.

If the longitudinal section is more than two inches long, it is not sensitive in all its length. When there are three pairs of nerves attached to it, the one nearest to the transversal section is hardly able to give slight sensations; the next is a little more sensitive, but much less than in a normal condition; and the third is very sensitive, though not so much as the others on the same side and behind it. —*Idem, Lancet,* No. 1,820, p. 55.

If the section be made two inches higher in the dorsal region, there is, as in mammals, though less marked, an increased sensibility in the posterior limb on the side of the section, and a diminution of sensibility in the opposite limb. The loss of sensibility is never complete, showing that the decussation is not complete. The same results are obtained in reptiles.—*Idem, Lancet,* No. 1,820, p. 56.

The laying bare of the spinal cord, and its free exposition to the action of the atmosphere, instead of being a cause of loss or diminution of sensibility, as it had been said, seems to be followed by a marked increase of sensibility in the parts of the body which are behind the place where the cord is exposed.

The laying bare of the spinal cord even in mammals is very rarely followed, after a number of days, by any kind of accidents (meningitis, myelites, &c.) producing a diminution of sensibility . . . . . .

Deep injuries to the posterior columns of the spinal cord are always followed by a degree of hyperæsthesia greater than after the laying bare of the nervous centres, hyperæsthesia which appears in all the parts of the body behind the place injured.

. . . .

All the parts of the encephalon which are situated in its posterior or superior side are like the posterior columns of the spinal cord in this respect—that a marked degree of hyperæsthesia always follows a transverse section upon any of them. If a complete transverse section is made upon any part of the restiform bodies, sensibility becomes very much increased in every part of the trunk and limbs. Hyperæsthesia is also but at a less degree, one of the results of a transversal incission in the cerebellum, in the processus cerebelli ad testes, and in the tubercula quadrigemina. . . . . Every small portion of a transverse section of the conducting zone, in a lateral half of the spinal cord, contains conductors—of sensitive impressions coming from all the points of the body, on the opposite side, which are behind the place of this small portion.—*Idem, Lancet,* No, 1,823, p. 137.

I have found that a convulsive affection very much resembling epilepsy may be produced in animals. A few weeks after certain injuries to the spinal cord, in the dorsal or lumbar region, especially in guinea-pigs, fits appear spontaneously several times a day, or, at least, once every two or three days. But the most interesting point is, that it is possible to produce a fit when we choose, by simply pinching a part of the skin. These fits consist in clonic convulsions of almost all the muscles of the head, the trunk, and the limbs, except those muscles which are paralysed . . . .

I have ascertained that one part only of the skin has the power of producing the fit, and this part is that which covers the angle of the lower jaw, and extends from thence to the eye, the ear, and nearly to the shoulder. It is only the skin that has the power of generating the fit, as even the three nerves that send filaments to this part of the skin can be irritated without the occurrence of convulsions. When the spinal cord has been injured only on the right side, it is only on that side that the skin of a part of the face and neck has the power of inducing fits, et vice versâ when the injury exists on the left side. If the two sides of the cord are injured, the two sides of the face can produce fits.—*Idem, Lancet,* No. 1,840, p. 571.

If we take two living animals of the same species and decapitate them by a section passing in one of them on the nib of the calamus scriptorius and in the other on the fourth or fifth cervical vertebra and cutting also in both the principal nerves of the neck and avoiding the section of the carotids, we often find that the first one has no convulsions, or in other words no agony, while the second almost always has very violent convulsions in the four limbs and in the trunk . . .

More than ten years ago I found that certain animals may live for many weeks, and in more recent researches, for eight months, after the extirpation of the whole medulla oblongata.

In these animals all the functions of organic life except pulmonary respiration continue without any apparent alteration, showing that these functions do not depend upon the medulla oblongata as some physiologists have thought. The persistence of life in these animals was possible on account of the cutaneous respiration, but in animals in which the skin absorbs but a small amount of oxygen, such as birds and mammals, death is said to be always rapid after the extirpation of the medulla oblongata, even when care is taken to avoid the influence of the operation upon the heart. It seems, therefore, that the medulla oblongata is an organ absolutely necessary to respiratory movements . . . . .

It is known that the only two appearances of proof that the medulla oblongata is the only centre of respiratory movements, or, in other words, the only source (direct or reflex) of these movements in the cerebro-spinal axis, are, 1st, that a transversal section of the lower part of medulla oblongata causes a sudden cessation of respiration; 2nd, that when transversal sections are made on the encephalon, from its front to its back, taking away layer after layer, it is said that it is only after the greater part of the medulla oblongata has been taken away that respiration is destroyed. As regards the first of these two assertions, we have already shown the objections against it, objections which are also very good against the second assertion. But we must say a few words more of this second assertion. When, after a series of transversal sections of the encephalon, we have reached the medulla oblongata, just above the upper roots of the par vagum, we find that respiration continues almost normal. If now we cut away the part of the medulla giving origin to this pair of nerves, we find in most cases that respiration is suddenly stopped . .

In weak animals after many parts of the encephalon have been taken away, the whole of the medulla oblongata and of the pons Varolii remaining, respiration sometimes continues normal, but it suddenly stops after a small part of the pons is removed. . . . .

The stronger an animal is the more parts of its encephalon can be taken away before we destroy respiration.

. . . .

In the strongest animals death occurs in a few hours, and from insufficiency of respiration, after the ablation of the encephalon except the whole of the medulla oblongata, and so it often is with anencephalic monsters. . .

A series of experiments on pigeons has given me the following results; with the spinal cord alone respiration continues a few minutes, with the spinal cord and the part of the oblong medulla giving origin to the principal excitors of respiration, the vagi, this function continues many hours (the longest duration we have seen is thirteen hours), if there is also a great part of the base of the encephalon left respiration continues longer but I have never seen it last more than a day and a half, . .

It seems indeed wonderful to see animals sometimes after a slight puncture of some part of the encephalon with the point of a needle, turn round, just like a horse in a circus, or roll over and over for hours and sometimes for days, with but short interruptions. . . .

Parts producing turning or rolling after an injury on the right side.

Turning or rolling by the right side.	Turning or rolling by the left side.
1. Anterior part of the optic thalamus (Schiff).	1. Posterior part of the optic thalamus (Schiff).
2. The hind parts of the crus cerebri (Schiff).	2. Some parts of the crus cerebri near the optic thalamus (Brown-Séquard).
3. The tubercula quadrigemina (Flourens).	3. Anterior and superior parts of the pons Varolii.
4. Posterior part of the processus cerebelli ad pontem (Magendie).	4. Anterior part of the processus cerebelli ad pontem (Lafargue).
5. Place of insertion of the auditory and of the facial nerves (Brown-Séquard and Martin-Magron).	5. Place of insertion of the glosso pharyngeal nerve (Brown-Séquard).
6. Neighbourhood of the insertion of the lower roots of the par vagum (Brown-Séquard).	6. Spinal cord, near oblong medulla (Brown-Séquard).

While rotation takes place it is easy to ascertain, 1st, that it is not its production by contractions resembling those of voluntary movements which causes the rolling or the turning; 2nd, that some muscles are in a state of tonic contraction; 3rd, that the trunk and neck of the animal are bent by a spasmodic action on the side of turning if it has a circus movement, and that it is bent like a corkscrew, as much as the bones allow, in cases of rolling; 4th, that sensibility and volition may remain, and there are frequent efforts to resist the tendency to turn or roll. . . .

And now, to add to the strangeness of the fact, in this last case the muscles remain contracted sometimes for hours, sometimes for days and weeks.—*Idem, Lancet*, No. 1,841, pp. 599, 600, 601.

M. Flourens has found that the section of the semicircular canals, in certain animals, is followed by a strange disorder of movements, and sometimes by a rotation (circus movement). I have ascertained that the phenomena observed in these experiments do not depend on the section of these canals, as this operation may not cause these phenomena, but that they are the results of an irritation of the auditory nerve, from the drawing upon it by the membranous semicircular canals at the time we divide them. In frogs and in mammals the direct irritation of the auditory nerve is followed by the most interesting phenomena. It is well known that in frogs the peripheric extremity of this nerve is enclosed in a bag containing carbonate of lime; as soon as this bag is laid bare and slightly touched, and still more if it be punctured with a needle or a bistonry, the anterior limb, on the opposite side, is thrown into a state of slight convulsion, and kept almost constantly in a spasmodic pronation, and almost at every attempt to move forwards the animal turns round on the side injured. As long as it lives (many days, or even many months) these phenomena may be observed, although not quite so marked as immediately after the injury or after the first 24 hours. In mammals the least puncture of the auditory nerve causes rolling, just as after the irritation of the processus cerebelli ad pontem; violent convulsions then occur in the eyes, the face, and many muscles of the neck and chest. The doctrine that the nerves of the higher senses are not endowed with general sensibility (*i.e.*, are not able to cause pain) seems not to be true with regard to the acoustic nerve; at least, the signs of pain given after an irritation of this pretended nerve are often as great as those observed after an irritation of the trunk of the trigeminal nerve.—*Idem, Lancet*, No. 1,842, p. 625.

I have now to speak of the condition of animal heat in cases of alteration of the spinal cord and the encephalon. The following conclusions may be drawn from a great many facts bearing on the subject: 1st, that usually anæsthesia is accompanied by a diminution of temperature: 2nd, that hyperæsthesia almost always co-exists with increased temperature; 3rd, that in paralysis, without either a notable hyperæsthesia or anæsthesia, the temperature is nearly normal. I must remark that the state of heat of a part is due to the amount of blood, the degree of heat of this fluid, the exposure of the part to the influence of the temperature of the surrounding medium, and the temperature of this medium.—*Idem, Lancet*, No. 1,843, p. 652.

Professor Brown-Séquard is continuing with indefatigable labour the series of his valuable researches and experiments upon the physiological pathology of the nervous system. His communications to the Academy of Medicine have formed the most marked features of the recent sittings of this learned society. As you will doubtless give full attention to these researches when they shall have been published in extenso, I shall only mention a few points thereof so as to keep you au courant of everything that crops up here at the very moment of the event. My letters must be photographs of the passing occurrences of the day. M. Brown-Séquard's most recent researches bear upon the consequences produced by certain lessons of the corpora restiformia and the sciatic nerve in guinea-pigs. According to the illustrious experimenter, when the sciatic nerve is severed in guinea-pigs on exciting a certain determinate part of the face which he names le zone epileptogene epileptiform phenomena are produced. Excitement of no other portion of the face can induce an attack of epilepsy. But in order that the phenomena may take place, it is necessary that the excitement should be produced on the side corresponding to the limb where the nerve has been divided. M. Brown-Séquard showed two guinea-pigs to the Academy and reproduced his experiments before the assembly. M. Colin, one of the members, mentioned that he had often performed the division of the sciatic nerve without being able to induce epileptiform attacks. M. Brown-Séquard's experiments on the restiform bodies are equally interesting and curious. One of the results of injury to the corpora is the production of hæmorrhage under the skin of the ear. Hitherto certain kinds of hæmorrhage had been observed as the result of injury to the nervous system, for instance hæmorrhage of the kidneys in diseases of the spinal cord and hæmorrhage of the intestinal tube in diseases of the brain. But the fact now illustrated is remarkable on account of its constant occurrence. Another result is the production of dry gangrene of the ear, which, according to the experimenter, is not the consequence of paralysis produced by the section of the restiform bodies. M. Brown-Séquard exhibited a guinea-pig showing this kind of gangrene. At the last meeting of the Academy he again adverted to the subject, and stated that the precise point of the restiform body which he excited in order to produce hæmorrhage and sphacelus was the nib of the calamus scriptorius. Some particulars he added tended to show that this situation was really the one which he excited in his experiments, for instance, the paralysis of the tongue and anæsthesia of the lips—the animal could not hold the food which was introduced into its mouth. It was a well known fact that the hypoglassal nerve and fifth pair take their origin in the neighbourhood of the calamus scriptorius. M. Brown-Séquard concluded by pointing out the strict analogy which exists in a great number of cases between the phenomena determined experimentally in animals and those which are observed clinically in man. On account of the extreme interest of these experiments M. Bouillaud has requested M. Brown-Séquard to repeat his experiments before a certain number of the members, which has been assented to.—*Lancet*, No. 2,380, p. 514.

In the course of some recent experiments on the establishment of artificial epilepsy in guinea-pigs, Dr. C. Westphal has been over the same ground as that explored by M. Brown-Séquard, and fully corroborates the results at which the latter observer arrived. He adds, however, some new and interesting facts. Thus, he found that if one or two slight blows on the side of the head are given to a guinea-pig they are sufficient to bring on an epileptiform attack, after which the animal again recovers its liveliness, or it remains heavy for some time and then exhibits a kind of rotatory movement, like those shown by Schiff to occur in rabbits after lesion of the crus cerebri.

. . . .

If the animal survives the blows, a similar epileptigenic zone is created as in the guinea-pig treated on M. Brown-Séquard's method by lesion of the medulla oblongata at certain points or section of the sciatic; and as in these last cases, the zone is near the angle of the lower jaw. Before the zone is well established, and four weeks are usually required for this purpose, the animals betray the presence of some irritation at this part by frequently scratching it. After it is established slight punching will induce tonic and clonic spasms though the sensibility of the skin is here diminished. The rapidity with which the zone can be established may be increased by striking the animal's head on successive days, and the excitability of the zone endures for a period varying from six weeks to six months. The condition is hereditary. M. Westphal set himself to ascertain the nature of the changes induced by the blow or blows. That the condition is not produced by any alteration in the integuments is shown by the circumstance that the fits occur when the exposed skull is struck.—*Lancet*, No. 2,528, p. 195.

Dr. Brown-Séquard has with great kindness related to me his more recent researches, and shown me the interesting little animal upon which he is now experimenting. I have therefore the pleasure of being able to speak of them de visu, and of adding some interesting details of the note which I sent you two weeks ago. If you remember I then mentioned that the professor's recent researches were upon some of the effects of section of the sciatic nerve and injury to the corpora restiformia in guinea-pigs. A great number of the animals were shown to me in which the sciatic nerve had been severed, and in all the zone epileptogéne did exist. I mean one single spot by exciting which the fit was immediately produced. Pinch wherever you like independently of that situation, and though the animal will not like it, and will scream more or less, there is nothing abnormal, but as soon as you excite the particular spot it goes into a fit. In this situation, which may be easily circumscribed, extending from the ear to the eye round below the jaw, and backwards to the shoulder-blade, there seems to be an obvious impairment of nutrition, the hair is much less abundant, parasitical animals are found there, &c., and besides sensibility is considerably diminished. The fits produced are quite similar to those brought on by section of the lateral half of the spinal cord.

Dr. Brown-Séquard showed me some animals in which the paw of the limb where the sciatic nerve had been divided was more or less injured. This, he observed, was not a spontaneous injury, which must be attributed, as had been thought to the division of the nerve. The dragging of the paralysed limb produced a slight abrasion, and as soon as there was a drop of blood, the little animal set to gnawing the insensible extremity. It is also the case with rabbits. There must be, however, a drop of blood to excite the voracity of the animals. In some of them two of the claws of the feet, the two outer ones, animated by the great sciatic nerve, which alone is severed, had fallen off, while the remaining claw and middle one, animated by the little sciatic nerve, was unimpaired. By wrapping up the limb in a bag as soon as section of the sciatic nerve is performed the experimenter prevents any injury occurring.

With regard to his other series of experiments, those in the corpora restiformia, the professor showed me a great number of guinea-pigs in which I could trace the effects of injury to that particular part of the nervous system. The ears of several guinea-pigs showed the appearances of dry grangrene. In some of the animals half of the ears had already fallen. The aspect produced by the solution of continuity along the edge of the ear is quite distinct from that of a bite. In a bite the little portion of the ear seems cleanly cut off, whereas in dry gangrene it appears to have crumbled off. I have noticed the remains of two or three hæmorrhagic clots which had formed on the ear.

To conclude, I may just mention that Dr. Brown-Séquard was much surprised when some of the French journals stated that he had neglected to note the occasional occurrence of hæmorrhage in the ears of insane people. The fact is that the very object of the illustrious experimenter in mentioning the phenomena which he observed in Guinea-pigs was to show that the hæmorrhagic clot observed on the ears of mad patients, and concerning the etiology of which there is still much discussion, may be due to purely internal nervous causes, and not to external violence, his chief aim in every one of these experiments being their application to the pathology and therapeutics of the human species.—*Lancet*, No. 2,382, p. 586.

Nothnagel employs a new method for the determination of the functions of the brain. His observations are made mostly on rabbits. An incision is made in the scalp; the skull is perforated with a needle. Through the canal thus formed in the bone a very small drop of a concentrated solution of chromic acid is injected by means of a hypodermic syringe with a very slender nozzle. The scalp wound is then united by suture, and the animal does not seem to be affected, except with regard to the functional derangement incidental to the lesion. Generally they survive the operation two or three weeks, and die from causes which Nothnagel cannot explain, no constitutional symptoms being developed.—*The Doctor*, November 1st, 1873, pp. 214 and 215.

Professor Nothnagel of Freiburg, contributes a paper to the last of Vichow's Archiv·Band, lvii. (Heft. 2, p. 184), containing an account of a series of researches he has recently made in Heidenhaur's laboratory upon the functions of the brain. With a few exceptions his experiments were made upon rabbits placed thoroughly under the influence of woorara. He acknowledges that dogs are better subjects for experiment, whilst their more convoluted brain resembles more closely than the rabbit that of man.

On the other hand, rabbits can be obtained in any number, and they are not difficult to keep. The plan adopted by Professor Nothnagel, was suggested to him by Professor Heidenham and is somewhat peculiar. It consists in drilling a small hole through the cranium, through which the cannula of a subcutaneous injection syringe is inserted and plunged to a greater or less depth into the brain. A minute quantity, perhaps not amounting to more than quarter or half an ordinary drop, of a concentrated solution of chromic acid is then injected and the effects watched. The pain must be slight, as the animal often remains passive till the sutures required to close the wound are inserted. It is also obvious that in this mode of procedure bleeding and those alterations which might result from evaporation and exposure to cold are avoided. When the acid was injected directly into the ventricles, death took place in from ten to thirty minutes, the pulse becoming very slow and great dyspnœa setting in, soon followed by increased action of the heart and convulsions. . . .

Similar experiments upon dogs, the acid being injected into the outer extremity of the gyrus postfrontalis, were attended with analogous results. . . . .

When the animals lived for a fortnight or more they appeared to recover completely. . . . .

Injection into the lenticular nucleus constantly produced the above described deviation of the limbs. Injection into the nucleus caudatus appeared for the first few minutes to be without effect, but soon the animal began to leap forward, the leaps succeeding one another faster and faster till, after some minutes, it dropped exhausted upon its side, the limbs continuing to move rapidly.—*Lancet*, No. 2,601, p. 18.

Nothnagel has continued his researches on this subject, still using the injection of chromic acid. When only one lenticular body was operated on the results were the following:—Deviation of the leg of the opposite side (right) towards the middle line and that of the same side (left) outwards, a lateral curvature of the spine with the convexity turned towards the opposite side (right) and at the same time a moderate cypposis. The animal could, however, execute all voluntary movements.

A different state of things occurred when lotn. micleilenticular is that operated on. In 26 cases the author succeeded with the operation, and the results in all cases coincided.

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The spinal column was sometimes straight, sometimes cypnotic, but never curved laterally. The ears were erect and never laid backwards upon the neck. Respiration and action of the heart normal. If the fore limbs were carefully extended so that the animal did not lose its equilibrium, and though the feet might be placed in a very unnatural position, as were the neck, they were not drawn back, as always occurs in the normal animal. Slight pinching of the tail, which a normal animal would notice, was followed by withdrawal of the foot from the unnatural position, and the animal appeared as if it would spring, but with one spring the movement came to an end, and the animal became motionless as before. The same thing could be repeated over and over again. In some cases the animal sprung four, six, or even sixteen times. The animal sat without making any attempt at spontaneous movement, and if not disturbed until death occurred, just like an animal from which the cerebral hemisphere had been removed. Nor did the animals eat of their own accord. Most of the animals died on the second or third day, but *six* lived till the seventeenth day and were very emaciated.—*Stirling, Journal of Anatomy and Physiology*, November 1874, p. 210.

On October 25th he divided the right hypoglossal nerve in a rabbit, and took out a piece about a quarter of an inch in length. Immediately after the operation, and during the whole time that the animal was under observation, the tongue was strongly protruded to the right side. On November the 27th the rabbit was killed.—*Mr. Clarke, Doctor*, January 1st, 1872, p. 24.

Dr. Milne Edwards also endeavoured to substitute the carbonates of iron, manganese, and magnesia respectively for that of lime in bones by an artificial diet. This diet having reduced the pigeons subjected to experiment to utter emaciation they were killed; the bones were extremely brittle and thin, but only traces of magnesia and iron were discovered in two and none in the third. So thus the composition of bone does not appear susceptible of alteration by substitution; and Dr. Milne Edwards concludes that bone is a compound of two primordial substances, viz., phosphate of lime and osseine, the immediate principle of bone. Rather a lame conclusion this after all.—*Dr. Milne Edwards, Lancet*, No. 1,977, p. 69.

Another, and a very droll discovery, made by the same Professor (who, by the way, should be called to order by some member of the Society for the Prevention of Cruelty to Animals) is that the testicle of one frog may be engrafted into the body of another animal of the same species, so that one "froggy" may "go a-wooing" with the testicles of another froggy, and not at all à ses propres frais; this seems a "leetle" too much. His last experiment is far more intelligible than the preceding, in part, at least. He says, "If you transplant into the abdomen (under the skin) of a female frog, a few days before the laying of her eggs, a frog's testicle, such is the attraction between this body and the eggs that ulceration of the abdominal muscles takes place, the male and female elements coming into contact, and so violently that the frog dies." This last result is, I confess, the only portion of the whole paper which I can conveniently credit.—*Lancet, No.* 1,931, p. 224.

One frog died in twelve hours, another was killed, and the third, after remaining on its back in a tetanic condition for ten days, recovered.—*Dr. Harvey, Lancet*, No. 2,053, p. 7.

C.

EVIDENCE OF DESIGN TO TEACH STUDENTS BY VIVISECTION.

[The numerous handbooks published for students would alone provide this evidence; but professors have spoken out their thoughts freely, and these are reported as well as the contents of handbooks. Foreign handbooks are extensively used in England, but these have not been quoted from. The volume published by Drs. Burdon-Sanderson, Klein, Foster, and Brunton, necessarily appears in this division. Its experiments might have been classed under (A.) and (B.), but the tendency of its teaching would not have been seen so well by such an arrangement. Consequently, a large number of experiments appear under this head. That the object of the editor and his co-adjutors was to induce young persons to perform experiments on their own account and without adequate surveillance is manifest throughout the work, by the supply of elementary knowledge and elaborate data. Not only are the names and quantities of necessary chemicals given, but the most careful description is provided in letter-press and plates of implements for holding animals during their struggles, so that a novice may learn at home without a teacher. Besides, the editor's preface states, that the book is "intended for beginners," and that "difficult and complicated" experiments consequently have been omitted; and that of Dr. Foster allures the student by assurances of inexpensive as well as easy manipulation, for it avers that the experiments "may for the most part be conducted on frogs, may be repeated any number of times without difficulty or expense, and so serve usefully as a means of training students in physiological study and inquiry."

Very seldom indeed is the student told to anæsthetise, and then only during an operation. It cannot be alleged that "beginners" know when to narcotise, and when not; but if they do then the few directions to use chloral, etc. are unnecessary. No doubt should have been left on this point in a handbook designed "for beginners." Besides, where will students find cautions against the infliction of unnecessary pain, and wanton experimentation? On the contrary, the student is encouraged to repeat the torture "any number of times." These facts are significant.]

Advance of Physiology.—*Nature*, No. 204, p. 456–7.

Presumption of students.—*Dr. Scoffern, Belgravia*, April 1867, p. 221.

Experiments during lecture.—*Lancet*, No. 2,471, p. 3.
Dr. Rutherford, Lancet, No. 2,476, p. 185.

Pflüger discovered a curious fact regarding the stimulation of a motor nerve, and the effect which it produces on a muscle. He found that when he stimulated with equal force the sciatic nerve of a frog at one time near the gastrocnemius and at another time at a distance from the muscle, in the latter case a more powerful impression was produced on the muscle. We shall perform the experiment.

I dissect out the sciatic nerve of a frog, sever it from the vertebral column, but leave it connected with the leg.—*Dr. Rutherford, Lancet*, No. 2,483, p. 438.

I shall illustrate what I have been saying by some experiments. We have here a white rabbit . . . . I now cut down upon the cervical sympathetic nerve. Let us take that on the right side for convenience. I expose the nerve about the middle of the neck. I separate it from the superior cardiac branch of the vagus, a smaller nerve which lies close beside the sympathetic, and I put a fine silk ligature round the sympathetic, and tie it firmly round the nerve. The effect of this is to paralyse the nerve as thoroughly as if the nerve were divided. Now compare the two ears and the two pupils again. The vessels in the right ear are much larger than those in the left ear. The right ear is in consequence distinctly redder than the left You see also that the right pupil is smaller than the left one. I shall now divide the nerve immediately below the ligatured point and then irritate the nerve just above the ligature. In an experiment of this sort we call this part of the divided nerve its "upper" end; the end of the nerve which lies below the point of section is called the "lower" end. Watch the vessels of the right ear while I faradise (stimulate with faradic or induced electricity) the upper end of the nerve. You see that they contract, and the ear in consequence becomes blanched. We shall wait for a few moments until the irritation passes off. Now look at the right pupil while I irritate the upper end of the nerve again. You see that it dilates enormously under the influence of the irritation. We have seen then that on paralysing the cervical sympathetic blood vessels of the ear on that same side dilate and the pupil on the same side contracts, and that on stimulating the upper or cranial end of the divided nerve precisely the converse takes place in both parts. We therefore conclude that this nerve contains fibres whose function is to cause the vessels of the ear to contract and the pupil to dilate, and that these fibres convey their influence up the neck. As I want to keep the experiment as simple as possible, I shall not stimulate the lower end of this nerve at present. I cut down the phrenic, and I open the abdomen in order that you may see the diaphragm. You see that at intervals both sides of the diaphragm are drawn downwards. I divide the right phrenic nerve in the neck. Now look at the right side of the diaphragm. It remains quite loose and flabby when the left side is drawn down instead of being tightly drawn down with it as before. I shall now irritate the lower end of the nerve. Watch the diaphragm. You see that the right half is drawn violently downwards during the irritation, and that the flabby state returns when I stop the irritation of the nerve. We conclude that the right phrenic nerve contains motor fibres for the right half of the diaphragm, and that these influences pass through the nerve down the neck.—*Idem, Lancet*, No. 2,483, p. 439.

Under this head some general questions regarding sensory nerves were discussed, and the mode of estimating the amount of ordinary sensibility in a part was demonstrated.

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I take another frog. In this case I open the cranium and remove the brain and medulla oblongata.—*Idem, Lancet*, No. 2,487, p. 565.

He was aware that there were some who entertained the idea that vivisection was not necessary when it had for its object the mere demonstration for educational purposes of facts already known.

Those who held this doctrine appeared to him to forget that physiology was an experimental science, and that no right conception of the subject could be obtained unless the student was shown the experiments that were necessary for the demonstration of certain facts.

. . . .

Now he maintained that this definite and critical knowledge regarding the bodily organism could not be attained unless their students were shown experiments on living animals.—*Speech of Professor Rutherford, the British Medical Association, Edinburgh*, 1875.—*Scotsman.*

In recent years the teaching of physiology had made a great stride in this country. Laboratories duly appointed had been and were being organised; and the method of physiological instruction had in most instances passed from the mere prelection illustrated by diagrams to an experimental exposition of the subject. In his student days the latter element was wanting, and at this moment there was distinct danger of a return to something like that miserable mode of instruction in consequence of the fanatical clamour of a number of persons, excited, it must be admitted, by one or two members of their own profession.

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Physiology was an experimental science, and that no right conception of the subject could be obtained unless the student was shown the experiments that were necessary for the demonstration of certain facts.

. . . .

Had not every teacher repeatedly observed the altogether different mental attitude which students assumed the moment he passed from mere description to a demonstration of phenomena? He far more forcibly arrested their attention, and far more deeply imprinted on their minds the facts he would bring home to them.

. . . .

Definite and critical knowledge regarding the bodily organism could not be attained unless their students were shown experiments on living animals, and he held that those authorities who seemed to be of opinion that this method of tuition might be dispensed with, were entirely overlooking the vast importance, not only to the student himself, but to the whole race, of an experimental manner of laying the foundation of a knowledge of the institutes of medicine.

. . . .

It was not necessary for a sound physiological education that their students should be shown all the experiments that were needed to demonstrate physiological truths; they probably did enough if they showed experiments on the cardinal points of physiology; and he averred that all the experiments on the higher animals that were really required for the purpose of education, could be performed with the aid of narcotics. Seeing that this was so, why should it be that some had become convinced that, in consequence of the present inflamed state of the popular mind on the subject of vivisection, the right education of medical students must be abandoned? The popular mind had been abused by inaccurate and misleading statements regarding both their motives and their actions. He maintained that a great and deplorable error was committed when the unreasonable clamour of the anti-vivisectionists was met in the spirit of compromise instead of the spirit of stern resistance. He believed that the unfortunate Vivisection Bill which was laid on the table of Parliament conferred a dignity on the policy of the anti-vivisectionists which, but for that Bill, it would probably never have possessed. It was true that there had been a withdrawal of that singular Bill, according to which they were to have been fined fifty pounds, or to have been sent to prison for two months, if they had dared to show to their students any experiments, even upon a narcotised animal. But the effect of the Bill was not effaced; the increased boldness which it had given to the pretensions of the anti-vivisectionists was only too evident. All that they could now hope was that the good sense of the Legislature would in the end prevail, and that it would do nothing to hamper the education of medical men.

The learned professor went on to explain, with the help of numerous diagrams, the result of a series of experiments he had made in reference to the action of certain drugs on the biliary secretion of the dog.—*Dr. Rutherford, Lancet*, No. 2,711, pp. 238–9.

On the art of experimenting.—*Professor Brown-Séquard, Lancet*, No. 2,380, p. 514.

M. Bernard, Lancet, 1872, No. 2,535, p. 438.

Students should spend time in the physiological laboratory.—*Dr. Ross, British Medical Journal*, No. 738, p. 238.

Dr. Burrows, Lancet, No. 2,371, p. 208.

Elementary lessons in physiology.—*Professor Huxley, Saturday Review*, 1/10/74.

Experiments performed at the Physiological Laboratory, University of Edinburgh.

W. B. A. Scott, M.D., Edin.

[Letter to the *Echo*.]

J. Burdon-Sanderson's Lectures delivered in the Physiological Laboratory of University College.

In 1863 the lamented v. Bezold published his well-known researches on the nervous system of the heart. Among a number of other less important discoveries, he showed for the first time the nature and extent of the influence exercised by the brain and spinal cord on the circulation of the blood. He found that when, in a curarised rabbit or dog, the spinal cord is severed from the brain, the arterial pressure sinks very considerably, while at the same time the number and extent of the contractions of the heart are diminished; and that if, on the other hand, the upper end of the divided spinal cord is irritated below the point of section, the arterial pressure rises to its original level and the heart to its previous activity.

The leading experiment is as follows:—Two centigrammes of curare, dissolved in a cubic centimetre of water, are injected below the skin, and immediately after artificial respiration is begun. This dose is sufficient, as was first shown by v. Bezold himself, to paralyse the extremities so completely that neither stimulation of the cord nor of any muscular nerve produces, the slightest contraction of voluntary muscles, while, as we shall see on another occasion, it is not sufficient to interfere with the action of the heart. Respiration of course ceases, but it is maintained, as I have said, mechanically, the means employed for the purpose being a pair of bellows the tube of which communicates with a cannula adapted to the trachea of the animal.

The membrane between the atlas and the occipital bone having been previously exposed and one of the carotid arteries connected with the manometer of the kymograph, observations are taken of the arterial pressure and of the frequency of the pulse. This done the spinal cord is divided at the atlas. Immediately the rate of pulsation is diminished, say from 140 to 100, and after a few seconds the arterial pressure sinks, say from three or four inches to one or two. Needles are then inserted into the spinal cord, one at the upper edge of the axis, both of which are connected with the secondary coil of Duboi's induction apparatus. At once the heart beats more frequently and vigorously and the mercurial column attains its former level.

The next step in the experiment is the destruction of the cerebro-spinal cardiac nerves. These nerves, as you know, reach the heart or leave it either through the vagi or the sympathetic.

The destruction of the nerves is best effected with the galvanic cautery, the action of which is more certain and more easily controlled than any other agent which could be employed. It answers the purpose so completely that in careful subsequent dissection it is found that every nerve is served. As soon as the destruction of the nerves is effected the spinal cord is again excited, great care being taken that the strength of the current shall be the same as in the previous observation.

There are, however, in these and other respects considerable differences in the results observed in different animals the conditions of which have not yet been determined.*

Upon another dog, under partial anæsthesia, he divided with a fine curved scalpel the corpus striatum and optic thalamus on one side, the corpus callosum having previously been cut through. The electrodes were then placed on the convolutions above and behind the sylvian fissure. Contraction resulted, when the current was strong, not only in the fore leg of the opposite side, but also in the hind leg. In another experiment he removed the whole cerebral masses above the fous varolii, and applied the electrodes to the surface of the section. Muscular contractions resulted, limited to the fore limbs, right and left.—*Lancet*, No. 2,630, p. 136.

Handbook Physiological Laboratory,

[Some of the following experiments are quoted not to show extreme torture, but regardlessness to suffering as well as the minute character of directions given, to bring them within the capacity of young people.]

Page 1.—Take the newt out of the water, dry the tail, cut off its end. If no blood comes, squeeze the organ from the root towards the tip until a drop is obtained.

Page 25.—To study the forms of the various cells of the separate layers we may obtain a thin shred from the surface of the tongue or gums of a mammal by energetically scraping it with a scalpel.

Page 26.—Finally, if we have scraped very energetically with the scalpel, we meet with cells corresponding to the deepest layers. . . . A frog is held by an assistant, its nictitating drawn down, and from the anterior corneal surface a thin layer is scraped with a lancet-shaped or a cataract knife.

Page 34.—It is not difficult to remove those structures [network of elastic fibres] even from the living animal. The easiest way is to place the vocal cord for a few minutes in dilute acetic acid, and then to scrape off the epithelium with a lancet-shaped needle, a process which is much facilitated by the previous steeping in the acid.

Page 35.—A frog is held by an assistant in such a way that the bulbus oculi is tense. The membrana nictitans is then drawn back, and the bulb penetrated with a cataract knife, just as in the operation for cataract, at the limbus conjunctivæ next the inner canthus. The point of the knife is advanced until it approaches the limbus of the opposite side, without puncturing it, and is then carried outwards and upwards, so as to form a flap, consisting of the upper half of the cornea. The extreme edge of the flap must then be seized with the forceps, while the lower half of the cornea is cut away with the aid of scissors curved in the direction of their edge. The cornea is next transferred to a drop of humor aqueus (previously obtained by puncturing the opposite eye), and spread out on the glass slide with anterior surface uppermost.

Page 38.—Preparations are obtained by stripping off shreds of a cornea (of a rabbit or frog). . . .

The centre of the cornea of a frog, which is held by an assistant in the manner previously described is firmly cauterized with a pointed stick of lunar caustic. One or two drops of salt solution are then allowed to flow over the cornea to decompose the excess of nitrate of silver. About an hour after the cauterization the cornea is excised in the manner directed in p. 35. . . . . .

It consists in first scraping the cornea of a living frog or small mammal with a sharp cataract knife so as to remove the epithelium completely. After a little practice, and provided the bulb is properly fixed by an assistant, it is not difficult to perform this operation without injuring the substance of the cornea, Thereupon the caustic is two or three times lightly rubbed over the whole surface, after which the eye is washed with saline solution, and the animal is left to itself for twenty or thirty minutes. The cornea is then excised.

Page 44.—If in a rabbit the skin and subcutaneous tissue are divided over the inner (anterior) third of the infraorbital edge, and the thin membrane which stretches over the infraorbital fossa is severed, it is easy to remove, along with the glandula infraorbitalis, a gelatinous hyaline mass.

. . . .

If the tail of a very young rat is amputated, and the tip torn asunder from the cut end, a great number of isolated lengths of tendon are obtained.

Page 54.—The leg of a water-beetle (hydrophilus) is torn out and its horny covering removed.

Page 60.—One of the hind legs of a tadpole is amputated at the thigh. The animal is then replaced in water. After forty-eight hours the loosened muscular fibres hang from the stump in long pencils. These are cut off close to the surface of the stump with sharp scissors. . .

Page 78.—In a living or recently killed rabbit the cornea is excised close to the limbus.

. . . .

A silk thread having been passed through the centre of the cornea of rana esculenta and brought out again at the sclerotic ring, the two ends are knotted together. After the thread has remained from five to eight hours the cornea is excised.

Page 97.—In a large frog secured on its back the abdominal vein is carefully exposed under a dissecting lens, in its course up the middle line of the anterior wall of the belly. A ligature is passed round the distal end of the prepared part and tightened.

Page 98.—The external jugular of the animal is then exposed by a sufficient incision, and cleared of the surrounding tissue with the aid of dissecting forceps.

Page 108.—A snip is made in the right side of the belly. . . . . The incision is then continued upwards and downwards in such directions as to avoid bleeding. . . . The muscles are divided in the same vertical line. . . . . The intestine and mesentery are drawn out carefully.

Page 109.—The animal must be cararised as before . . . . . the tongue drawn out by the cornua, around which a thread must be secured . . . . . to pins.

Page 113.—In a large or middle-sized rabbit which has been kept from sixteen to twenty hours without food, ten cubic centimeters of a warm five per cent. solution of Prussian blue are injected into the abdominal cavity . . . . . After three hours and a half the animal is bled to death by opening the carotid artery, or killed by strangling.

Pages 158 to 162.—The inflammatory changes of the epithelial elements of the cornea may be studied by abrading the epithelium over a limited surface in several frogs, and examining the organ at various periods after the injury. . . . . . . After two or three days sections may be made by shaving off a portion of the mucous membrane. . . . In the blood vessels the inflammatory changes may be studied by cauterizing the external surface of any superficial vein (*e.g.*, the external jugular or femoral), or even by simply ligaturing the vessel. Three or four days after the injury the vessel is excised. . . . The best method is to pass a needle into the knee-joint of a rabbit in such a way that it penetrates into the tibia. A few days after sections are made of the fresh cartilage, and stained in gold. . . . Germination of the cells of bone may be induced in the long bones of mammalia by passing a red-hot needle as deeply as possible into a bone previously freed of the soft parts covering it, and then cauterizing the hole with a pointed stick of nitrate of silver, or by violent fracture. After a week or more the bone is excised. . . . Inflammation of the tissue of the liver may be induced by passing a needle into the organ. Twenty-four to forty-eight hours after the injury the animal must be killed. . . . The cornea may be cauterized at the centre to such a depth as almost to perforate it, or a thread may be drawn through it, entering at the centre and passing out through the sclerotic beyond the margin, the ends of which are then tied. . . . . To study the successive stages of the process half a dozen corneas should be prepared in this way at a time, which can then be excised after eight, twelve, eighteen, twenty-four, thirty-six, and forty-eight hours. The best preparations are obtained from rana esculenta during the summer months, from eight to twenty-four hours after the introduction of a silk thread as above described. . . . Inflammation is produced in one eye by cauterization, and then twenty-four hours after a portion of the cornea of the other eye is excised, spread out carefully, and lodged between the membrana nictitans and the cornea of the injured eye. The membrana nictitans is then drawn up and secured by two or three ligatures to the skin. After twenty-four hours more the sac is opened and the cornea taken out. . . . . . . The mucous membrane covering the large lymphatic sac of the under surface is snipped off with curved scissors. The observation is necessarily tedious, often lasting for forty-eight hours. It is therefore desirable to replace the tongue in the mouth for a time after each examination. . . . . In a curarized tadpole the required degree of irritation can be produced either by simply pencilling the surface or by allowing a drop of ammonia to fall on it from a capillary pipette, or, finally, by piercing it with a needle. The research must be continued often for many hours.

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* This beautifully simple experiment we owe to Dr. Stricker, with whom I had the pleasure of repeating it here very frequently last summer.—*Medical Times, No.* 1,094, *pages* 683 *and* 684.

Page 166.—Several frogs are then selected, in each of which the pericardium is exposed, and divided as directed in § 46, and a snip made in the ventricle with fine scissors.

Page 174.—In a rabbit two small incisions are made across the course of the external jugular vein (*see* § 48), one near the clavicle, the other near the origin of the vessel, great care being taken not to go deeper than is necessary, in order to see the vessel through the fascia. A small needle is then passed under the vein, near the proximal incision, in a direction at right angles to that of its axis, and corresponding to that of the incision, but deeper. A second needle is then laid in the course of the incision, and drawn tightly towards the first by a ligature at either end, by which means the blood-current is entirely arrested, while the coats of the vein are absolutely protected from injury. A second pair of needles is then inserted at the distal incision, and secured in a similar manner so as to shut in the blood with which the vein becomes distended after the tightening of the first ligature. After the lapse of a couple of days the ligatured portion of the vein is exposed at some part of its course, and punctured with a glass pipette, by means of which the blood is withdrawn from it by suction in a perfectly liquid state. . . . . . The arterial trunks leading from the heart of a frog or tortoise are first tied, and then (as soon as the heart has become distended) the venous trunks. The heart full of blood is removed from the body. . . . . The pericardium of a frog is then exposed and divided, and a snip made in the ventricle with absolutely clean scissors.

Page 176.—A frog having been secured in the usual way (*see* § 46) in the prone position, the heart is exposed, and the right aorta ligatured. A clip is then placed on the left aorta at its origin from the bulb.

Page 212.—The animal having been secured on Czermak's rabbit board, and the fur clipped, the skin is pinched up between the finger and thumb on either side of the upper end of the trachea so as to form a horizontal fold, which an assistant divides vertically. . . . . The opening having been enlarged with the aid of a second pair of blunt forceps, the sterno-mastoid is slightly drawn aside, so as to bring the artery with its three accompanying nerves, the vagus, the depressor, and the sympathetic, into view. The sheath having been opened the artery is raised on a blunt hook, and easily cleared from its attachments to a distance of three-quarters of an inch in either direction . . . When kept closed by the adjusting screw, seize upon the head of a cat or rabbit in such a manner as to hold it firmly without inflicting the slightest injury. The neck of the animal rests on a cylindrical cushion, covered with waterproof cloth, and the rest of the body on a mattress of similar material. Along the edges of the board there are convenient attachments for the extremities.

Page 229.—The excellency of this method lies in the fact that the animal can be kept under observations, without the use of any narcotising drug, for a long time in a perfectly natural condition. The frog is used both in the larval and adult state. To observe the circulation in the tail of the tadpole, the animal is placed in a moderately strong solution of curare, care being taken to remove it before it is completely paralysed—the moment, in short, that its motions became sluggish. It is also possible to secure it, without the aid of curare, in a holder of construction similar to that of the instrument I have just described—a method which has this great advantage, that the animal is in a more normal condition; for even when curare is given with the greatest care, the action of the heart is weakened by it.

. . . .

There are three transparent parts of the frog—the mesentery, the web, and the tongue—each of which has its special advantages for the purposes of study. For a first view of the relation between arteries, capillaries, veins, and lymphatics, the mesentery is superior to either of the other two. The frog must be placed under the influence of curare, the dose of which for the ordinary specimens of rana temporaria, is about $\frac{1}{2000}$th of a grain. . . . .

The solution of curare is prepared by weighing out five milligrammes of the substance, and rubbing it up in a glass mortar with a little alcohol. . . .

The proper quantity of water—that is, sufficient to make up ten cubic centimeters—is then added, and a straw-coloured, nearly limpid liquid is obtained, a single drop of which is a sufficient close. It is injected under the skin of the back with an ordinary subcutaneous syringe, and answers best when the effect does not manifest itself for some time after the injection.

Page 230.—The observation may be continued without material change for many hours.

Page 233.—All being now ready, a frog, previously slightly curarized, is fixed on the table in the supine position. The integument is divided over the sternum in the middle line, and the anterior wall of the upper part of the visceral cavity removed, so as to expose the pericardium, great care being taken not to injure the abdominal vein, or any other large vessel. The ventricle is then opened, and the cannula passed through the opening into the bulb, and secured by a ligature. This done, the heart is drawn upwards.

Page 235.—Two frogs are slightly curarized, and placed side by side on the same board in the supine position. In both the heart and great vessels are exposed as in the preceding section. . . . . . The brain and spinal cord are destroyed in one of the frogs by inserting a strong needle into the spinal canal immediately below the occipital bone, and then passing it upwards and downwards. [The other frog is left sensitive].

Page 236.—Two frogs are suspended side by side, one of which has been pithed in the manner above described. In both the heart is exposed and the ventricle cut across.

Page 237.—A frog having been curarized just sufficiently to paralyse its voluntary muscles. . . . . All being now ready, the integument is opened along the middle line of the back of the neck, and the occipital bone perforated in the middle line with a fine awl close to its posterior margin. . . . . The frog is then laid, back downwards on the board, in such a position that one of the needles enters the cranium through the hole in the occipital bone, the other the spinal canal. . . . . . Finally, the heart is exposed as before.

Page 238.—The dose of curare must be very small, and should therefore be given an hour or two before the observation is made. One at least of the electrodes must be inserted within the cranium; for if both are below the occipital bone the effect is uncertain. . . . . The integument is divided in the middle line from the pomum adami downwards, as directed in section 1. On drawing the edge of the incision to either side, the external jugular vein is readily seen as it crosses the sterno-mastoid. It is then carefully cleared of the platysma fibres and fascia which cover it, and of its sheath to the extent of an inch or more, with the aid of two pairs of blunt forceps. . . The animal being under the influence of curare its voluntary muscles are paralysed.

Page 240.—The cannulæ having been placed in the trachea and external jugular vein, and the apparatus for artificial respiration being in order, three-tenths of a centimeter of a one per cent. solution of curare is injected. . . . . . With the help of this needle three ligatures are passed underneath the muscles which stretch vertically on either side of the spine of the atlas. . . . . . The ligatures having been tightened and the muscles divided in the middle line . . . . . all that is necessary is to divide carefully first the skin, and then the fascia which covers it. . . . . . To observe the effect of vascular contraction on the heart that organ must be exposed. In a curarized animal this can be effected without interfering materially with the vital functions. Ligatures of fine copper wire having been passed with the aid of a curved needle around the 3rd, 4th, 5th, and 6th cartilages, close to the left edge of the sternum, and a second vertical series of ligatures around the corresponding ribs at a sufficient distance outwards, the portion of the thoracic wall which lies between the two series can be removed without hæmorrhage. It is then seen that after section of the cord the heart is flaccid and empty, and that its cavities fill and its action becomes vigorous when the vascular contraction caused by excitation of the peripheral end forces the blood forwards so as to fill the right auricle.

Page 242.—For this purpose the occipital bone must be perforated with a small trephine in the middle line between the occipital protuberance and the occipital spine. By this opening a thin-bladed knife is introduced in the middle plane with its edge outwards, by which the medulla is divided, first on one side, then on the other. . . . The currents employed must be feeble when the nerves are excited by the direct application of the electrodes to the sensory nerves, but strong when it is intended to excite their cutaneous or mucous endings. The periods of excitation should always be very short. The experiment may be varied as follows :— A frog having been carefully curarized . . . . the points of the excitor are placed upon the tongue, the mouth being kept open for the purpose.

Page 243.—A frog having been curarized the integument is divided along the outer and posterior aspect of the thigh in a line which corresponds in direction with the slender biceps muscle, or rather with the groove between the muscular mass which covers the front of the femur (triceps femoris) and the bulky semi-membranosus. The sciatic nerve, accompanied by the sciatic artery and vein, lies immediately underneath the biceps, between it and the semi-membranosus. In order to separate it from the vessels it is best to bring it into view by raising the biceps on a blunt hook. . . . . . . The nerve is divided a little above the knee and the central end laid on the copper points.

Page 245.—The animal having been curarized the apparatus for artificial respiration is connected with the trachea. . . . . . The great auricular nerve is then carefully exposed, separated from the surrounding parts with the aid of two pairs of blunt forceps, and divided. . . . . If care is taken neither to prolong the excitation unduly, nor to use too strong currents, the re-action may be witnessed a great number of times in the same animal.

Page 265.—The heart of a frog having been exposed in the usual way, a stout glass rod is introduced into the æsophagus. All the other organs may now be removed in the manner directed in § 63, care being taken to avoid interfering with the venæ cavæ.

Page 269.—A frog having been slightly curarized. . . . . . the sternum is then divided in the middle line, and the two halves of the wall of the chest drawn to either side, so as to expose the pericardium and lungs, while a stout glass rod is passed down the æsophagus.

Page 271.—The preliminary steps of the experiment are those described in § 34. Loose ligatures having been placed round both vagi, and a kymographic observation made to determine the normal arterial pressure and frequency of the pulse, both nerves are divided simultaneously. [Of a dog.] . . . . . . The contractions of the heart become so frequent that the oscillations can no longer be followed by the eye, all that can be distinguished being a vibratile movement of the column. On exciting the peripheral end of either vagus the same effects are produced as in the rabbit.

Page 272.—A frog is secured in the supine position. The pleuro-peritonæal cavity is then opened, and the intestines and other viscera are removed, great care being taken not to injure the mesentery or the vessels and nerves which it contains.

Page 273.—To show this a frog is secured on its back, the pleuro-peritonæal cavity opened, and the heart exposed as before. The surface of the intestine is then smartly tapped. . . . . If the ganglionic cord is then divided on each side opposite the junction of the two aortæ, and the experiment repeated, no effect is produced, Another frog is prepared in the same way, with the exception that both vagi are divided. . . . . . . The same thing happens if, instead of dividing the vagi, the cord is divided immediately below the medulla. . . . . In a rabbit the trachea is connected with the apparatus for artificial respiration, and the vagi are exposed in the neck. Thereupon the spinal cord is divided immediately below the medulla oblongata. . . . . In the dog, section of the cord generally diminishes the frequency of the pulse. There is no such effect in the rabbit.

Page 278.—In a curarized rabbit, in which artificial respiration is maintained in the usual way, an incision is made in the middle line, extending from the upper third of the sternum to the upper end of the trachea. The external jugular vein of one side is then brought into view, tied in two places, and divided between the ligatures. The sterno-mastoid muscle is also divided between ligatures; a strong threaded aneurism needle is thrust under the sterno-clavicular ligament, and the upper fibres of the pectoral muscles; these with the ligament are divided between ligatures, and the cut ends drawn aside. . . . . The superficial parts having been exposed by two lines of incision, one of which is in the middle line, while the other extends from it on either side in the direction of the sterno-clavicular ligament, and the jugular vein having been divided between ligatures, the next step is to find the pneumogastric nerve at the upper part of the wound, and free it from the surrounding tissues. . . . . To find it, the most certain method is to seek for the trunk of the sympathetic in the upper part of the space, where it lies concealed behind the carotid artery, and then to trace it down to the ganglion. All this having been accomplished without bleeding, there is no difficulty in passing a ligature round the ganglion, so that at any desired moment it may be extirpated. . . . . . . Both ganglia having been thus prepared with as little loss of time as possible, the sympathetic and vagus are divided. . . . . . . The medulla oblongata is then divided.

Page 285.—Two frogs having been slightly curarized are prepared thus: the heart having been exposed lege artis, a small opening is made in the skin in the occipital region. In one of the frogs the brain and spinal cord are completely destroyed by passing a needle upwards and downwards from the occipital region, and then both are hung vertically on a board, side by side, looking in the same direction. [Sensibility is retained in the other.] . . . . The discharge of sanguineous liquid goes on for one or two hours, and if during the progress of the experiment, the vasomotor centre is stimulated reflexly by exciting a sensory nerve or the surface of the skin, it is seen that the rate of flow is at once augmented, but becomes less after the cessation of the excitation than it was before.

Page 289.—This may be demonstrated graphically by puncturing the anterior wall of the visceral cavity, and introducing through the puncture a cannula in such a way that it communicates with the cavity of one lung.

Page 293.—A rabbit having been secured on the rabbit support, the skin is perforated with a scalpel close to the left edge of the middle of the sternum. This having been done, the point of the tube is easily passed into the right pleura by pushing it in a horizontal direction behind the sternum, with its point against the posterior (*i.e.*, as the animal is placed the under) surface of the thoracic wall.

Page 297.—The student must avail himself of the excessive and infrequent respirations of animals in which both vagi have been divided.

Page 298.—The best view of the movements is obtained by dividing the hyothyroid membrane. The skin having been carefully divided in the middle line, lege artis, the membrane must be exposed with the aid of two pairs of forceps.

Page 306.—A ligature is passed round each nerve a little below the cricoid cartilage.

Page 308.—Rabbits in which both vagi have been divided commonly die before the end of the first day. Dogs live longer, often two or three days.

Page 309.—By far the best method is to introduce into the peritonæal cavity, by means of a small opening in the linea alba, close to the ensiform cartilage, a small flat bag of india-rubber of such size that it can be conveniently slipped between the diaphragm and liver.

Page 312.—Excitation of the superior laryngeal nerve.— The experimental investigation of the superior laryngeal is much more difficult than that of the trunk of the vagus, partly because the nerve is difficult to reach and runs a short course, partly because it is very slender. To expose it in the rabbit, an incision should be made extending from the side of the trachea at the level of its first and second rings to the hollow between the angle of the jaw and the larynx. After severing the skin in the usual way, the fascia which extends forwards from the edge of the sterno-mastoid muscle must be carefully broken through with the aid of two pairs of dissecting forceps, so as to expose the parts seen in fig. 227. The space is divided into two by the artery, the direction of which coincides exactly with that of the original incision. Near its lower end the artery gives off its thyroid branch. At the top the space is limited by the tendon of the stylohyoid muscle, and the posterior cornu of the hyoid bone. Immediately below the muscle is the trunk of the ninth nerve which arches forwards towards the tongue. The descending branch of that nerve passes downwards and forwards to reach the muscles which cover the front of the trachea, giving communicating branches to the cervical plexus, and a branch which arches forwards over the artery to gain the muscles which draw the larynx upwards. Before proceeding to expose the deeper nerves, it is well, in order to avoid confusion, to remove the descendens noni; the next step is to draw the larynx well to the side opposite to that chosen for the incision, so as to widen the space between it and the carotid artery. This done, the exposure of the superior laryngeal becomes easy. Its exact position is indicated in the figure; its course is much twisted so as to allow of the up-and-down movements of the larynx. In preparing it, no cutting instruments must be used. It must be freed from the surrounding structures with the aid of two pairs of forceps, any veins in the way having been divided between two ligatures.

Care must be taken, however, to leave a certain quantity of cellular tissue about it to serve as a kind of protective sheath, and make it somewhat less liable to get dry. The nerve having been prepared, a ligature must be tied round it as near as possible to the thyrohyoid membrane, after which it must be divided beyond. In the dog or cat the mode of preparation is very much the same as in the rabbit. In the cat, the comparative thickness of the nerve facilitates the manipulation.

In exciting the superior laryngeal, the great source of difficulty is the proximity of the vagus and the consequent liability of that nerve to be acted on by the induced current in a unipolar way. This accident which is of course fatal to the success of the investigation, the functions of the two nerves being opposite, is to be avoided, not by the use of complicated arrangements for the insulation of the nerve, but by placing it in such a way on the ordinary copper points that the part acted on is separated by a considerable air space from the surrounding tissues. Before beginning the excitation, the secondary coil must be shifted to a distance from the primary, and the primary current divided by means of Helmholtz's side wire into two branches, one of which only passes through the breaker. The other is led directly from the battery to the coil, so that the primary current is never entirely opened. In this way the opening induction shock, which, in the ordinary arrangement of the induction apparatus, possesses a much greater tension than that of the closing shock, is so reduced that the two become nearly equal to each other.*

Consequently, as the risk of unipolar action varies with the maximum intensity of the current, it is very much diminished by this contrivance, so much so, indeed, that if care is taken to prepare the nerve properly, even moderately strong currents may be used without any effects referable to unipolar excitation of the vagus manifesting themselves. Excitation of the central end of the superior laryngeal produces, according to the strength of the current used, either diminution of frequency of the respiratory movements or complete relaxation of the muscles of inspiration. The most advantageous way of judging of its effect on the diaphragm, is to expose that muscle in the way directed in § 91. It is there seen that that muscle becomes absolutely flaccid during excitation of the nerve, and it is drawn up by the elastic contraction of the lungs, so as to assume its highest possible position. When the excitation is discontinued, the relaxation either gives way to natural breathing or is immediately succeeded by one or two vigorous inspirations. If the current is so feeble that it merely diminishes the frequency of the respirations, without arresting them, the tracings show that there is no diminution of the duration of the inspiratory acts, and that the slowing is entirely due to a prolongation of the intervals, *i.e.*, of the periods during which the diaphragm remains in the position assumed by it at the close of ordinary expiration. To record the effects graphically, any of the methods recommended in the preceding paragraphs may be used. If the method described in § 99 is employed, a tracing is obtained which exactly resembles fig. 255.

The tracing fig. 256 † was drawn by inserting a bag between the diaphragm and the liver.

Page 316.—The experiment by which it is proved that the respiratory phases of arterial pressure and pulse frequency are independent of the thoracic movement consists in curarizing a dog by the injection into the venous system of a dose of solution of curare only just sufficient to paralyse the respiratory muscles.

Page 319.—For this purpose a cannula must be fixed air-tight in the trachea. . . . . .
Excessive respiratory movements in which at first the expansive efforts of the thoracic muscles, afterwards the expulsive efforts of the muscles of the abdominal wall are most violent. . . . . . .
Towards the close of the first minute the animal becomes convulsed. These convulsions must be attentively studied, because they are the type by comparison with which all other convulsions of the same order are described or defined. . . . . . Afterwards the contractions of the proper expiratory muscles are accompanied by more or less irregular spasms of the muscles of the limbs, particularly of the flexors. Early in the second minute the convulsions cease, often suddenly; simultaneously with their cessation, the expiratory efforts become indistinguishable, . . . and the animal lies in a state of tranquillity, which contrasts in the most striking way with the storm which preceded it. . . . . In these spasms which accompany the final gasps of an asphyxiated animal, the head is thrown back, the trunk straightened or arched backwards, and the limbs are extended, while the mouth gapes and the nostrils dilate.

Page 321. The respiratory movements, at first natural, are gradually exaggerated, both as regards their extent and frequency. . . . . . Towards the end of the period, as in the former case, the expiratory movements gain in vigour, both absolutely and relatively to those of inspiration, so that each inspiratory act is immediately followed by a sudden tightening of the anterior abdominal wall, accompanied by convulsive twitchings of the limbs. . . . . . Suddenly the violent expulsive efforts cease and the inspiratory movements assume the character already described, consisting in spasmodic contractions of the diaphragm, accompanied by gasping movements of the head and neck.

Page 322.—During the convulsive struggle, and particularly towards its close, the heart enlarges to something like the double of its former dimensions,—this enlargement being due to the lengthening of the diastolic interval, and to the quantity of blood contained in the great veins, which in fact are so distended that if cut into they spirt like arteries. . . . . The effect of these changes on the arterial pressure can be best studied in a curarized animal.

Page 363.—Poison a frog completely with curare . . . Dissect out carefully one of the large muscles of the thigh . . Cut away with it the piece of the pelvis to which its origin is attached.

Page 395.—Introduce between the skin of the back of a strong frog a drop or two of a solution of curare. . . . In a short time the frog will be found perfectly motionless, with its respiration arrested, but its heart still beating.

Lay bare the sciatic nerve in the thigh, slip under it a pair of electrodes connected with an induction coil and stimulate the nerve with an interrupted current. . .
If the animal has been thoroughly poisoned, no contractions whatever in the muscles of the leg will follow upon the application of a stimulus. . . . If contractions do make their appearance, the poisoning is not complete; and the student must wait or inject a further quantity of the poison. . . . Lay bare any of the muscles of the leg and apply the electrodes directly to them. Contractions will be manifest upon the application of a very slight stimulus. . . .
In a strong frog make an incision through the skin between the ilium and coccyx along the line *k.m.* fig. 266. Cut cautiously through the ileo-coccygeal muscle until the peritoneal cavity is reached. The three nerves which go to form the sciatic nerve will come into view when the sides of the wound are held apart. Very cautiously, by means of a small aneurism needle, pass a thread under these nerves, putting it under from the outside and bringing it out again on the median side. . . . Repeat the same process on the other side, passing the same thread under the nerves of that side too, but putting it in at the median side and bringing it out at the side. . . .
Tie the thread very tightly round the abdomen so as to check entirely the flow of blood to the lower limbs. All this may be done under a slight dose of chloroform . . . Pinching or otherwise stimulating either hind foot may produce movements in either one or both hind limbs, but in no other part of the body.

Pinching or otherwise stimulating the skin of the head fore limbs or trunk above the ligature may produce movements in the hind limbs, but in no other part of the body.

These facts are intelligible only on the hypothesis that the curare has destroyed (or suspended) the irritability of the motor nerves in that part of the body to which, by means of the blood current, it has had access, but has not destroyed the irritability of the sensory nerves or of the central nervous system. Pinching the skin of the fore limb gave rise to an afferent nervous impulse, which, either by volition or by reflex action, gave rise in turn to efferent impulses, which were unable to manifest themselves through the poisoned motor nerves of the fore limbs and trunk, but found vent through the unpoisoned motor nerves of the hind limbs. In order to bring out these results well, the dose of poison must not be more than sufficient to poison the motor nerves. . . . . In a fresh strong frog lay bare the sciatic nerve on one side, . . place a ligature under it, near where it divides into two branches, and tie the ligature tightly round the leg above the knee. The circulation of the lower right leg will thus be completely arrested; but, inasmuch as the nerve is not included in the ligature, there will be complete nervous connection betweeen the right lower leg and the rest of the body. Poison with curare . . . . . In a fresh strong frog dissect out a gastrocnemius (or any other single muscle), dividing both insertion and origin, and ligaturing its blood vessels, thus leaving it connected with the rest of the body by its nerve only. Poison the frog with curare.

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* For a fuller explanation of the difference between the two induced currents and of the effect of Helmholtz's modification, see Rosenthal, "Electricitätslehre," p. 120.

† The tracing fig. 256, shows that during the whole period of excitation the diaphragm remained motionless in the position of expiration, with the exception that at gradually lengthening intervals it executed momentary contractions. When after the cessation of excitation the respiratory movements were resumed, they were slower but more extensive than before.

Page 400. The results are most clear and distinct when the organs of consciousness are intact, and the ordinary tokens of sensation are used to determine whether the impulses caused by stimulation of the peripheral terminations reach the conscious central nervous system or not. But the facts may also be readily shown in the absence of the brain, when reflex action is taken as a proof of a centripetal impulse having reached the spinal cord. In the former case, the frog should be placed under chloroform during the laying bare of the roots. In the latter the medulla should be previously divided in the neck. The frog being placed on its belly, make an incision in the middle line of the back, from the upper end of coccyx to the level of the fore limbs. Having hooked back the flaps of skin, carry the median incision down to the spines of the vertebræ and dissect away the longitudinal muscles on either side, so as to lay bare the bony arches, and then hook back the muscles on either side, or cut them away altogether.

With a small but strong blunt pointed pair of scissors cut through, on either side, the arch of the last (eighth) vertebra (be careful not to thrust the scissors in too deep), and remove the piece so loosened. Proceed then to the next arch above and so remove, three arches. The roots of the nerves will be seen lying in the spinal canal. Snip away the remains of the arches, on each side, until the last three (or four) roots are quite clear, being very careful not to touch the nerves with the scissors. . . . The frog being completely at rest, draw the ligature tight, observing the frog all the while. If the animal be in good condition, some movements will be visible in some parts of the body as evidence either of sensibility or reflex action. Now cut the nerve between the ligature and the cord: some movement will probably be again witnessed.

Lift the peripheral stump of the nerve carefully up by means of the ligature, and slip it upon the curved shielded electrodes which may be held in the hand, or, better, fixed on a movable stand. To prevent any escape of the current, slip a fragment of india-rubber sheeting beneath the nerve and electrodes, so as to isolate these from the cord and from the rest of the nerves. Pass a moderately strong interrupted current through the electrodes. If there be no escape of the current the animal will not move in the slightest.

Repeat the observation with the nerve root next above (the 8th), with this difference: place the ligature as near as possible to the walls of the spinal canal; divide the nerve between the ligature and the wall, and place the central instead of the peripheral stump on the electrodes.

Ligature and section as before produce movements. A very moderate current applied to the central stump will produce very considerable movements in various parts of the body, *i.e.* signs of sensation or reflex action, as the case may be.

Ligature or section of the posterior roots of spinal nerves produces movements in various parts of the body. Stimulation of the peripheral stump produces no movement whatever; stimulation of the central stump produces considerable movements.

These movements, be they simple reflex actions or more complicated voluntary movements set going by conscious sensations, are evidences of centripetal sensor impulses, excited in the posterior sensory roots.

Page 402.—In a fresh strong frog lay bare the roots of the spinal nerves and divide the posterior roots of the 7th, 8th, 9th, and 10th nerves on the right side and the corresponding anterior roots on the left side.

The left leg will remain motionless, being simply dragged along by the rest of the body, but never moving of itself. (If the brain has been previously destroyed or separated from the spinal cord, the right leg will be drawn up as usual, but not the left leg.)

Page 403.—Recurrent sensibility. This is never witnessed in the frog. It can only be shown in the higher animals, the cat or dog being best adapted for the purpose. The method adopted is very similar to the above, the arches of one or two vertebræ being carefully sawn through or cut through with the bone forceps, and the exposed roots being very carefully freed from the connective tissue surrounding them. If the animal be strong and have thoroughly recovered from the chloroform and from the operation, irritation of the peripheral stump of the anterior root causes not only contractions in the muscles supplied by the nerve, but also movements in other parts of the body indicative of pain or sensations. On dividing the mixed trunk at some little distance from the junction of the roots, the contractions of the muscles supplied by the nerve cease, but the general signs of pain or of sensation still remain.

Page 411.—In a frog with divided medulla, ligature the hind limbs, leaving the nerves free as directed in chap. 31 for curare, and afterwards inject a small dose of strychnia.

Demonstration of the functions of the chorda tympani and sympathetic fibres of the submaxillary gland in the dog. The animal was placed under the influence of the chloroform, when no less than 15 different dissections were made to obtain a fistulous opening. After which the author says, p. 473, "The animal must previously be " allowed to recover from chloroform, or no increase [of " saliva] will be observed."

Page 474.—After division of both nerves the secretion of the submaxillary gland, which in the normal state only goes on when the gland is directly or reflexly excited, becomes constant and abundant. This effect does not occur until some time after section, and may last for days or weeks. A similar condition of the gland is produced by the introduction of curare into the blood which is supplied to the gland by its arteries.

Page 477.—Give the dog a hearty meal, so as to distend its stomach completely, and make it lie close against the intestinal walls. Anæsthetise the animal by chloroform, taking care that the vapour is mixed with a sufficient proportion of air. Lay it on its back on the table, shave off the hair from the epigastric and hypochondriac regions, and remove the hairs carefully by a sponge, so as to prevent the risk of their getting into the peritoneal cavity. Make a vertical incision, about an inch and a half to one side of the linea alba, preferably the left, and parallel to it, extending downwards from the lower edge of the costal cartilages to a distance somewhat less than the diameter of the flange of the cannula. Divide the muscles parallel to the course of their fibres. Tie every bleeding point before opening the peritoneum, so that no blood shall get into its cavity. Open the peritoneum on a director. Lay hold of the stomach with a pair of artery forceps at a point where there are not many vessels, and draw it forwards. Pass two threads with a curved needle into the gastric walls, at a distance from each other about equal to the diameter of the tube of the cannula, and bring them out again at a similar distance from the points where they were introduced. Make an incision into the gastric walls, between the two threads, rather shorter than the diameter of the tube of the cannula. Put a pair of forceps, with the blades together, into the incision, and then dilate it by separating the blades till it is large enough to allow the cannula to be introduced. Push the cannula into the stomach up to its outer plate. Tie the stomach to it by the threads, and then pass their ends through the edges of the wound in the abdominal wall in such a way as to fasten the stomach to it, and at the same time to keep the cut edges in apposition. No other suture is required. Leave the cannula uncorked for at least half an hour after the operation is finished, for when the dog recovers from the chloroform it will vomit, and if the cannula be corked the fluid contents of the stomach are apt to be forced passed the side of the cannula into the abdominal cavity. Feed the dog on milk for one or two days, and if the operation be performed in winter, keep it in a place warmed night and day. The day after the operation the edges of the wound will be much swollen, but the swelling will subside in a day or two. After the wound has begun to heal, the cicatrix may thicken, and the outer plate of the cannula begin to press too much on the skin, so that it ulcerates. If this should occur, the cannula must be lengthened by screwing the two flanges further apart. The cannula may be closed by an india-rubber stopper, or by a cork. If the dog tears out the cork with his teeth, soak it in decoction of colocynth, or put a little phosphoric acid on its outer end. In order to collect the juice, let the animal fast for several hours, so that its stomach may be quite empty, but not for more than a day, as the mucus membrane would become covered with a thick coating of mucus. Let an assistant pat the dog and keep him quiet, with draw the cork from the cannula, and tickle the inside of the stomach with a feather tied to a glass rod. Put a small beaker underneath, so that the end of the rod rests on its bottom: the gastric juice will flow into it down the sides of the rod.

Page 494.—This may be still better seen in the cat. . . . . To show these facts, a cat must be placed under chloroform, after which both vagi are prepared and the stomach exposed. If now the animal having partially recovered from the anæsthetic, the stomach is seized between the thumb and forefinger, and subjected to traction in the direction of its length, slight but unequivocal signs of uneasiness are perceived. The vagi are then divided.

Page 499.—To get the urine, hold the rabbit over a large beaker, compress the abdomen with the palm of one hand, and press with the thumb of the other on the bladder just above the pubes, pushing it well down into the pelvis.

Page 505.—Chloroform the animal, and secure it on the rabbit-support. Make an incision from an inch to an inch and a quarter long through the abdominal parietes in the linea alba from the xiphoid process downwards. The pyloric end of the stomach is thus exposed. Pull gently on the stomach until the duodenum is brought into view. The part corresponding to the superior transverse part in man forms a loop with its convexity directed towards the diaphragm, into the top of which convexity the ductus choledochus enters. Tie the duct in this situation, then seize the gall bladder with a pair of forceps. It is always full, and cannot be missed if the forceps are passed immediately under the edge of the costal cartilages. Make a small incision into the gall bladder, introduce a cannula and tie it in. The diameter of the cannula should be from two to three centimeters, and the end to be inserted should have a projecting rim. This can be made very readily by heating the end of a piece of glass tubing of the proper size, and pressing it, while hot, against a flat piece of iron. Sew up the wound, leaving the free end of the cannula outside. The bile in guinea-pigs is secreted in very large quantities, being as much as 7·3 grammes in an hour per kilogramme of body weight. It contains a very small proportion of solids, about 1·3 per cent. When the bile duct is tied the guinea-pigs die in less than 24 hours, but when it is not tied they will live for a week.

Page 515.—Place a rabbit in the prone position on Czermaks' rabbit-support, and fix the head to the upright at the side. Feel for the occipital protuberance, and make an incision over it, about half an inch long. Fix the point of the chisel in the middle line of the skull, just behind the protuberance, and bore through the bone, moving the handle of the instrument from side to side, in order to assist its passage, but not pressing with too great a force. When the skull has been penetrated, push the chisel downwards and forwards through the cerebellum in such a direction as to cross a line joining the two auditory meatus until it is stopped by the basilar process, and then gently withdraw it. Remove some of the urine in half an hour or an hour afterwards, and test it for sugar.

Page 516.—Bernard prefers for the purpose large dogs, sheep dogs being best, as they are less subject to peritonitis than others. Five or six hours before the operation the animal should get a large meal of bread and meat. The operation, which must be performed as quickly as possible, consists in laying the dog on its left side, and making an incision 5 centimeters long in the right hypochondrium. . . . . . The duodenum lies opposite the wound. As soon as it is exposed it is drawn out. . . . . . . The duct is opened with scissors, and a plain silver cannula, about 5 millimeters in diameter and 10 or 12 centimeters long, pushed into it up to its first division.

Page 517.—For permanent fistulæ, Ludwig and Bernstein choose small dogs, as in them the duodenum is more easily reached from the middle line, and is not drawn so far from its natural position by the fistula as in larger animals. The dog must be kept fasting on the day of the operation, as the pancreatic vessels are full during digestion, and bleed easily. Narcotize the animal by injecting opium into the tibial vein, and open the abdomen by an incision about two centimeters long in the linea alba, midway between the ensiform cartilage and the umbilicus. The duodenum is then searched for and drawn out of the wound along with the attached pancreas, and a thread looped round the duct. Instead of then putting in a cannula a piece of lead wire is inserted into the duct, so that one end of it passes into the intestine, and the other into the gland to a considerable distance. The middle part of it is twisted together and projects through the wound. Owing to the T shape thus given to the wire, it cannot either slip out or move about in the duct; but wire being chosen which does not fill it up, the flow of the juice is not hindered. Three threads having then been passed through the wall of the duodenum near the duct, the intestine and omentum are replaced in the abdomen, and the duodenum fastened by the threads to the abdominal wall. The wound is then sewed up, care being taken that the twisted part of the lead wire passes through the wound. Twenty-four hours after the operation the stitches are taken out, but the wire left in. In two or three days afterwards the juice can be collected. For this purpose the animal must be supported by two straps which pass under its belly, and are attached to a horizontal bar hung from the roof by a cord and pulley. The dog is thus suspended over a table, at such a height that it can barely touch it with its toes, in which position it remains perfectly still.

[The second volume of this work consists of elaborately drawn plates and keys designed to assist "beginners" in manipulation.]

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[Address.] T. Cooke, Esq., M.D., 30, Gower Street Bedford Square.

D.

OPINIONS MORE OR LESS AGAINST VIVISECTION.

Sir Charles Bell, *Spectator*, Feb. 6, 1875, p. 167, No. 2,432.

Times, Aug. 13, 1863.

Letter of *Dr. Ebenezer Watson* to *The Glasgow Herald*, April 13th, 1875.

Dr. J. J. Garth Wilkinson, Letter to "Morning Star," August 20th 1863.

Dr. Abernethy's Lecture, No. 4, pp. 164 to 166.

Abernethy's Lecture, No. 1, p. 31.

Dr. J. B. Yeo, *British Medical Journal*, No. 699, p. 671. (*Clinical Lecture.*) [*See also* Langley, p. 7; Harley, p. 8; Marcet, Thorowgood, and Lanat, p. 25; and Moore and Reynolds, p. 39.]

Dr. Murchison, *British Medical Journal*, No. 696, p. 569. [*See also* p. 21.]

Mr. Wagstaffe, *British Medical Journal*, No. 692, p. 454.

Mr. Reynolds, British Medical Journal, No. 711, p. 203.

Frederic D. Dyster, M.D., British Medical Journal, No. 734, p. 126.

Dr. Crisp, British Medical Journal, No. 747, pp. 558–9. [*See also* pp. 8 and 24.]

Dr. Brunton, Lancet, No. 1,941, p. 456.

George Rolleston, M.D., F.R.S., Linacre Professor of Anatomy and Physiology, Oxford, *Lancet,* No. 2,345, p. 182.

Dr. Epps, Daily News, January 13th, 1864.

Dr. Powell had, however, so far as he could remember, in his experiments on a dog obtained results similar to those of Dr. G. Johnson and others. He wished, however, to draw attention to the fact that the same phenomena were not observed in experiments upon different animals. In a series of experiments which he had commenced last summer, he had found that in the cat, instead of closure, powerful abduction of the cords was obtained on galvanising the recurrents; and he thought it very important that we should ascertain which animal man resembled as regards the innervation of the larynx, in order to understand better the mechanism of nervous dyspnœa in aneurism. If man resembled the dog or the rabbit in this respect he might have spasm of the glottis as the result of irritation of the recurrent nerve; but if he resembled the cat, the so-called spasmodic dyspnœa, when of purely nervous production, was really due to paralysis. And it was even possible that a ready means of relief might be found in galvanism, for nothing could be more striking than the instant relief afforded to the dyspnœa in the cat when the divided recurrents were galvanised.

. . . .

Dr. Johnson, in reply, said there must be some fallacy in Dr. Reid's experiment quoted by Dr. Powell; it did not agree with Dr. Powell's results on the cat, which were in accord with those of Dr. Rutherford.—*Dr. G. Johnson, British Medical Journal,* No. 729, p. 791–2.

Dr. Day, Lancet, No. 2,682, p. 120. [*See also* p. 15.]

Experiments which have been made on the physiological action of codeia tend to show various results. These in some measure are due to the experiments having been conducted on animals of different species, genera, and classes. Some, and those most reliable, have been on man; and, as it is for human benefit that we prosecute these researches, it would be better if further secondary experiments were always so conducted.

. . . .

Dr. Stocker injected some cats subcutaneously with hydrochlorate of codeia. In each instance the pupils were dilated; cerebral congestion was present, as determined by ophthalmoscopic examination; there was much reflex excitability; in one case, epileptic convulsions; salivation and purging occurred in two cases; there was no vomiting. The dose used was about a grain and a half.

In the case quoted there are several points of disagreement with these results, which are probably due to the difference in the animals—one a man, the other a cat. Thus the cat's pupils were dilated, the man's contracted, and, while there was no increase of alvine or salivary secretions in the man, two of the cats were salivated and purged.—*Mr. Moore, British Medical Journal,* No. 696, pp. 576 and 577. [*See also* Langley, p. 7; Harley, p. 8; Marcet, Thorowgood, and Lanat, p. 25; Yeo, p. 38; and Reynolds, p. 39.]

Dr. Savory, Lancet, No. 2,001, p. 30.

A protest signed by Dr. James Syme, and Mr. William Wilkinson, Principal Veterinary Surgeon to the Forces.—"*Times,*" 20th April 1867.

James Syme, Jan. 30th, 1869. — *Lancet,* No. 2,371, p. 194.]

Richard Congreve, M.R.C.P.
J. H. Bridges, M.B., F.R.C.P.
Fortnightly Review, March 1875.

Professor Simonds address at the opening of the winter session, Royal Veterinary College, London, October 1875.

British Medical Journal, No. 744, p. 454–5.

Professor Sir William Thompson, Glasgow Herald, March 30th, 1875.

Professor Nichol, Glasgow Herald, March 30th, 1875.

"Vivisection, is it necessary or justifiable?"

Dr. Markham's Prize Essay on Vivisection, published by the Royal Society for the Prevention of Cruelty to Animals, pages 101 and 102.

Professor Owen, ["Vivisection, is it necessary or justifiable?"] page 112.

Dr. Bigelow, Professor of Surgery at Harvard University, being extract from the annual address read before the Massachusetts Medical Society, 1871.

British and Foreign Medico-Chirurgical Review, No. 110, p. 283.

The meeting of Convocation of the University of London has this year been remarkable for two things. The first was the passing of a resolution recommending the admission of women to degrees. The second was the refusal to entertain a resolution requesting the Senate not to allow painful experiments on living animals to be carried on at the Brown Institution.

This latter resolution was a direct blow aimed at vivisection, and was moved in a most able speech by Mr. Hutton, M.A., a gentleman of great influence. Only sixteen voted with him and fifty-nine against,—a result, we believe, partly due to the Chairman reading a remarkable letter from Dr. B. Sanderson, the learned Superintendent of the Brown Institution, who stated to our amazement that the laboratory was not used for physiological inquiries, only for investigations into the nature and treatment of disease. It was necessary, he added, to perform small experiments, such as the abstraction of a little blood, &c. We have expressed surprise at this statement. Can it be possible that Dr. Sanderson is ignorant of the fact that physiological teaching is carried on at the Brown Institution, and that such teaching is based on physiological experiments of the most painful kind? The controversy on vivisection has been best handled in the columns of the "Spectator," and most men have come to the conclusion that for proper purposes, within certain limits, vivisection is justifiable, but that anæsthetics should be employed whenever possible. Indeed, it has been urged by many that pain is very seldom inflicted, because the use of anæsthetics is universal. Now, this we deny. In several places in London most painful experiments have been recently performed without producing anæsthesia, and the Brown Institution is one of them. Indeed, after Dr. Sanderson's letter, we think it right to assert that, whether he knows it or not, the use of anæsthetics is the exception, not the rule, in the institution.

We take the liberty, too, of describing two of the experiments lately done, and we suppose frequently done, for teaching purposes, in order that Mr. Hutton may at least have a faint idea of the facts he might easily collect.

First experiment.—Take a lively frog and tie his four legs to a square board, stretching the legs so that he cannot writhe; crucify him, in fact. Then make two cuts, one on each side of the sternum. Another cut across the breast bone is the next step. Then cut through the bone, and take it right out.

The next step in the vivisection is to make a cut the whole length of the abdomen, so as to open the cavity. The end of the heart, which is now visible beating, is to be cut off. Through the opening thus made in the heart push a cannula right up to the bulbos aortæ. Now open the abdominal vein. All this in preparation for the experiment itself, which is thus proceeded with: Inject warm distilled water into the cannula until the whole of the blood has been washed out of the frog, when he will appear to be dead, and swollen up to about four times his original bulk. The injection takes some ten minutes. After the water an injection of silver nitrate is to be made.

Second experiment.—Press your finger under a frog's mouth until the eyeball protrudes. Then pull down his nictitating membrane with a pair of forceps. Next, scrape off with a sharp knife the three layers of anterior epithelium. Now let him rest ten minutes. You may then observe his actions; he pokes his head down between his fore legs, jumps, or turns over, wriggles, and otherwise acts in a strange manner.

Having watched this, take him again and push out the eyeball once more. Again hold down the protecting membrane, and rub a stick of solid lunar caustic all over the eye until the aqueous humour of the anterior chamber shows a precipitate. Now release your frog, and his actions will be similar to those previously watched, but more intense; in fact, he plays such fantastic tricks as few could look upon without that blunting of sensibility on which so many have dilated. It is necessary for him to be left for ten minutes, after which his head is cut off with a pair of scissors, and so his suffering ended.

The object is, to demonstrate the structure of the cornea by staining the intercellular substance. We do not propose to assert that these experiments are useless, but we do say that they could be just as easily performed under the influence of anæsthetics. If Mr. Hutton has any doubt that they are done, we have none. We beg to say that they have been done since his speech in Convocation, since Dr. Sanderson's letter, and done, too, without chloroform or any other anæsthetic.

We have no desire to put an end to scientific investigation, but needless cruelty seems to require curbing, and Mr. Hutton is fighting in a good cause. If he wants details of other experiments, he has only to read the "Handbook" edited by Dr. Sanderson himself, and be informed that that work is used as a text book at the Brown Institution. So it is in most of our medical schools. In the majority of the latter chloroform is used.—*The Doctor*, June 1st, 1874, p. 101.

In reference to the above, Dr. B. Sanderson wrote to *The Doctor*, July 1st, 1874, as follows:—

"Sir,

"My attention has to-day been drawn to an article "in your journal of June 1st, headed 'Vivisection,' in "which you comment on the management of the laboratory "of the Brown Institution. All that is necessary for me "to say with reference to the article is, first, that so far as "relates to the laboratory of the Brown Institution, the "assertions contained in it are untrue; and secondly, that "the statements with reference to this subject which were "embodied in the letter to the Chairman of Convocation "of the University of London, to which you refer, are "perfectly correct.

"Your obedient Servant,"

"The Brown Institution, J. Burton Sanderson.

"June 26th, 1874."

[We are amazed at this letter, which we invite the writer to reconcile with the facts. The assertions contained in our article are true. We did not comment on the management of the "laboratory." Does Dr. Sanderson mean to deny that the experiments we described in *The Doctor* for June 1st have been performed in the Brown Institution? We deliberately repeat that within the walls of the Institution such experiments have been frequently performed.—*Ed. Doctor.*]

The Doctor, February 1875, pp. 21–2. [*See also* p. 37.]

X. Dec. 8th.—Letter to *Nature*, No. 216, p. 121.

Nature, August 13th, 1875.

The Gazetta Italiana di Milano contains an essay of Professor Mantegazza on experiments carried on under his direction at the laboratory of experimental pathology of the University of Pavia. It will suffice to state that the experiments were intended to study the action of pain on digestion and nutrition. They were, as the Professor himself confesses, agonizing to the animals subjected to them and distressing to the experimenters, and simply proved that loss of appetite, great weakness, and a peculiar imbibition of moisture were the result of the pain inflicted. It is added that no alteration of the spinal marrow could be detected after the agony had been protracted for one month very meagre results of unpardonable cruelty.—*Lancet*, No. 2,482, p. 415.

Lancet, No. 2,086, p. 224-5.

Lancet, No. 2,627, pp. 22 and 23.
Lancet, No. 2,656, p. 139.]

Lancet, No. 2,712, p. 286.

Lancet, No. 1,928, pp. 143–4.

Lancet, No. 1,938, p. 396.

Lancet, No. 2,684, p. 204.
Lancet, No. 2,677, p. 877.

The Times (Paris Correspondent), August 8th, 1863.

Report of the Committee appointed at the Liverpool Meeting (held 1870) of the British Association to consider the subject of Physiological Experimentation. (Edinburgh: Volume 1871 of British Association Reports, p. 144.) *Medical Times and Gazette*, Feb. 25, 1871.

LONDON:
Printed by George E. Eyre and William Spottiswoode,
Printers to the Queen's most Excellent Majesty.
For Her Majesty's Stationery Office.

DIGEST OF EVIDENCE

TAKEN BEFORE THE

ROYAL COMMISSION

ON THE

PRACTICE OF SUBJECTING LIVE ANIMALS TO EXPERIMENTS FOR SCIENTIFIC PURPOSES;

WITH AN

ALPHABETICAL LIST OF WITNESSES.

Presented to both Houses of Parliament by Command of Her Majesty.

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DIGEST OF THE EVIDENCE.

Sir THOMAS WATSON, Bart., M.D.

Physician in Ordinary to the Queen, formerly President of the Royal College of Physicians, and member of the Medical Council, 1.

Has been 50 years in the medical profession, 31. Has published a work on vivisection, and has bestowed much thought on the subjection of live animals to scientific experiments generally, 2–6.

equency experi-nts.

Cannot say whether experiments on living animals have grown more frequent of late or not, 104–5. Believes they are more common abroad than in England, 117. Has never witnessed any such experiments himself, 32–3, 62; but has made himself acquainted with the results of Majendie's and of other similar experiments, 64–5.

Considers the infliction of animal pain is a great evil, only justifiable when there is a direct prospect of greater good, or of solving a doubt, 91–3.

tigation human ffering.

Believes, however, that knowledge is obtainable at a small expense of animal suffering whereby human pain may be mitigated and even life itself preserved, 8; and is of opinion that the present state of surgical knowledge could not have been reached without such experiments, 34.

scovery of e circula-on of the ood.

Results obtained by Experiments.—Although the discovery of the circulation of the blood was verified, rather than made by vivisection, 84; it was aided and perfected by Harvey's innumerable vivisections, 35; and microscopic investigations into the hearts of animals, 85–8. From this discovery the cure of aneurism followed, and Harvey was enabled to get rid of a dangerous tumour by tying the artery feeding it, 35.

the treat-ent of eurism.

the func-ons of the rves.

The experiments of Sir Charles Bell and Dr. Marshall Hall on the nervous system enabled drugs to be applied in new ways for the soothing of pain and the diminution of danger to life, 35.

Induction of Disease.—Believes experiments as to the induction of disease are designed to ascertain the exact cause of the disease with a view to its prevention, 98; has no personal knowledge of the subject, 99; but thinks them of but little use, 61. Is not aware of any experiments having been made with regard to the induction of disease on grouse or other animals, 96–7.

Pharmacological Experiments.—New drugs and remedies are tried on animals, but the effect on animals often differs widely from that on man, and experiments may be therefore at once painful and misleading, *35f, g*, 58; a drug which will poison a dog may not be poisonous to a man, 57. Experiments are, however, sometimes made on animals with the view of ascertaining the action of poisons, and of bringing the results to bear in legal investigations, *35e*.

edical rispru-ence.

itigation animal ffering.

The knowledge of the effect of a drug on a single individual animal may save many animals from pain, 59; and such experiments are justifiable therefore, 60; as also giving animals special diseases, 61.

Limits of Experiment.—Considers experiments on live animals are necessary, 41; and justifiable, but only when undertaken with a definite object, and performed by a skilful anatomist with proper apparatus and after due study and preparation, 9–10, 13–16, 18, 71. Experiments made simply to see what will happen are inexcusable, 12.

xperi-ents by competent ersons.

Has no knowledge as to the extent to which experiments are performed by incompetent persons, *35h*; but thinks they must generally be useless and should therefore be put down, 37–8; there are very few persons in the country who are competent, or who should be allowed, to conduct such experiments, 106.

or teaching urposes.

Experiments for the sake of demonstrating accepted facts to classes should be forbidden, 39–40; after once the result is established experiments should not be repeated, 17; such repetition is cruel and useless, 67–70; complicated experiments often prove nothing and should not be allowed, *35i*–36.

n the iving issues.

Does not approve of such experiments as those for the purpose of studying the living cellular tissues unless a definite object which would alleviate human suffering is in view, 89–90.

or surgical ractice.

Doubts whether the practice of surgical operations on a live animal would be of any use to the student and thinks they should not be allowed, 72–3.

Watson, Sir Thomas.

Sensibility of animals.

Does not know whether the sensitiveness of the different orders of animals to pain varies, and cannot tell how it could be ascertained; some, however, think the highest orders feel most acutely, 100–3.

Harvey's experiments.

Anæsthetics.—A new era in experimentation commenced with the introduction of anæsthetics, 35; when Harvey examined the double hearts of vertebrate animals and the single ones of molluscs and insects, *35a, b*, anæsthetics were not known, many of his experiments would now be performed painlessly, *35b, c*.

Nearly all experiments may be made entirely or comparatively painless by the use of anæsthetics, 42–3; even if the manifestation of pain is essential to the success of the experiment the amount of suffering can be lessened, 19–24.

Cannot always be used.

The use of anæsthetics would frustrate the objects of many experiments on the nerves and possibly of some experiments as to the action of poisons; anæsthetics themselves being to a certain extent of the nature of poisons, 53–6; but witness is not aware of any other cases in which they would interfere with the result, 54.

Experiments on th nerves.

In Sir Charles Bell's experiments the nerves of motion were distinguished from the nerves of sensation by the presence or absence of pain, and the use of anæsthetics, had they been known, would have defeated the object of the experiment, 47–50; lingering pain, however, was not necessary, 51–2; considers that it is never necessary, 25–6, or justifiable, 44; by lingering pain means pain felt after the influence of the anæsthetic has departed, 94–5.

Effects on lower animals.

Has no knowledge of the effects of anæsthetics on the lower animals, 74.

Chloroform.

Considers that the use of anæsthetics should be compulsory, 29–30; chloroform is the anæsthetic best understood, 63.

Curari.—The effect of the curari or worari poison is to deprive an animal of motion, and it therefore might be used to facilitate an experiment, but to deceive lookers on by saying it destroyed sensation also would be a fraudulent trick, 75–83.

Licenses to perform experiments.

Regulation of Vivisection.—Thinks that licences should be granted by the Home Secretary or some other high authority, on the professional recommendation of the Inspector of Anatomical Schools or some similar adviser, 107–111, 113–4; licences should be general, not for particular experiments only, 118; and should be subject to revocation if abused, 119.

Thinks that it would be an invidious task for either the College of Physicians or College of Surgeons to undertake the granting of such licences, 115; but observes that his views with regard to the licensing system have been in his mind only since talking the matter over that morning with Sir George Burrows, 121.

Cruelty to Animals Act.

Has not considered whether it would suffice to strengthen the present Cruelty to Animals Act and throw on the experimenter the *onus* of proving that he himself was a fit person, and the object of the experiment a proper one, 120.

Inspection of physiological laboratories.

Would prefer to have an inspector of physiological laboratories like the inspectors of dissecting rooms and schools of anatomy, and would leave the rest to the conscience and humanity of the experimenter rather than attempt to place restraints by law, 116.

Both branches of the medical profession are entirely desirous of mitigating pain, and would concur in any legislation having that result, 27–8, 45–6.

Sir GEORGE BURROWS, Bart., M.D.

President of the Royal College of Physicians, late President of the Royal Medical and Chirurgical Society, &c., of the General Medical Council of the United Kingdom, 122–4; and for 30 years was the physician and principal medical teacher at St. Bartholomew's Hospital, 180.

Nature of experiments made by witness.

Has witnessed and has performed some operations on live animals, 125; the object of the latter was to elucidate the condition of the blood vessels of the brain under different modes of dying, 126. Death in all cases ensued in less than five minutes, 127–8, 130; sensation would cease before death, 130; and the pain produced would be less than from the ordinary methods of killing, 131.

Of those seen by witness.

The experiments seen by witness were performed by Dr. Charles J. B. Williams and by the late Dr. Hope to determine the cause of the sounds of the heart in health and in disease, and they resulted in the introduction of improved method of treatment of heart disease, 132–3. The animals were rendered insensible before being operated upon by curari, 132.

Physiological Handbook.

Frequency of Vivisection.—Has no personal knowledge as to how much vivisection is practised out of London, 181; but knows that the number of experiments made at St. Bartholomew's in 30 years was very small. Dr. Legg's experiments on the biliary fistula of cats were made subsequent to witness leaving Bartholomew's Hospital, 227. Cannot give much information as to Majendie's visit to this country, 257–8. Does not believe that medical students practise much vivisection, 204. Is aware of the existence of a "Handbook for the Physiological Laboratory," which would seem to imply that these experiments are more frequent than witness is aware of, but it has only recently appeared, and is, in witness' opinion, for teachers rather than students, 205–7.

Antiquity of vivisection in England.

Vivisection is no new thing in England; in addition to Harvey's private experiments he demonstrated the circulation of the blood by experiment on the living animal to Charles I., his queen, and the princesses, and the animals in the royal parks were placed at his disposal for his investigations, 138.

Development of practical physiology.

The experimental method is thought more highly of than formerly, and is growing in physiological laboratories, although not in medical schools, such as St. Bartholomew's, 230–1; and physiology is now practically taught in a way in which it was not taught before, 232.

Improved treatment.

Results obtained by Experiment.—These experiments increase knowledge, and increased knowledge leads to the alleviation of human suffering, 233; has himself seen improved methods of treatment result therefrom. Considers such experiments are necessary for the progress of medical science, 134; thinks Sir Charles Bell's expressions of reluctance to perform certain experiments, on the ground of the pain they must inflict, probably refer to some performed by Majendie about the same time, 221–2.

Sir C. Bell's nerve experiments; benefits derived therefrom.

Sir Charles Bell's experiments on the facial nerve would not cause more pain than the prick of a spur, 141–3; but yet they resulted in enabling a facial paralysis proceeding from a temporary cause to be distinguished from a serious one proceeding from a diseased brain, 142; previous to this discovery the facial nerve was often divided by surgeons to cure tic doloreux; the knowledge that it was not a nerve of sensation showed this to be useless, 215.

Dr. Marshall Hall.

The reflex action of the spinal cord was discovered by Dr. Marshall Hall's experiments in which the spinal cord was severed, and the functions of the part below the section ascertained, and symptoms which formerly were referred to disease of the brain were found to proceed from the state of the spinal cord; gives instance, 184–7.

Claude Bernard.

The vivisections performed by Claude Bernard established the fact that, in addition to the secretion of bile, the liver elaborated the materials which form the saccharine principle of the blood, 188; a result which could probably have been arrived at in no other way, 189–90. Such experiments could not in the main have been performed on the human subject without resulting in death, 191. Harvey's discovery of the circulation of the blood was merely an addition to scientific knowledge at the time, the important advantages resulting from the discovery were not foreseen, 244–8.

Sounds of the heart.

Admits that it may have been within the limits of possibility that the discoveries as to the sounds produced by the heart might have been arrived at by the comparison of a large number of carefully tabulated cases; but points out that as a fact no such result was obtained by that method, 214.

Necessity of experiments.

Limits of Vivisection.—The cases in which vivisection is necessary are very rare, 174; it is, however, sometimes necessary both for the progress of knowledge and for medico-legal investigation, 153–4.

Experiments for teaching purposes.

Experiments for the sake of illustrating known truths in the schools should not be allowed, 139, 175, 199; nor should experiments made from pure curiosity, 239–40.

Is of opinion that if there is a reasonable prospect of an experiment advancing knowledge, settling a discordance of opinions, for instance, it should be performed irrespective of any ulterior benefit being directly attached to it, but should be done with proper precautions, 235–9, 242.

Competent men only should be allowed to experiment.

Would separate experiments of this kind from all others; students should be deterred from performing them, although it is conceivable that a young man with a thorough knowledge of the science might advance knowledge, 211, 700–3; as a rule, thinks that persons who have not a thorough knowledge of anatomy and physiology ought not to be allowed to undertake these experiments, although by chance a useful result might be arrived at, 149–51, 198.

Induction of disease.

Considers the induction of disease, tubercle for instance, for the purpose of elucidating the causes and conditions of the disease quite justifiable, although attended with suffering, 216–8.

Action of drugs.

Approves of ascertaining effects of drugs and poisons by experiment on animals, and thinks that such experiments have been most useful, 219–20; although the knowledge obtained is necessarily imperfect; 259–61.

Anæsthetics.—The majority of experiments could be, and ought to be rendered painless, 160, 249–50. The experiments performed by Harvey in his researches, touching the circulation of the blood, for instance, might now all be rendered painless, 38.

Experiments on the nerves necessarily painful.

Some experiments, however, cannot be performed under the influence of anæsthetics, 162; some on the nervous system are necessarily more or less painful, all others can be performed without causing pain, and in those which cannot, such as dividing the spinal cord, the more painful part, the actual cutting, can be done while the animal is under anæsthetics, 144, 192–5, 251; many experiments on the nerves are, however, not of a serious nature, 140; is not a scientific physiologist, but doubts whether any means exists of rendering all experiments on the nervous system painless, 196–7.

Physiological Handbook.

The experiment referred to in the "Physiological Handbook," and entitled "excitation and section of the spinal cord in the rabbit," would be both lingering and painful, cannot say whether anæsthetics would interfere with its object, 208–10.

Painful experiments on frogs.

Does not know whether placing frogs under the influence of curari, slitting them open, dragging out the mesentery and placing it under the microscope for two hours, would be justifiable or not without seeing the experiment and knowing all the conditions, 243; the amount of pain justifiable must depend on the importance of the object sought, 145.

Can give no information as to the effect of anæsthetics on frogs, 228–9.

Curari appears to render animals insensible, but is not an anæsthetic because it destroys life, 135–7; if it can be shown not to destroy sensibility it should not be used, 212–3.

Licences.

Regulation of Vivisection.—Suggests the licensing of particular places for such experiments, 173; and thinks that the few operations that cannot be performed without pain should only be allowed under sanction of some authority guided by a responsible person possessing a thorough knowledge of anatomy and physiology, 163, 252–4.

Adviser to the licensing officer.

Qualification and functions of adviser.

Considers it essential that the controlling authority should act under professional advice, which would command the respect of the profession, 164–6; and that he should have functions analogous to those of the Inspector of Anatomy, 167; and should have a knowledge of the character, position, and attainments of the individual, 255–6; and be himself a good anatomist and physiologist, and retain office *quam diu se bene gesserit*, 172.

Anatomy Act.

Instances the former scandalous manner in which the practice of anatomy was sometimes conducted, and the good effects of the Anatomy Act, and of the appointment of inspectors of anatomy, 167–71.

Is of opinion that to require permission to be obtained for every experiment on live animals would be an overgreat interference, but thinks a series of such experiments should be undertaken only under some controlling authority, 152.

Necessity of despatch in obtaining permission.

Persons skilled in anatomy and physiology should not be refused leave to conduct such experiments elsewhere, 176–7; cases might easily arise in which the surgeon or physician might wish to fortify his opinion as to the course to be adopted, by first trying it on an animal, in such instances the delay occasioned by having to get leave would be serious, 178–9.

Miscellaneous.—Considers the members of the medical profession as humane as other men; they would readily

Burrows, Sir George.

assent to any reasonable proposition for checking abuses, 146–8.

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ies on
ubject.

Believes that popular opinion has been excited partly by the discovery of certain cases of abuse, and partly by ignorance of the real nature of vivisection, 159; and a prevalent belief that it is a dissection of the living animal; it ought to be made known that very often the operations are of a most trifling character, such as in inoculation, and often there is no operation, but the administration of a drug only, 155. On the other hand there have been great abuses in operations on the living animal which should be put a stop to, 156–8.

tence of
es.

SIR JAMES PAGET, BART.

President of the Royal College of Surgeons and of the Medical and Chirurgical Society, 262, &c. &c.

ease of
riments
nimals.

Has given much attention to experiments on animals, 263; considers that physiological experiments have much increased in number since Sir Charles Bell's time, but thinks when he speaks of their being then monstrously prevalent, he refers to the French School and Majendie, whose experiments, although they have led to the saving of lives, were needlessly repeated to classes, 349.

Believes such experiments are not performed in England at all, except in some schools of medicine and by some physiologists connected with them, 307.

siolo-
l Hand-
k.

Considers Dr. Burdon-Sanderson's handbook clearly designed to aid advanced researches and not for ordinary students, 354–6.

Results obtained by Vivisection.—Such experiments are often necessary in novel cases of injury, 271–5. Gives instances of the advantages resulting from experiments on animals. The knowledge of the method of curing aneurism by tying the artery, and the means essential to recovery were learnt from the animal vivisections of Hunter and those who followed him, patients' lives were formerly lost from ignorance on those points, 294–5; admits that the method, however, might have been learnt from observations on men only in time, but points out that it was more quickly learnt and completed by experiments on animals, 294.

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son.

Further illustrates the necessity for experiments on animals by referring to the desirability of speedily finding an antidote for snake poison; such a remedy can probably be discovered only by trying various drugs, &c. on men and animals bitten; if only on those accidentally bitten the number of cases in which they can be tried must be very small, and the chance of discovery remote, but by trying experiments on animals inoculated with the poison the number of cases may be indefinitely increased and the discovery of an antidote probably proportionately accelerated; in the meanwhile 20,000 people a year, it is said, die from this cause in India alone, 305.

eatment
' angina
toris."

Dr. Brunton having observed that in *angina pectoris* extreme tension of the blood vessels accompanied the paroxysms, exposed the blood vessels of living animals, and after trying various substances found that nitrite of amyl possessed the power of relieving the tension of the vessels, and this substance is found to check the agony and probably avert the sometimes fatal result. Dr. Brunton's experiments might be performed nearly if not entirely under chloroform, and could not have been attempted on the human body, 308–15.

ansfusion
blood.

As another instance of the advantage of experiments on animals, refers to the discovery of the possibility of the transfusion of blood, of the causes of its frequent failure, and of the manner in which they may be averted. Had it not been possible to experiment on animals it would have been abandoned as impracticable before being brought to its present state of perfection, 380.

sk attend-
g the use
chloro-
m.

Also refers to the negative evidence obtained, that no certain means of averting the occasional fatal effects of chloroform exists; has himself seen four deaths therefrom, and has reverted to ether, in spite of its inconvenience, in consequence; by experiments on animals an anæsthetic both convenient and safe is being sought for, 380.

Anæsthetics.—A very large number of experiments can now be performed under chloroform, 278; Hunter's experiments would now be conducted under chloroform and the animal only subjected to the trivial pain of recovery, 296; some painful experiments, however, are necessary in the promotion of science, 293, but their number is very small, 297; Majendie's scientific researches might have been as successfully prosecuted with less animal suffering; he appears to have been quite indifferent to animal pain, 368–74.

ajendie's
xperi-
ents.

Paget, Sir James.

All such experiments should be performed so as to minimise the amount of pain, 298–301; death under anæsthetics is painless, 381.

Believes the lower animals have greatly diminished sensibility, experiments can generally be performed after the brain has been removed, as in frogs for instance, 357.

Sensibility of animals to pain.

Believes the pain caused in studying inflamed tissues in the frog to be small, 358–60; and not very great in bats, 361–2.

Study of Physiology.—Physiology is now a branch of study, and physiological laboratories are attached to hospitals, 316; the students are required to attend a course of practical physiology, but are not required to perform experiments on living animals, and, as far as witness is aware, they do not perform them, 285–9.

No special arrangements are made at St. Bartholomew's and the larger hospitals for living animals, the bulk of the work consists in the microscopic examination of tissues, &c., and the chemical testing of the animal fluids, &c.; the provision for live animals at Edinburgh is exceptional, 317–9; the elaborate machinery for the administration of anæsthetics tends to the reduction of the amount of pain.

Physiological experiments at hospitals.

Laboratory at Edinburgh.

Thinks the physiological school may be unduly developed.

Thinks that experiments for ascertaining the effects of poisons and drugs are increasing, but those for physiological and surgical purposes diminishing materially, most facts connected with the latter having been ascertained, 345–7; the effects of poisons and drugs and the progress of diseases cannot be studied under chloroform, 278.

Increase of experiments.

Limits of Experiments.—Considers experiments on animals should not be appealed to until all other methods of settling a moot point have been tried; is inclined to think it is now frequently resorted to prematurely, 302; experiments should be strictly limited to those from which there may be a reasonable expectation of discovering new facts, 363; the cases in which it is necessary to have an animal under experiment are very rare, 344.

Painful experiments should not be allowed in teaching, 282–4, 290; and even under anæsthetics should be limited to the illustration of facts which cannot readily be apprehended from description and diagrams only, 291; such as the action of the heart as seen in a decapitated turtle, or, in a warm blooded animal under anæsthetics and kept alive by artificial respiration, 292; this experiment can be shown in 20 minutes or half an hour, the animal remaining wholly under anæsthetic influence, 375–8; but speaking generally, classes can be as well taught without experiments as with them, 375.

Experiments for teaching purposes.

Demonstration of the action of the heart.

The live animals said to be used for purposes of demonstration at Guy's Hospital are probably insensible; believes live animals are never so used at St. Bartholomew's, 352–3. Dead animals are of very little use in the study of anatomy, 342–3; knows nothing of the experiments at Florence, Leipzig, Vienna, or Paris, 379; foreign methods are not common in English schools, 351.

Demonstrations at Guy's Hospital.

Regulation of Vivisection.—Approves of the resolutions passed at the meeting of the British Association in 1871, to the effect that the use of anæsthetics when possible should be compulsory; that no painful experiments should be made for teaching purposes; that no unskilled person should be permitted to perform such; and that they should not be practised in learning veterinary surgery, 267; believes that these resolutions are supported by all the leading scientific men of the day, 268–9, who would cordially support any reasonable law based on them, 270. The leading men in physiology and medicine are as desirous as other men for the promotion of humanity and abolition of abuses, 279–81. Medical men may feel somewhat doubtful as to whether they would not prefer that the control should be exercised by public opinion rather than by law, 281. Believes that public opinion would prevent the abuse of vivisection, 341; and be a sufficient restraint without legislation, 329–330; if a law is to be made, thinks it should provide for licensing teachers of physiology in the same manner that all teachers of anatomy are now licensed by the Secretary of State, 328–32; no private practitioner is allowed to, nor does, dissect, 333–5.

The British Association resolutions.

Public opinion on error.

The Home Secretary should be aided by advisers as to the competency of the person applying for a licence in the same manner as in the case of teachers of anatomy, in which the signature of two magistrates is necessary, 336. Is of opinion that unfit persons should be prevented from experimenting, and that only persons of

Licences.

Nature of experiment to be left to experimenter.

high scientific attainments should be allowed to perform them, and that the propriety of performing any experiment should be left to the humanity and judgment of the experimenter, 364–6.

Demonstrations.

At present the arrangements as to experiments are entirely in the hands of the lecturer on physiology, 320, 325; thinks this wrong; no experiments on living animals should be made except under sanction of a committee of the officers and lecturers or even governors of the hospital, 320–1, 326. When there is no medical school the performance of such experiments might safely be left to the discretion of a body of surgeons, 322–4.

Objections to legislative restriction.

Admits that it might, however, seem unreasonable to compel men thoroughly conversant with such experiments to apply for permission, 327; points out also that a private practitioner might sometimes find it highly desirable to try a special operation on an animal before performing it on a human being; a law of the nature suggested would make this illegal; thinks therefore a law would be inexpedient, 337–40.

Fear of fettering scientific research.

Considers that many things essential for the preservation of human life can only be ascertained by experiments on animals, and that medical men seeing much human suffering and death resulting from things they cannot yet cure, naturally attempt to discover remedies for them, even at the expense of animal suffering, and therefore is anxious lest in putting down abuses, undue restraint should be put on scientific research, 382–5.

WILLIAM SHARPEY, M.D., LL.D., F.R.S.

Professor of Physiology for more than 30 years at University College, and for nearly 20 Secretary to the Royal Society, 386–7.

For the increase of knowledge,

Necessity of Experiments on Live Animals.—Has performed experiments on living animals, and sometimes exhibited them, 389–91, and considers such experiments absolutely necessary for the progress of physiology, 393; and that scientific investigation by competent persons is important for the development of medical and surgical improvement, 398–401; reads extract from Haller in support of this view, 592–3. Is of opinion that the knowledge derived by really scientific observers from experiments on animals is most useful in interpreting the results of observations on the human subject in disease, 406–7; and that in order to deal rightly with diseased conditions of body, its condition in health, and the causes of its change of condition must at any rate be highly desirable.

and ascertaining the action of drugs.

Thinks, too, that students should see the effects of poisons, so that they may at once recognise them in man; points out that one animal would suffice for 300 students; and that although chloroform could not be administered death would be speedy, 507–11. Considers that the effects of new drugs should be first tried on animals, 504–6.

Stethoscope.

Results obtained by Experiment.—Sometimes there is a direct connection between increased knowledge and improved treatment, for instance, the use of the stethoscope became possible only when the connection between external sounds and internal conditions had been established; the discoveries of Harvey and Sir Charles Bell have been followed by important medical improvements which could not have been foreseen at the time, 395–7. The feasibility of tying large arteries, the best kinds of ligature, the influence of the periosteum in removing dead bone and reproducing bone have all been ascertained by experiment. Would, however, lay stress on the additional knowledge acquired rather than the improvement in the art of healing resulting from vivisection, 394.

Functions of the periosteum.

Galen's discoveries.

Instances, as illustrations of the advancement of science by means of vivisection, the discovery of Galen, that the arteries contained blood, and not vital air, as was formerly believed; admits that Harvey was led to reject the opinion in his day commonly received from the arrangement of the valves in the arteries, but the fact of the circulation of the blood and the true nature of the motions of the heart were only fully established by vivisection. The existence of the capillary vessels by which the blood passes from the arteries into the veins was established by the microscopic researches of Malpighi on the transparent parts of cold-blooded animals. The pressure of the blood was measured by Stephen Hales, Poiscuille, and Ludwig, and its rate of circulation ascertained by experiments on live animals, 394.

Functions the capillaries.

Sensory and motor nerves.

The sensory and motor functions belonging respectively to the posterior and anterior roots of the spinal nerves were discovered by the vivisections of Sir Charles Bell, and fully demonstrated by those of Majendie, 394. The functions of the spinal cord, medulla oblongata, and other nervous centres, and of particular portions of the brain, as far as they are known, have been ascertained by vivisection, 394. The nature of the reflex action of the nerves has been similarly arrived at, 394; as also the discovery of the functions of the facial nerves by Sir Charles Bell and Herbert Mayo, which at once changed the old treatment for tic doloreux, 394. The connection of the pneumo-gastric nerve and the medulla oblongata with the movements of respiration of the heart and vocal organs were discovered by vivisection, 394. The discovery of the vaso-motorial system of nerves, on its dependence on the medulla oblongata, and of the true explanation of the phenomena of blushing all resulted from vivisection, 394.

Nerve centres.

Reflex action.

Vaso-motorial system.

The immediate causes of the arrest of the functions of the brains and the motion of the heart in asphyxia and the correlative knowledge of the methods of resuscitation have been ascertained by vivisection, 394, also the conveyance of gases to the tissues by the red portion of the blood, and the effect of certain gases on the blood, 394. Vivisection brings increased knowledge, increased knowledge brings improved remedies and improved treatment, 394.

Red corpuscles of the blood.

Sensibility of Animals.—No animal, not even the lowest, has been proved to be free from pain under such experiments, 516; but it is difficult to say whether movements of animals experimented on indicate pain or not, 516.

Relative sensibility of different classes.

Can hardly say whether vertebrated suffer more than invertebrated animals; the writhing of a worm on a hook may be from muscular excitement and independent of pain, 512–4. A polyp may be cut into pieces and each grows into a new animal; thinks, therefore, the pain can hardly be severe, 513. Invertebrated animals, except insects, show small sensibility; in vertebrate animals it appears to be more acute, but it differs, or rather the manifestations of pain differ, 502, in individuals of the same species as pointed out by Dr. Waller and Vesalius, 496–7.

In different individuals.

Painful Experiments.—Does not approve of lingering and painful experiments generally; if it is not clear that the results will be useful and decisive they should not be performed, 446–7, 578–9; but even a lingering and painful experiment is justifiable if it be for a great scientific end, and if it will probably lead to a decisive result, 576, and is performed by a very competent person, 416–9, 425, 428. The number of experiments entailing severe pain is very small, 415. Pain should be minimised, 452; and if recovery after an experiment would be attended with pain, the animal should be destroyed, 451.

Pressure of the blood. Effects of starvation.

Thinks experiments as to the pressure of the blood must be very painful without anæsthetics. Starving animals to death would be another instance of protracted suffering, 470. Would not have undertaken such an experiment; it might be worth doing once but should not be repeated, 572–4. Refers to a long and tedious experiment for ascertaining the effect of mercury on the flow of bile which was made by Dr. Bennett at Edinburgh, 575.

Investigation as to the uses of the bile.

Reflex action.

The brain must remain in action in experiments with regard to the controlling centres of reflex action, 533; but the destruction of the upper part of the spinal cord destroys sensation in all parts below the medulla oblongata, while organic life may be maintained, in the case of cold blood animals for days even; this was Sir Benjamin Brodie's method, 490–5.

Inflammation of the cornea.

The experiment of causing inflammation in the cornea of a frog's eye may not be very painful, and it might be performed after the brain had been destroyed, 548–51.

Biliary fistula in cats.

Thinks it quite possible that Dr. Legg's experiments on 16 cats, some lasting three or four weeks, put to a lingering death, with a view of testing the functions of the liver, may not have been advisable, 577.

Schwann's experiments.

Some severe experiments on the gall bladder were performed by Schwann of Louvain, 575. He connected the gall bladder with an opening in the side of the animal, so as to divert the bile from the intestines, 420–1; with the object of discovering the use of the bile, 426–7; the operation itself would be performed under chloroform, 423; but the experiment would last some weeks, 420–1.

Artificial respiration.

Ludwig has a gas engine for keeping up artificial respiration in the animals; which is also used for driving lathes, &c.; there is a similar one at University College, 552–5.

Experiments on Human Beings.—Has himself made experiments with various medicinal agents on men to see how soon they would appear in the urine; instances also Dr. Stak's experiments on himself, and Dr. Parker's with alcohol, Sir James Simpson's with chloroform, and Spallanzani's on digestion on themselves, 594–6.

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All novel operations are in reality experiments on the human subject; as for instance, Hunter's ligaturing an artery for aneurism, 596. Experiments on men with a view of ascertaining whether curari has anæsthetic properties, are not necessary as safe anæsthetics already exist, 568–70.

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ection.

Human vivisection is said to have been performed by Herophilus of Alexandria, who was consequently denounced by Tertullian; in modern times inoculation was first tried on criminals, but cannot think the possibility of subjecting criminals to experiments can ever become a practical question, 558–67.

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Ancient Vivisection.—Galen describes his tables and appliances, and the manner of vivisecting in the second century; gives details of his experiments on the spinal cord, 499–500.

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Vesalius gives a chapter on vivisection in his great work on anatomy in 1543, gives particulars, 497. He appears to have been in the habit of exhibiting his experiments to a class, 501.

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Modern Vivisection.—Vivisection is far more frequent abroad than in England; in France, Claude Bernard is deservedly eminent, and has been most active and successful in his researches, 517–8.

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Majendie's experiments threw light on some matters, but some of them were perfectly unjustifiable, such as cutting out a dog's stomach and illustrating the mechanical action of the contraction of the diaphragm and walls of the abdomen, by inserting a pig's bladder in lieu, 474–5. Attended the first of a series of lectures by Majendie; the experiments were painful (there were no anæsthetics then) and without sufficient object, and the animals were finally killed in a very painful way; witness was so repelled by what he saw that he never went again, 444. Does not think experiments now made in France are so purposeless and cruel as some of Majendie's, but is less conversant than formerly with such matters, 480–1.

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Flourens removed portions of the brains of animals and kept them still alive, and made some discoveries which could not easily have been otherwise made, 476–8; these severe experiments are seldom performed in England, 479.

any.

In Germany scientific men are numerous and zealous and their universities afford scope for all kinds of experiments, 517; of which the advancement of science is the primary object, but they are also employed for the verification of facts; one or two experiments by a single individual can hardly be relied on to establish a new fact, until it has been tested by other observers, 519–22.

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Limits of Experimentation.—Experiments without a definite object are mere purposeless inflictions of suffering and are of no use, 402–3; although conceivably a discovery might result from one, 523. Would not recommend therefore the practice of experiments by uninstructed persons on the chance of something turning up, 524.

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When facts are once thoroughly established by painful experiment before skilled persons, such experiments should not be repeated, 404–5. In exceptional cases veterinary surgeons might be competent to conduct such experiments, but not ordinarily, 429–30.

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Experiments for teaching should be limited to those which can be performed without pain, 466; such as the action of the heart, which should be shown to students in living animals, 467. Illustrations involving any severe pain should be abstained from in teaching, 539–40; except those showing the action of poisons, 300, 541–2.

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The repetition of the painful experiment, described by Dr. Foster, for illustrating the functions of the anterior and posterior roots of the nerves of the vertebræ, is certainly not necessary for exhibition, and the fact is well established, 536–6.

Thinks that some of Sir Charles Bell's experiments, such as that with regard to the spinal cord, should not be repeated on the ground of their painfulness; and others because the facts are clear and admitted, 464–5.

Anæsthetics.—Anæsthetics have made a great difference in experiments on animals, 534. Most experiments can now be rendered painless, 392, either in whole or in part, 412–3; and should be performed under anæsthetics, 448–51, 590. In tying an artery the operation may take place under anæsthetics, and the pain of the recovery is small, 414. Sir Charles Bell's experiments

on the facial nerve were all but painless, and would have been interfered with by chloroform, 408–9.

Effect of chloroform on frogs.

Thinks it possible that chloroform would destroy life in the frog, 532. Regards the administration of curari to frogs before the experiments described by Dr. Foster as unnecessary, 531. In most cases the brain can be first destroyed, 535. Alcohol acts as a very strong narcotic on frogs, 532. Chloroform, and chloral even more so, interferes with the action of the heart and cannot be used in many experiments. Opium is available however, 556.

Of alcohol.

Chloroform, chloral, opium.

Curari.—Curari is not, like chloroform, ether, &c., proved to be an anæsthetic, it is really given to keep the animals still, 468–73; although by some it is considered to be of the nature of an anæsthetic, 462–3; thinks that it should not be represented to the public as being an anæsthetic, 441–3; Claude Bernard's opinion that sensation remains is of much weight, 440. Considers himself that the curari poison does not destroy consciousness, it cuts off the power of the nerves over the muscles and may destroy sensation, but the last is a moot point and requires further investigation, 435–9; curari has been suggested as a remedy for tetanus, 571.

Extent of the Practice of Vivisection.—Thinks the popular mind is excited by what it hears of vivisection on the continent, 455; has himself heard no evidence as to the existence of any large amount of vivisection, and does not believe that it exists, 456; and has heard no evidence of any excesses in England, 454; on the contrary believes that there are very few painful experiments performed in this country, 584.

Doubts existence abuses in England.

Thinks it probable that young physiologists who have studied abroad, particularly at Leipsic, would be disposed to introduce here the methods in vogue there, but does not believe that experiments are carried on by persons who are not either teachers or investigators, 538.

English students at Leipsig.

Does not think that there are more than a dozen experimenters in this country who practise vivisection, 453; believes it is not resorted to for individual satisfaction, as the results are generally announced in scientific publications, 458–60. As a rule such experiments in England are carried out in the physiological laboratories attached to schools and established under the directions of the College of Surgeons and other examining bodies, 482–4; gives names and particulars, 457; and refers to the judicious experiments made by Professor Syme, 410–1.

Few vivisections in England.

Mostly in physiological laboratories.

Study of Physiology.—Students are required to have a knowledge of practical physiology, but not to perform experiments, 485; and as a rule they do not practise vivisection, 453; recollects, however, an exceptional case, that of Mr. James Blake, who made several valuable additions to scientific knowledge, 488–9. A great portion of practical physiology has nothing to do with vivisection, a great part of the student's work consists in the microscopic examination of the tissues, 486–7. Experiments are sometimes exhibited in the theatre, but more often to a few students only in the laboratory, and are generally performed under the influence of anæsthetics at University College, and probably at other schools, 461.

Agrees with Professor Foster that the methods of experiment are becoming year by year more physical in character, and that observations having the widest bearings on physiology may be conducted for the most part on frogs, 525–8.

Experiments are of a more physical nature than formerly.

Admits that the advance of science opens up new questions as it brings us into wider contact with the unknown, and necessitates new experiments, but thinks the fear that they will increase in "geometrical progression" chimerical, and points out that the advance of science has led to the discovery and use of anæsthetics, and apprehends that all such experiments should be made under the direction of some skilled person, 543–7; does not think that witnessing these experiments tends to blunt humane feelings, 585–6; gives instance to the contrary, 503.

Increase of experiments.

Should be under control.

Regulation of Vivisection.—Admits that the zeal of young scientific men may carry them too far, and that if he had been asked beforehand as to some experiments he should have recommended their not being done, 580; does not, however, believe that an indiscriminate practice of vivisection prevails in this country; thinks that such experiments are very rare except those performed by competent persons, and that therefore no control is needed, 431–4.

The more eminent men in this country reprobate the needless infliction of pain, and have vivisected with the greatest reserve, and have avoided severe experiments

Sharpey, William, M.D., &c.

except when absolutely necessary, and have in fact discouraged vivisection as far as possible, 445.

Deprecates legislation.

Doubts whether abuses requiring legislation exist, believes public opinion and the example of leading physiologists will prevent excesses, 453, 591; if there is legislation it should provide for the licensing of competent persons only, for a record of experiments being kept, and for withdrawal of the licence in cases of abuses, 583, 591; but is of opinion that no restriction should be imposed with regard to animals rendered insensible either by anæsthetics or by destruction of the brain, 451, 453. Points out objections to certain phrases in the draft of a bill on this subject laid before Parliament, 453.

PROFESSOR GEORGE M. HUMPHRY.

Professor of Anatomy at the University of Cambridge, and surgeon to Addenbrooke's Hospital, 596–7.

In medical schools.

Among private investigators.

Extent of Practice of Vivisection.—Does not believe vivisection is carried on to any extent in England, except in the schools of physiology, 638–40, and does not think the number of these reaches a dozen, 763–5; private investigators are very few in number; very little physiological work is done in England, 766–8; thinks that any important discoveries would naturally be speedily announced by those who make them, 774–6.

Increase in number of experiments.

The number of experiments, however, has increased with the number of investigators and the use of anæsthetics, 675, under which such a large number of experiments, especially physiological ones, can now be performed without giving pain, 679, 725–6.

Is not improperly conducted in England.

Experiments by students.

Is unaware of any abuse of such experiments on live animals, believes public statements with regard to them are greatly exaggerated, 619; thinks they are not improperly conducted in England, believes they are in the hands of competent persons, 620; thinks, however, that although a teacher would naturally associate his best students with him in conducting investigations, experiments on live animals should be only under a responsible professor, 627–9, and that it can rarely be necessary for students to practise vivisection, and that when necessary it might almost always be done under anæsthetics, 623–6.

Cambridge laboratory.

At the physiological laboratory at Cambridge the senior students are deputed by the lecturer on physiology to perform experiments on live animals, but only under anæsthesia, 689–4; but many of the students are men advanced in years and hardly *in statu pupillari*, 695–6; the experiments at Cambridge are not made under the control of the authorities, but are performed under the control and direction of one of the professors, 757–61.

The Norwich experiments.

Was in the room when the Norwich experiment of injecting absinthe into the veins of a dog was being tried, but did not learn the nature or object of the experiment until afterwards, 716–7; could not tell whether the animal was suffering or not, it did not appear to suffer much, 707–8; considers that the knowledge that absinthe injected into the veins produces epilepsy, has some bearing on the question of its action when taken into the stomach and is so far valuable, 717–23.

Physiological Handbook.

Does not think the publication of a handbook for the physiological laboratory leads to students trying experiments themselves, 767.

Experiment as to recurrent sensibility.

The sawing through the arches of the vertebræ in demonstrating the existence of recurrent sensibility in the cat or dog referred to therein would be performed under chloroform, 746; the subsequent excitation of the nerve roots would, however, necessarily be performed while the animal was conscious, 747–8; does not think such experiments are described for the purpose of being repeated as demonstrative experiments as it cannot be often necessary to perform them, 747–8.

Experiment described in "The Doctor."

Doubts the veracity of the description in "The Doctor" of an experiment stated to be for the purpose of "demonstrating the structure of the cornea of "a frog by staining the intercellular substance" with lunar caustic, considers the account sensational and the experiment meaningless, 656–7, 662–5. Points out that although pain in a frog is not to be measured by pain in the human subject, 666; this experiment was not tried under anæsthetics, although it apparently might have been, 658–61.

Necessity for Experiment.—Considers experiments on live animals are necessary for the advancement of science, 630; that the advancement of science tends to the relief of human suffering, and that therefore experiments which advance science are justifiable, 778–9.

Development of diseases by civilization.

As civilization which causes diseases increases, the number of experiments necessary will increase rapidly,

Humphry, Professor George M.

especially in pathological subjects, 724; believes that accurate knowledge of the functions and relations of the several parts of the body can only be gained by examining them during life, 602; and that without vivisection our ignorance of the vital processes would be barbarous, 685–8.

Results obtained by Experiments.—Points out that great results are being, and have been attained from experiments on animals, 677; and that animals as well as men are benefited thereby, 683–4.

Nerves of the heart.

Inflammatory processes.

The action of the nervous system on the heart and on the blood vessels, the latter the basis of our knowledge of the processes of inflammation, would not have been dreamed of without vivisection, 603.

The discovery of the passage of the blood corpuscles through the walls of the vessels, could only have been discovered by seeing them so pass and repass in a living animal, 615, 743–5.

Pathological Experiments.—Pathological experiments on animals should to a great extent form the basis of the treatment of scientific knowledge of medicine, 611; admits that many pathological experiments involve pain or distress to the animal, 740–2; which cannot be mitigated to the same extent as in physiological experiments on account of the time, sometimes weeks, which they take, 667–9; many, too, cannot be made under anæsthesia, 615.

Necessity for increase of experiments.

Civilization is the great engenderer of disease and will tend to physical and moral degeneracy unless medical art advances likewise, and therefore pathological experiments become more and more necessary, 635; the number of these investigations is very small, knows only of one or two persons engaged in them, 771; the necessity of a competent operator is even greater than in physiological experiments, 614.

Induction of disease.

The induction of disease in animals is necessary to learn its nature and remedies, 615, with the object of preventing human suffering, it should only be done by competent persons, 634; thinks, however, that disease in animals is not so painful as in man, 616–8.

Inflammatory action.

Action of mercury.

Information as to the causes and treatment of inflammation can only be thoroughly learned by observations more or less painful on the living animal. Believes that by experiments on animals the nature of tubercle is beginning to be understood, even cancer may possibly be one day understood, and be found to be preventable, 615.

Considers observations on the action of drugs and poisons to be a branch of pathology, 698–9; many of the experiments as to the action of drugs on animals are not painful, but anæsthetics can rarely be used, 700–1. Observes that the old notion as to the action of mercury on the liver has been disproved by the experiments on living animals at Edinburgh, 699.

Reasons justifying vivisection.

Limits of Experiment.—Considers that as a large number of animals live by killing other animals it is justifiable for man to inflict death or pain on other animals if there is a reasonable prospect of his being benefited thereby, 599–601; vivisection is often less painful than the ordinary methods of killing, 604.

Wishes what does take place in physiological laboratories were more generally known, there would not then be so much exaggeration and mis-statement, 762.

Difficulty of establishing discoveries.

With regard to the repetition of experiments, points out that the facts ascertained are often of a very complicated nature, and many observations are required to fully confirm them, 635, 680–2; thinks even Sir Charles Bell's experiments on the nerves could hardly render further experiments unnecessary. The results required observation by more than two persons, 636.

Assuming a fact to be established, the experiments demonstrating it should not be repeated except under anæsthetics, 772–3; thinks, however, that there is no definite line between experiments for demonstration and those for investigation, 735, 739.

Physiological education.

Considers that medical students should pass through a physiological laboratory and see some painful experiments; very few, however, would suffice, except in pathology, 736–8.

Vivisection, especially of conscious animals, which is much more difficult, is valueless unless performed and observed by competent persons, 607; when performed by incompetent persons it produces useless suffering, 608, 629, 634; the experiments in which consciousness is necessary are however very few in number, and very seldom necessary, 604–6.

Sensibility of Animals.—Thinks animals do not feel as acutely as human beings, as their nervous systems are relatively much smaller, 641–8; believes that warm blooded animals are more sensitive than cold blooded ones, and man is most so of all creatures, 702–4; points out that a great amount of human pain is in the anticipation and retrospect which is absent in animals, 655; for a similar reason chloroform is more needful in the case of adults than for infants, as is seen, for instance, in the operation for harelip, 704–6.

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Admits that contortion usually implies suffering, but thinks not necessarily so, as the sensory nerve may be destroyed while the motor nerve remains in full action; points out that in epilepsy there is no pain, although there is biting of the tongue and laceration of the muscles, and that there are sometimes violent contortions under chloroform; thinks the contortions of a worm on a hook do not necessarily imply pain, 649–655; in vivisection, especially before chloroform was discovered, has often been surprised by the stillness of the animals under the knife, 707, 710; but admits that absence of pain can hardly be inferred because animals when tied down often do not move under the knife, 711.

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viduals.

Questions whether after the skin has been divided the subsequent pain may be less in animals, but doubts whether the skin in the human subject is the great seat of sensibility, 711–5; believes that sensibility among men differs greatly in individuals, but thinks it more doubtful whether similar differences exist among animals of same species, although a blood horse probably feels more keenly than a cart horse, no one however can be sure, 751–2.

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Anæsthetics.—Anæsthetics have been used from the date of their discovery, about 28 years ago, 678; and have facilitated and increased the practice of vivisection, 604; before their discovery experiments were fewer from the reluctance of medical men to inflict pain, 676.

Vivisection both for physiological and pathological experiments can nearly always be performed under anæsthetics, 631–2; has seen many experiments on arterial pressure, but always carried on under anæsthesia, and believes they should always be so performed, 749–50; thinks there is no difficulty in administering them to frogs, 727, although they and the lower animals generally are easily killed by anæsthetics, 731–2; has not, however, had much experience in this direction, 733–4.

Curari.—Is not well acquainted with the effects of curari, 777; and has never experimented with it, but believes it quite possible that the motor action only may be suspended under its influence, and if so it follows that it is not an anæsthetic and frogs under it would suffer, 729.

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Regulation of Vivisection.—Is of opinion that no experiments on live animals should be allowed to be performed by students except under responsible control and direction, and thinks that a law enforcing this would have the assent of the profession beforehand, 753–6. Has himself, however, more confidence in leaving the limit to which vivisection should be carried to the right feeling of competent men than in legislation, 672.

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Thinks it is inconceivable that anybody should enter upon such experiments without much forethought and definite ideas as to the points to be elucidated, 618; does not believe they deaden humane feelings, gives instance, 622; and thinks that men of education do not voluntarily inflict pain on animals without good reason, 676; and that the more eminent scientific men would support any reasonable measure for preventing abuses, 609–10; considers that such an experiment as that described in "The Doctor," 656–66, would never have taken place if the matter had been under the control of any eminent man, 673; in any legislation enforcing the use of anæsthetics, pathological experiments must of necessity be excepted, 670–1.

W. PRITCHARD, M.R.C.V.S., F.C.S.

Professor of Anatomy at the Royal Veterinary College.

Has care of the infirmary and performs most of the operations, 780–2; which are entirely for curative purposes, 785. Would not perform an operation unless there were a reasonable hope of cure, 837–8.

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Veterinary Operations.—No one at all, except the principal, the professor, and the assistant professor, is allowed to operate, 807–11. Students are not allowed to perform even the minor operations with the knife, 839–40; no operations for the sake of experiments are performed, 786; would consider it very improper if there were, 786–7; but admits that if the knowledge derived from an experiment would probably benefit a large number of animals it should be performed, 823.

Navicular disease.

The operation in cases of ulceration of the navicular bone, 881, or navicular disease, consists simply in the division of a nerve, is performed under anæsthesia, gives no subsequent pain and does not interfere with the nutrition of the bone, 873–8. Experiments on healthy horses are not necessary to learn the proper treatment of the disease as it is so frequent; believes half the horses in London suffer from it, 379–80.

Is unaware of any experiments on healthy live animals, but instances novel attempts to cure diseased animals, such as the division of the tendons in the case of a colt with short leg, which was successfully performed, 836–7, under the local influence of ether, 844–5.

Is opposed to physiological laboratories.

Does not say that physiological experiments have no beneficial results; but that the results are not sufficient to trust him to advocate them, especially as direct evidence as to the right remedies is obtainable from treating diseased animals, 882–4. Is therefore adverse to the setting up a physiological laboratory in connection with the Veterinary Institution, believes it would be difficult of control; that needless experiments would be performed, and that the results would not be sufficiently valuable to render it justifiable, 869–71.

Has not seen any physiological researches conducted in any veterinary college; believes abroad a courteous concealment was practised with regard to such experiments, 872. Does not believe that such experiments are performed by veterinary surgeons throughout the country; and believes that if there are, any reasonable law against it would be supported by the authority of the college, 788–90.

Gives details with regard to the working of the College, 783–4, 806–7; knows nothing of the Brown Institution, 834–5.

Sensibility of Animals.—Has performed thousands of operations, and thinks the sensibility in the human and other animals, including frogs, is much the same in all, 846–9. Points out that the irritation produced by a parasite is as great in a small as in a large animal, 850.

Anæsthetics.—Sometimes chloroform, sometimes local anæsthetics, such as ether spray and iced water, are used in operations on animals, 791–3, 889.

Effect of chloroform on horses.

It is almost impossible to destroy horses by chloroform, has had them for hours under its influence, 795; they are however injured by the quantity necessary to produce insensibillty, 804–5; and do not recover so well when it is used, it poisons their blood, 794.

On oxen and sheep.

On dogs.

Oxen and sheep, especially the latter, do not require as much chloroform as horses, 812–4. It is not however safe to apply chloroform to dogs, as the action of the heart is with them intermittent, and they often die, 797–9, 803, 820–2. Would use it on cats, but has had no experience, 800–1.

Curari.—Curari is not now used; believes it was found to produce violent muscular spasmodic contraction, but has no personal knowledge, 816–9. Would not use it himself as there is a doubt as to its being an anæsthetic, 885–7.

Pathological Experiments.—All experiments have been with regard to diseases of animals only, 841.

Inoculation pleuro-pneumonia.

Inoculation has been tried for pleuro-pneumonia, 896; but has not as yet led to a satisfactory result, 906; for small-pox in sheep it is found successful, 904–5.

Foot and mouth disease.

Experiments on healthy animals have been tried with regard to the foot and mouth disease; cannot state the results, but has no hope but they will tend to its cure, 897–900. Was engaged with Professor Varnell in inquiries on behalf of the Government into the cattle plague, and tried many internal and external remedies; such as the injection of opium into the veins, inoculation, &c., but without success, 888, 890–893; the experiments involved little or no extra pain, 894–5. Is not aware of any experiments made with the object of tracing the cause of the disease, 832, although that has been attempted with regard to glanders, 833.

Fluke disease.

Typhoid fever is being induced in pigs to determine a point not ascertainable by other means, 842–3. Experiments with regard to parasites were made by Dr. Cobbold, 829–31. The development of the fluke disease has been watched by causing sheep to take the pupa and destroying them when the disease had advanced, 853–60. Superintended experiments with a view of finding in what animals trichiniæ would live, and how they could be conveyed to the human being, and how this could be prevented. No cure for the disease was found, but much beneficial knowledge as to its prevention was gained, 861–8.

Effects of drugs.

Does not conduct any experiments as to effects of poisons, 851. Believes that something may be learned from the action of drugs on animals as to their probable action on the human frame, but thinks that the results cannot be depended on, 907–10. As the results of experiments on live animals are unfavourable, except as regards determining whether a disease is infectious or contagious, is not in favour of such experiments, with that exception, 901–3.

H. W. ACLAND, M.D., F.R.S.

Regius Professor of Medicine at the University of Oxford, President of the Medical Council, and Fellow of the Royal Society, 911–3.

Is justifiable for the advance of knowledge.

Necessity of Experiment.—Has given attention to this subject from the time of being a student 33 or 34 years ago, and considers that if it is held to be desirable to extend the science of physiology, experiments on live animals are necessary, 915–20. Some of the painful experiments have elucidated points that could be made out in no other way, 922. Is of opinion that putting animals to pain or death is clearly justifiable if benefit to man will result from it, or even if the object is simply to add to the stock of human knowledge, although the question assumes another aspect, 992. Is of opinion that it is impossible to draw a line between knowledge which is, and which is not, likely to be practically beneficial, 993; and that, as in the pursuit of physiological knowledge, experiments must be made, 923, and as medical science is advanced by such experiments, considers they are justifiable, although it is erroneous to say that the progress of science mainly depends upon them, 956–9. Considers that seeing the actual working of the structures of living creatures gives quite another conception of them from studying them in the dead subject; instances the palpitation of the heart of the trout, witnessing which probably led to his own choice of his path in life, 990; and points out that the practical observation of the living structure has revolutionized the old theories of animal economy, 922.

Limits of Experiment.—Approves of the British Association plan of making grants for definite purposes in their experiments, and thinks public grants might be treated analogously, 986, 1003–4; but is of opinion that experiments not calculated to elicit new truths, and not necessary for the education of competent medical men, ought not to be performed, 975; whether such experiments should be forbidden by law is another thing, 975.

Students should be forbidden.

No one but experts should be allowed to attempt or observe such experiments, 927, 947; would not perform them himself, 953–4; and thinks that as students generally will never become experts, they should not perform them, 968. At Oxford students were not, and he believes are not, allowed to perform such experiments, although no particular persons are authorised to perform them, 960–2. Thinks that when the first burst of this knowledge came, 25 or 30 years ago, students were not wrong in verifying observations by actual experiment; joined in many for this purpose himself, 927.

Except under great necessity, would not perform a painful experiment except under anæsthesia, 966; and has a horror of dissection, although he has done a great deal, 989.

Painful but necessary experiments.

The larger number of experiments which have added to our knowledge were performed without anæsthetics, many indeed took place before their discovery, 921. Many of the experiments relating to the nervous system, circulation of the blood and structure, were very severe and painful; nevertheless, men of tender and gentle natures, such as Sir B. Brodie, Dr. Hope, and others, performed them, because no other means of solving the question at issue was available, 921, 926. Men like Hunter, Brodie, and Davey seem to have an instinct which restrained them from unsuccessful experiments; inferior minds do not seize the precise points where further development is possible, and hence much useless suffering is thereby caused, 1005–7. Some of the experiments of Sir B. Brodie, &c., were on animals just killed, rather than on live animals, 921. It is not certain whether observations on live or dead subjects led Harvey to his great discovery, 991. Thinks that the experiments of able men, like Dr. Klein, although fruitless as regards mitigating human pain, may advance knowledge, 1008. Cannot give an opinion as to Brodie's experiments with regard to the nerves influencing digestion; but points out that deciding that no further advance is possible is itself a gain, 1005. Painful experiments which have established a fact should not be repeated, unless it should subsequently appear that the conditions under which they were performed were of a nature to modify the result, 924–5, 987.

No experiments on live animals should be allowed for demonstration.

Dragging out the mesentery of a frog, although very painful, may not have been cruel in the first person who did it; but repeating the experiment, when the result is known beforehand, is both cruel and useless, 922–3. For the purpose of demonstration witness performed a series of experiments to show the development of the mammiferous ovum, involving the deaths of animals, but not their vivisection, 988–9; but thinks there is much less need than formerly for experiments by way of demonstration, 988. Goes further than the resolution of the British Association, saying that "painful experiments" should not be used for demonstration; thinks no experiments on live animals should be performed for this purpose, 947; thinks it wrong to make a common practice of sacrificing the life of a rabbit, even painlessly, for purposes of demonstrating things which could be learned from diagrams or dissections, 933, 989; has himself furnished hundreds to prevent the necessity of repetition, 947; these dissections were prepared from dead animals, sometimes killed for the purpose, 964–5.

On the other hand many experiments are painless, as for instance the observation of the circulation of the blood in the transparent texture of a frog's foot, 922.

Change in the motive for investigation.

Extent of the Practice of Vivisection.—The Royal Society's Transactions and other scientific publications are full of accounts of such experiments from the time of Harvey downwards, 986; and the Royal Society awarded the Copley medal to Sir Benjamin Brodie for his experiments on live animals, 985. The improvement of the microscope about 30 years ago gave a great impulse to experiments, 922. Points out that formerly scientific research was in the hands of medical practitioners only, but that now the extent of knowledge has been so vast that the field has become subdivided, and professed physiologists have arisen whose sole object it is to advance knowledge, 981–2; formerly the first object sought was new methods of alleviating human suffering, 983–4.

Physiological laboratories were the outcome of the zeal of the physiological teachers, and of the general desire for increased and more perfect appliances in all branches of the sciences, 978, 980.

Thinks there is an increasing number of biological students who study anatomy and physiology in the pursuit of abstract knowledge, rather than as medical men do, with object of discovering remedies and mitigating human suffering, 944.

A desire for pure research, irrespective of ulterior benefits received therefrom, is now spreading over Europe, and it may be feared that a practice has arisen with it of dealing with animal organisms much in the same way as if they were unconscious inorganic substances, 944–5.

Disregard of suffering.

Has not recently been much in foreign laboratories, but believes the use of chloroform is habitually neglected, 941; and that a great deal is being done on the continent which would shock the feelings of this country and not be tolerated here, 946; the extent to which these experiments are carried abroad is much greater than here. Considers the tendency of one phrase of modern thought is to treat living organisms simply as forms of matter which it is desirable to investigate, 999; and the consequent disregard of suffering in these experiments touches on very serious ground, 999.

Dr. Sanderson's Handbook.

Dr. Sanderson's Handbook for the Physiological Laboratory appears designed to explain to learners how to pursue these experiments themselves, 970–1, 999.

Physiological education.

The General Medical Council has the supervision of medical education, 976–7; and the question of requiring practical experimentation from students will doubtless come before it when the whole plan of medical examination and education is considered, 967, 979; this would have been done during the present year had not the Commission been appointed, 951.

Brown-Séquard.

Pathological Experiments.—Thinks that although Brown-Séquard had 584 guinea pigs under experiment at the same time and had had in the course of 30 years very many thousands in his laboratory, that public opinion might deal with the much larger amount of needless suffering caused by field sports and what are called innocent amusements, 944–5; experiments on the scale of Brown-Séquard's would be liable to abuse in the hands of inferior men and would be useless, 997. Refers to the researches made by artificially producing tubercle in animals, and the remarkable discovery of

Brown-Séquard's as to the possibility of producing epilepsy in pigs and of rendering it hereditary. Adverts to the possible beneficial results of such experiments, and deprecates the censure of any but those specially acquainted with the facts, 952.

Anæsthetics should be used whenever possible, 938–9; nearly all necessary experiments can now be performed under their influence, 932–7; agrees on the whole with resolutions of the Committee of the British Association, but is almost ashamed to have to formally assent to such an obvious proposition as that all experiments that can be performed under anæsthetics should be, 947.

Cannot say whether curari destroys sensibility, 973; there are some niceties about its use, its effect is to destroy voluntary motion, 972; and for this purpose it is used in the experiment in which the mesentery of a frog is laid bare for observing the circulation of the blood, 972–4.

Regulation of Vivisection.—Whether control should be exercised by law or by the Royal Society or some scientific body is a question which witness feels it his duty to leave to the Commission, 1002–3; thinks that legislation might restrain incompetent persons from performing such experiments, 930; but is not prepared to say that observations for scientific purposes should be under ordinary legislation, 928; and is unwilling to think that such a course is necessary, 929.

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t person.

Does not consider the control exercised under the Anatomy Acts analogous, 1000–1.

Is of opinion that the competent men in England are to be trusted, 969; that the general tone of scientific feeling is good with regard to these experiments, and that abuses must be very rare, 998; believes that in the great schools the control and discretion of the professors is sufficient safeguard against abuses, 930–1.

Thinks that the habitual performance of experiments without chloroform would not be possible in any English operating theatre, the good feeling of the profession and of the public would make it impossible, 940–4; if wanton cruelty is proved against teachers it should be put down, 1002; supposing that it exists among students it could be punished under the Cruelty to Animals Act, and thinks that if it occurred in schools the schools should be suppressed, 949–50.

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gland.

Does not believe the scientific men of England are regardless of suffering; if it is proved that they are, they deserve the humiliation of having a legal enactment levelled against them, 948.

SIR WM. FERGUSSON, BART., F.R.S.

Sergeant Surgeon to the Queen, Surgeon of King's College Hospital, and Fellow of the Royal Society 1009–11.

In early life had a great deal of experience in experiments on live animals, but not much latterly, 1012–4; witness' opinion of these experiments is much less favourable now than formerly; would not now perform many of the experiments he did then, 1031.

Results obtained by Experiment.—Considers that although these experiments have been of great service, their value has been altogether exaggerated, 1015, 1032, 1117; and that they often give fallacious results from the introduction of abnormal conditions both by the use of the anæsthetic and by the disturbance caused by the operation, 1085.

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gerated.

Admits that the knowledge of the circulation of the blood and of the nature of the actions of the heart, liver, kidneys, and nerves was ascertained or increased by these experiments, but doubts the value of knowledge thus obtained, seeing the observation could not be made on these organs in their normal condition, 1051–3; but thinks many of the most striking experiments are performed on the lower animals *after* they have been tried successfully on the human subject. Hunter's discovery of the practicability of tying the arteries, 1024–5, some of Sir Astley Cooper's operations and Sir James Simpson's experiments with chloroform, 1033–4, were performed first on the human subject, 1026; believes that all surgical operations without exception were first tried on human beings, 1092–3; is not aware of any experiment which has led to the mitigation of pain or improvement of method in surgical cases, 1032, 1049; indeed more experiments are performed by those who are not practical surgeons than by those who are, 1033.

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Thinks the experiments referred to by Sir J. Paget are not of the smallest practical value with regard to the transfusion of blood in human beings, 1104–5; doubts whether Mr. Syme's operation as to the life and death of bone had much beneficial influence, 1027; their object, however, was physiological rather than surgical, 1030; subsequently Mr. Syme was strongly opposed to operations on animals, "unless for the purpose of causation"; reads extract from the "Veterinarian" to that effect, 1027–30; considers this, the mature opinion of such a man, of very great weight, 1039; admits that Mr. Jones' experiments with regard to the best sort of ligature were valuable, but there remains as much doubt now as then in spite of experience and Mr. Lister's experiments at Edinburgh, 1094–9, 1108–9.

Mr. Syme's experiments, and subsequent disapproval of them.

Ligatures.

Considers that the practice of medicine is based upon clinical and pathological rather than physiological observations, 1102–3; is himself guided by the result of his observations on the human subject, and does not think he has gained any increase of knowledge from these experiments, 1100–1.

Extent of Practice of Vivisection.—Is impressed by the fact that the taste for this sort of investigation is on the increase, 1016; as is shown by the provisions made for it in the new University Buildings in Edinburgh, 1115; doubts, however, whether the gain to knowledge has been commensurate, 1017; believes that painful experiments are more numerous than necessity warrants, 1018; and that there is much useless repetition; when a fact has been established, the experiments demonstrating it should not be repeated, 1019; unless some new element comes under view, 1020–1; and even then great judgment is necessary, and the number of persons fit to perform such experiments is limited, 1022–3.

Experiments are over numerous;

Believes many experiments are performed in a most reckless manner, 1035; even in the London laboratories, judging from what he is told by students; thinks inquiries should be made with regard to them, 1112–6, of the younger members of the profession or senior students, 1125.

and recklessly performed;

Fears that experiments even in England are protracted in a very shocking manner; instances the case of an animal kept crucified for many days together and brought many times into a class room, 1037–9.

and often needlessly protracted.

Believes that if the manner of conducting these experiments were fully known, the reputation of certain eminent scientific men would be greatly lessened, 1036; and as much public indignation aroused as there was formerly with regard to body snatching, 1040, 1048.

If details were known great indignation would be aroused.

Although many eminent English surgeons have very legitimately performed various experiments on animals for the elucidation of definite points, 1091, they have not been extensive experimenters in this way, 1026.

Legitimate experiments.

Operations on animals are not practised in England to acquire dexterity; thinks such a course would be useless, the circumstances are so different, 1087–90; is not aware of anyone who has become an expert by this means, 1026.

Surgical dexterity not acquireable thereby

Believes there are very few amateur physiologists in England, 1139; and that very little physiological work is done outside the great schools of physiology, 1139; if unnecessary cruelty exists in them a remedy is necessary, 1140.

Private experiments.

In the Norwich experiments the dogs, witness is informed, were kept strapped down to boards; the straps, however, were cut by a surgeon to give them some kind of relief, 1086; is happy to say he did not witness them, and thinks they were founded on altogether incorrect views, 1106–7.

The Norwich experiments based on incorrect views.

Has purposely avoided Dr. Sanderson's Handbook, but believes it describes a large number of experiments which ought not to be allowed, 1110–1.

Dr. Sanderson's Handbook.

Anæsthetics.—Attaches very little value to experiments under anæsthesia, because an animal is no longer itself under such abnormal conditions, 1077–83, 1085.

Experiments such as those relating to the discovery of the circulation of the blood, are not however open to this objection, 1084.

Anæsthetics alter the conditions and vitiate results.

Curari.—Knows that curari is a poison, but is not aware of anything tending to show that it is an anæsthetic; believes it has never been tried on the human subject, 1071–6; and very much doubts that it makes animals insensible, 1077.

Regulation of Vivisection.—Considers a remedy for existing abuses desirable, but it is very difficult to point one out, 1046–7; the subject is more difficult of regulation than dissecting. Scientific men should not be trammeled, but enthusiasts should be restrained within

Licences.

bounds, 1045. Thinks the appointment of an officer to attend all these experiments would be offensive to all scientific men, 1058, and the plan of granting licences for these experiments of questionable policy, 1060–1.

Force of public opinion better than legislation.

But a formal opinion should be expressed by the legislature.

Would leave the control of these experiments to the influence of public opinion rather than legislation, 1055; considers that the greater humanity of treatment existing in English hospitals is due to the operation of public opinion, 1056; thinks Government could only interfere by recommending carefulness to scientific men with regard to performing such experiments, 1041, 1054, 1057; but believes that the expression of the views of the Houses of Parliament or of Government would result in a man's losing caste by going beyond certain bounds, 1041–3, 1062–3; and thinks therefore that it would suffice that the Commission should strongly express their views on the subject, and leave the rest to public opinion, 1059; which would be a sufficient check. Men would avoid gaining a reputation for indifference to animal suffering, 1118–9.

Facts should be made known.

Public opinion and Society for Suppression of Cruelty to Animals would be sufficient check.

Admits that at present the public is not informed as to what experiments are taking place, and that public opinion cannot therefore be now brought to bear on them. Thinks this should be obviated by some change of the law, and that then the force of public opinion, coupled with an expression of opinion on the part of the Government, and supplemented by the action of the Society for the Suppression of Cruelty to Animals, and the restraints of the present law would suffice, 1120–35.

Governors of Hospitals.

Thinks that the attention of the governors of hospitals should also be drawn to the matter, in order that some of their number might be appointed to prevent unnecessary cruelty, 1138.

Private experiments would be reached by public opinion.

Ignorant operators.

Is aware that many of these experiments are performed in private houses, but believes the experimenters would be reached by public opinion in the long run, even if not by the Society for Suppressing Cruelty to Animals, 1064–6; many private experimenters are very talented men, and should not be interfered with unless they were found to perform unnecessarily cruel experiments, 1141; any excess of cruelty on the part of ignorant experimenters should be punished, but fears lest quacks and bone-setters would thereby come to be martyrs in popular estimation, 1136–7.

Record of experiments should be kept.

Inspectors of physiology.

In any legislation it would be very desirable to provide for a record of all experiments being kept, just as is done with such strictness and great advantage as regard subjects for dissection, 1044, 1142–4; provided the Commission finds that these practices were carried to an exorbitant extent, 1147–8; believes it quite possible that the appointment of an inspector of these experiments, having analogous duties to the present inspector of anatomy, would be desirable, 1145.

Amendment of Cruelty to Animals Act.

Thinks it would be highly desirable to include "wild" as well as "domestic" animals under the Cruelty to Animals Act, 1066–9; does not think the argument that this course might apparently tend to stop field sports of any weight, 1070.

ALFRED SWAINE TAYLOR, M.D., F.R.S.

Fellow of the Royal College of Physicians, and for 44 years lecturer on medical jurisprudence and toxicology at Guy's Hospital, 1148–9.

Is the author of a work on poisons and medical jurisprudence which is often referred to in the courts of law, 1150; has performed about 12 or 14 experiments on living animals in 40 years, 1151, 1266; sometimes using the knife to make a slight wound in which to insert the poison, 1152–3; and has in other ways experimented largely in toxicology, 1154.

There is a practical class at Guy's for the chemical examination of poisons; arsenic has been occasionally given to rabbits; but no experiments are performed by students, 1233–5.

Medico-legal Experiments.—Has been employed by the Government in all the great cases of poisoning during the last 20 years, including Palmer's and Smethurst's cases, 1155.

Palmer's case.

In Palmer's case witness and Dr. Rees experimented on six rabbits, and proved that an animal might be killed by a quantity of poison received by absorption, so small that it could not be detected, and that the possibility of discovering poison depended on the time the animal lived after taking it, 1157; one medical witness for the defence experimented on 60 animals, 1165–6; apparently with the object of ascertaining under what circumstances poison could and could not be detected

in the viscera, gives details of the special difficulties on the point in Palmer's case, 1167; no additional knowledge was gained by these wholesale experiments, but it was argued that the four or six cases in which the poison could not be found should not avail against the 100 or so in which it was found, 1168.

Cobra poison.

Instances a physiological experiment performed by himself with regard to the poison of the cobra di capello proving that after having been kept in a dry state for 12 years it had not lost its power, and that it operates by absorption through a wound but not through the stomach, the latter having been a moot point previously, 1160–1.

External arsenical poisoning.

Instances the case of a woman who applied arsenic in an ointment to the child's head for ringworm; the child died and arsenic was found in the stomach. Dr. Pavy and witness found on experimenting with rabbits and dogs that arsenic received into the system externally found its way into the stomach if the animal lived a few hours, a scientific fact not clearly proved before. The child in question was found to have lived two days after the application of the ointment, and in the end a verdict of accidental death was recorded, 1162; thinks this result was a justification for these experiments, 1163; and that such experiments are often productive of benefit, 1164; although as in Palmer's they are sometimes conducted on an unjustifiable scale, 1165–6.

Tawell's case.

In Tawell's case prussic acid was the poison, the action of which is known; there were therefore a few experiments only, 1200–1.

Necessity of Experiment.—Experiments are useful for the discovery of antidotes, 1203; and as new drugs are continually being found, they are necessary for discovering their properties, 1188. Again, when chemistry fails to detect a poison it may be necessary to apply part of the substance said to have produced death to a living animal; gives instance, 1188. In these experiments it is found that the quantities of poison which will kill a dog or a human being, and also the symptoms and effects are much the same, 1202; points out that although the effects of a drug on small birds and frogs may prove it to be a poison, it does not give much information else, 1171–2.

Dogs.

Extent of the Practice of Experiment.—Neither at Guy's nor elsewhere in England is there any reckless toxicological experimentation, except possibly in connection with great trials; the great number of experiments are in France and Germany, 1235–7. Believes that English scientific men perform these experiments only to obtain actual benefits, and that they would support any legislation for restraining it with these limits, 1184; for a particular experiment rarely more than one animal would be necessary, 1263–5; and in England the total number so destroyed is not large, 1267.

Not abused in England,

except in criminal trials.

Believes, however, that these experiments are resorted to more often than is necessary; they were especially so in Palmer's case, 1158, 1195–9; thinks this objectionable in itself, and also because a jury might be influenced by the mere number of experiments; in the interest of science would not however approve of any legislative limitation, 1213–6.

French experiments often useless and cruel.

Many evidently useless painful experiments are performed, especially by French experimenters, 1178. Instances some hundreds of experiments performed by a very eminent French physiologist on dogs with all manner of poisons, which were useless because he introduced a non-natural condition by cutting into the neck and tying the æsophagus to prevent vomiting, 1171. Has studied in Paris; believes experiments are very numerous there, and that at Alfort they are carried out on a very cruel scale, 1269.

Repetition of experiments.

When a thing is once proved repetition is useless, 1169; and many experiments, even by eminent men, have no useful result, 1170; does not think the publicity given to discoveries much affects the number of experiments, 1212.

Experiments for demonstration unnecessary.

Experiment for the sake of demonstrating the known action of poisons, instances strychnine, is purposeless cruelty; has never performed such experiments himself, 1179–80, 1260; with the exception of one or two with regard to the action of the mineral acids, 1227; but as the students did not gain more from these experiments than they did from the drawings and models he discontinued them, 1261.

Considers verbal description, supplemented by coloured wax models of the stomachs of human beings and dogs and by drawings, is sufficient to enable students to recognise the action of ordinary poisons, 1223–7, 1261–2. Has also a large number of stomachs of human beings

Taylor, Alfred Swaine, M.D., F.R.S.

who have been poisoned; uses them for demonstration, 1228.

Cannot remember any instance where the death of an animal is necessary in medico-legal teaching, 1229; the only operation necessary is the slight puncture required to get fresh blood, 1230–1.

Anæsthetics.—Has not heard much of anæsthetics being used in toxicological experiments, 1241; they would render it impossible to rely on results, 1189–90. In many cases too nervous activity is essential; protracted suffering is never, however, necessary, and the animal should be destroyed directly the object had been attained, 1181-3.

Curari.—Believes curari to be a sedative, 1238; and that it acts upon the muscles through the nerves; has never heard of any recovery from poisoning by it, 1191–4. It is commonly used in physiological laboratories in England, 1240. Would not himself use it as an anæsthetic, 1242–3; but thinks the diversity of opinion with regard to it arises from variations in the preparation, 1239. The alkaloid curarina appears to destroy sensation, but it is hardly obtainable; it acts as a poison, but too little is known of it to warrant its general use, 1244–7. The sedative action of curari is said to afford a remedy for the excessive stimulus produced by strychnia, 1204.

Curarina.

Dr. Sanderson's Handbook.

Dr. Sanderson's Handbook is a book of very considerable reputation, 1270; thinks the application of nitrate of silver to the cornea of a frog, in order to produce inflammation, would not be very painful, 1256–7; considers putting a frog, a cold-blooded animal, into water and gradually raising the temperature to 100° Fahrenheit equivalent to boiling it alive, and that it would be a cruel and apparently purposeless experiment, 1258–9. The extraction of the mesentery of the frog for observation, referred to in Dr. Sanderson's Handbook, must be a very painful experiment, and witness cannot see what object it can serve, 1271.

Boiling a frog alive.

Extracting the mesentery of a live frog.

Norwich experiments.

Considers the Norwich experiments were most cruel and unjustifiable, 1188.

Chronic poisoning.

Chronic poisoning is rare, and has only occurred when the criminal has some medical knowledge, 1207; thinks it desirable that the knowledge of the action of poisons, which cannot be detected chemically, should be confined to the profession, 1208–11.

Strychnine.

Strychnine is a spinal poison and kills all vertebrate animals, but not the invertebrate; gives instance, 1174. It was not made known before the trial of Palmer, although a case of poisoning by it occurred in 1831, when the symptoms were set down to tetanus, 1220–1. Thinks it is quite unnecessary to illustrate its action by experiment, it can be accurately described; there is something dreadful in its operation, 1218–9, 1222.

Prussic acid.

Placing a dog under a tap of cold water has been found to restore it when poisoned by prussic acid, and a similar course has been found useful with human beings, 1204.

Snake poison.

Considers the discovery of an antidote for snake poisoning unlikely, as it takes effect so rapidly and could only be combatted by some substance at once entering the blood, 1205.

Sensibility of animals.

Thinks that the sensibility of frogs and other anirnals varies in proportion to the perfection of their nervous system, and that the excitement of the motor nerves does not necessarily imply the existence of pain, 1554–5.

Exemption of certain animals from experiment.

Regulation of Experiments.—Thinks that any law should define the classes of animals that were not to be experimented upon except under restriction, and that some competent person should be appointed to assist the magistrates in deciding whether a given experiment were necessary, 1185; a point which requires much scientific knowledge, 1175–8.

Licence system.

Is inclined to approve of the suggestion that licences should be granted to fit persons to perform these experiments, but a great deal must in any case be left to scientific men, 1186, who are as a rule humane, 1187.

Licensed laboratories.

Thinks that the performance of toxicological experiments might be confined to certain licensed laboratories, except in cases where despatch would be necessary and which might be specially provided for, 1248–53.

GEORGE ROLLESTON, M.D.

Linacre Professor of Anatomy and Physiology at Oxford, 1272, 1333.

Is a member of the committee, formed by representatives from the Universities and the Colleges of Physcians and Surgeons, for the purpose of preparing a conjoint scheme for medical examination, 1274–5.

Rolleston, George, M.D.

Witness' own experiments have been on a very limited scale, being quite as much an instructor of ordinary English gentlemen as of young doctors, 1342–3.

Resolutions of the British Association.

Drafted the resolutions passed by the British Association at Edinburgh, 1273, separating experiments for investigation from experiments for demonstration, and insisting that the latter if performed should be made under anæsthesia, 1278; wishes they had also required animals to be killed before awaking to consciousness, 1276–7.

Results obtained by Experiment.—Admits that it is one only among many means of gaining knowledge, 1280; but considers that it has thrown great light on those terrible diseases diabetes and epilepsy, 1280, although a remedy may not yet have been discovered for them, 1302; and that the possibility of tying an artery without starving the limb is another instance of its usefulness, 1280.

Diabetes and epilepsy.

Tying arteries.

Action of poisons.

Necessity for Experiment.—Testing the action of poisons on animals sometimes, as in Palmer's case, necessary, 1280; as it might also be if a man died after eating some particular substance the nature of which was not known, 1280. Points out that the effects of a drug on animals is no certain guide to its effects on man; a rabbit will eat enough belladonna to poison a dozen men without injury, 1280.

Effect on animals not a certain guide.

Belladonna.

In Ireland.

Extent of the Practice of Vivisection.—In the Irish School of Physic vivisections for teaching purposes are strictly prohibited, 1286. In the recommendations of the committee for forming a uniportal system of examination and in the bill introduced by Sir John Lubbock actual vivisection is expressly excepted from the subjects to be required from students, 1275, 1283; those intending to become physiological investigators might, however, practise it, 1284–5.

Sir John Lubbock's bill.

At Oxford.

No student is allowed to experiment in the University Museum at Oxford, nor is there any reckless experimentation there, 1334–5; does not show students going in for examination experiments upon live animals at all, but a few under anæsthetics to those who graduate in arts through a science curriculum, 1336–7.

Professor Schiff.

In Professor Schiff's work almost every lecture is accompanied by the sacrifice of some animal; 700 a year, it is stated, thus perish, 1343. Calls attention to a discrepancy between two statements of Professor Schiff, one stating that the dogs made no noise because they were not in pain, the other stating that their nerves of vocalization had been cut "*pour les empêcher* * * "*de discréditer ainsi les études physiologiques auprès* "*des habitants du quartier*," 1287.

Private experimentation.

Knows that private experimentation exists, but cannot say to what extent it is carried on, 1332. Doubts how far it can be brought under control, 1291–2, 1301. It should not be forbidden, and would be controlled indirectly by the restraints placed on public experiments, 1303–5, 1330.

Causes tending to produce abuse of practice.

Limits of Experiment.—Considers these experiments are very liable to abuse, investigators are carried away by the absorbing nature of their researches, 1286–7; and that lecturers are under considerable temptation to show off their skill in these experiments. Argues with Mr. Skey that "a splendid operator is ever a just object of suspicion," 1287.

Thinks the desire of impressing facts on the mind through the medium of the sight sometimes leads to extravagance in experimentation, 1286; thinks the practice of vivisection is abused just like that of taking alcohol, but does not think therefore it should be forbidden, but considers that it should be controlled, and that the number of experiments might be much diminished without injury to science, 1293.

Address to section of British Association in 1863 on vivisection.

Reads extract from the Transactions of the Biological section of the British Association for 1863, containing the gist of witness's address on vivisection, pointing out that sensibility was usually extinguished more speedily by the physiologist than by the butcher or sportsman, and that valuable knowledge had been gained by these experiments with regard to epilepsy and diabetes, but that they could not be justified when performed for the sake of acquiring operative dexterity only, 1273.

Effects of experiments before a large number of men.

Believes that painful demonstrations are demoralising, 1299–300; especially when performed before large numbers, and that they act on the emotions-motor nature and wake "the sleeping devil in the hearts" of men. Quotes Kingsley and Lipsius in support of this, 1287, 1338. Considers therefore that such experiments should be performed before a very limited number of individuals only, 1279, 1308–11; and should be subject to regulation, 1313. Thinks painful experiments for demonstration to students should be forbidden, 1306–7, 1309, 1314; a few cardinal experiments, however, may be desirable to enable students to appreciate records of other experiments, provided anæsthetics be used and the experiments be such as cannot be otherwise demonstrated, 1283, 1291, 1294–5; and that the animals were always killed before recovering from the anæsthetic, 1315–6.

Painful experiments not allowable in teaching.

Very few painless ones are necessary.

Uncertainty of effect.

Anæsthetics.—The whole question of anæsthetizing animals has an element of uncertainty about it, 1350; it is not easy to know when an animal is anæsthetized, and there is a great difference with regard to the duration of its effects on different individuals of the same species, 1349. Is inclined to think that a large dose of curari acts as an anæsthetic, 1350.

Curari.

Dr. Sanderson's Handbook of Physiology.—With regard to the portion contributed by Dr. Michael Foster, points out that experiments may be trusted to such men which would not be rightly left to the ordinary physiologist, 1344; admits that the experiments are obviously described for the purpose of being performed, 1346–9. A frog killed by placing it in water and raising the temperature to 100° Fahrenheit, would not be "boiled or blistered," as albumen either in the frog or in the human being does not coagulate under 140° Fahrenheit; thinks, however, such an experiment should not be repeated, 1345–6; in the experiments described, the frogs are in many cases pithed, in others put under the action of curari only, 1352–4; is sorry to have to say that he thinks the handbook to a great extent warrants the present public feeling on the matter, and regrets that it is likely to represent its authors as something other than one at least, Dr. Foster, is, 1351.

"Boiling" a frog alive.

Effects of the handbook.

Is of opinion that there is, in spite of a downward tendency during the last year or two, 1339, a growth of moral sentiment in the human race generally tending towards greater carefulness in the infliction of pain, and that a similar development takes place in individual minds; quotes the "Philosophical Transactions" of 1667, and "Spectator" of 1874 in support of the general proposition, and refers, with regard to individuals, to Haller, who in his latter years fell into a "permanent anguish of conscience" at having vivisected to such an extent as he had, 1290; a man in his maturer years will condemn what he did in his youth, a child will pull off a fly's wings without remorse, a man would think it extreme cruelty, 1296–7; thinks therefore that society may rightly control the individual, 1298.

Popular feeling is growing more humane;

and individual feeling becomes more so in mature years.

Regulation of Experimentation.—Quotes opinion of Dr. Child that legislative interference, which was not necessary 10 years ago, is now required, the inclination to resort to experiment being now so much greater, 1340–1.

Public laboratories.

Thinks very little inconvenience would be caused by forbidding experiments to be performed except in public laboratories; experiments necessary in poisoning cases, however, should be exempted, although generally these even might be sent to a public laboratory, and be performed there by experts, 1327–9, 1331; with regard to private experiments, is inclined to say "*de minimis non curat lex*," 1323–6; thinks an inspector of physiological laboratories, with duties analogous to those of inspectors of anatomy, would probably be beneficial, 1319–22, 1324; and that a record of all experiments made and the reasons for making them should be kept, 1317–8.

Inspectors of laboratories.

JOHN SIMON, M.R.C.S., F.R.S.

Medical Officer of the Privy Council and Local Government Board, 1354–5.

Has been engaged in various pathological investigations, 1356–7, 1396; but, with the exception of some experiments on the inoculation of tubercle in 1866 or 1867, has not recently performed experiments himself, 1472; has also for many years been surgeon to St. Thomas' Hospital, 1440; and has been connected with the hospital for about 42 years, 1450–3.

Necessity and Results of Experimentation.—Justifies experiments on animals on the ground that by them the causes of diseases are found out, and that this is the first step towards preventing them, 1409, 1358; the physiologist seeks to discover facts, and these may, and often do, render improved treatment possible, 1462; the knowledge obtained by the physiologist is used by the general practitioner, 1460–1.

Ligature of arteries.

Mr. Jones' experiments in connection with the ligature of arteries are the foundation on which the present practice rests, 1446–7. Hunter himself lost patients from want of knowledge on this point, 1448–9. The knowledge of the nature of inflammation and of fever derived by experiment is so important that witness relies upon it in preference to that obtained from other sources, 1443.

Discovery as to nature of inflammation and fever.

Believes that experiments on animals have tended immensely to the advancement of surgical science, 1442; and that those eminent men who have considered the subject will concur in saying that these experiments are necessary, but should be conducted with all possible regard to humanity, 1368.

Medicine and surgery are based on clinical observation, physiological research, and experimentation, 1444–5; and experiments on animals obviate much needless experiment on man, 1442; wishes to rest the case for these experiments on the general basis that exact knowledge is the foundation of preventive or curative treatment, and is therefore most desirable, whether an immediate benefit follow or not, 1417, 1420–1.

The advancement of knowledge sufficient justification for these experiments.

Puts in a pamphlet on vivisection, written he believes under a pseudonym, by a man of eminence, 1511–4.

Government Investigations.—There is an annual grant of 2,000*l.* made by Government for medical scientific investigation, one half of which goes to aid investigations in which pathological experiments on animals are sometimes necessary, 1360–2; a great many of these are on the dead subject, 1363–4.

Grant for investigation.

Reads an official note by Mr. Forster recording his opinion that experiments made at the cost of the state should only be performed, if painful, under anæsthetics, and that a record should be kept of each, stating its aim and the necessity for it, 1367; this minute was referred to Dr. Sanderson "to read and note as an instruction," 1511.

Mr. Forster's instructions

Reads his own official report setting forth that these experiments are sometimes necessary to obtain exact information as to the causes of disease, as for instance those with regard to cholera, sheep-pox, and cattle plague; that they are sometimes necessary to test the value of alleged remedies; that they often are attended with little or no pain beyond that of the disease; that animals should be destroyed when the object of the experiment had been obtained; that anæsthetics should be used when possible, and that the experiments performed on behalf of the Government had been strictly confined within these limits, and only undertaken with the object of obtaining knowledge beneficial to humanity, 1367.

Objects of official investigation.

Dr. Sanderson, Dr. Klein, Dr. Creighton, and Dr. Baxter have been the investigators, 1473–4; and among the chief subjects which have been studied are cholera, tubercle, præmia, sheep-pox, and disinfectants, 1359.

In the experiments to test the infectability of animals by choleraic discharges, mice were generally the animals selected, the only pain was that caused by the disease induced, 1364–5.

Cholera.

By experiments on animals, consumption is found to be inoculable; points out the emphasis this lends to certain observed instances of apparent infection in human beings, 1420–4.

Consumption.

Admits that no improvement of treatment has resulted from the experiments with regard to sheep-pox, but believes the increased knowledge of the contagion of the disease will result in rendering prevention easier, 1417–9; thinks it probable that there is considerable pain with the sheep-pox, 1431; but the inoculated disease is usually very mild, and after temporary inconvenience and distress the animal would usually recover, 1432–6.

Sheep-pox.

Valuable experiments have been recently made with regard to disinfectants, the results of which will probably form the basis of the advice given to the public for years to come, 1474.

Disinfectants.

Typhoid fever is a form of blood poisoning, but has not been artificially produced by them, 1470–1; scientific results intimately related to practice have been obtained by experiment in pyæmia; intends shortly to publish some which will be in print, 1465, 1469; the duration of very painful experiments would be reckoned in hours, 1466–8.

Typhoid fever.

Pyæmia.

Is now making investigations, but not on animals, who do not appear to be liable to it, with regard to scarlet fever, in the hope of obtaining a knowledge of its causes and of so preventing its spread, 1425–9.

Scarlet fever.

Admits that pathological experiments sometimes give fallacious results, but believes every effort is made to guard against it, 1482.

In the Government investigations witness has never had to perform a severe surgical operation, and in linger-

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ents.

ing diseases the animals are put out of pain as soon as possible, 1430, 1438–9. Reports are made on all experiments performed; the number and kind of animals operated on are generally mentioned, as well as the objects of the experiments, 1397–405.

Puts in a return showing the investigation made during the last four years, 1368, 1398.

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overnment
ased on
esult of ex-
periments.

Considers that the advice that the Government was enabled to give the public with regard to preventing the spread of cholera by keeping water pure from the slightest taint of choleraic excrement, was entirely arrived at by experiments on animals, although other facts pointed in the same direction, 1406–16.

Extent of Practice of Vivisection.—Thinks the practice is confined in England to a few competent persons, 1369–71; does not believe that these experiments are recklessly undertaken or performed by any appreciable number of persons, 1378–81.

Past and
present
extent of
practice.

Believes the number of painful experiments has relatively and probably actually diminished since the time of Majendie; doubts whether any one in England now performs severely painful operations on the same scale as Mayo did then, 1372–3, 1478–80; has not, however, been working in this direction of late years, and therefore speaks with reserve, 1437.

Most eminent surgeons have been experimenters on animals when definite points required investigating, those who have not were probably experimenters on their human patients, 1459.

Is not aware that experimental physiology has lately been recommended as a study for all medical students, 1494.

Experiments on
frogs.

Students at Guy's Hospital do not perform experiments on living animals, 1451–2; although advanced students probably occasionally use frogs for observations causing pain, 1495; foreign frogs are preferable to English frogs for some physiological purposes, 1496; the use of anæsthetics is general with regard to domestic animals, but not with frogs, 1498; has difficulty in comparing the sensibility of frogs to pain with that of other animals, but does not doubt their full susceptibility to pain, 1499.

Sensibility
of frogs.

Pain rarely
necessary.

Limits of Experimentation.—Believes that it would rarely be necessary for an animal to be sensible to pain during a severe operation, general or local anæsthesia is nearly always possible, 1374–7; considers that great pain may be necessary to achieve a great result, but that it is not justifiable for small results, 1483–4; much must be left to the individual conscience; it may be desirable that the scientific conscience should be influenced by the conscience of the educated public, but certainly not by that of the mob, 1485–7; if abuses exist, scientific men would concur in supporting a remedy, 1383.

Doctors who have performed these experiments are not unfeeling men, 1493; does not know a kinder man than Dr. Sanderson, 1475–7.

Ocular demonstration
often necessary.

Experiments for Demonstration.—Considers that scientific education is necessarily based to a large extent on the evidence of the senses, and the question often arises whether a class is to be instructed by one experiment performed by the professor, or whether each individual should experiment for himself, 1454; such phœnomena as the action of the heart and the vermicular motion of the intestines for instance, can be demonstrated by painless experiments in a way which no other method, either artificial representation or description, can equal, 1464, 1455–8; and sees no reason why experiments before classes should exert a bad influence, 1497; considers moreover that the study of physiology necessarily involves a limited sacrifice of animal life, which is, however, as nothing in comparison with the common uses to which animals are put, 1458; on the other hand, would not be disposed to consider such an eminent man as Dr. Taylor wrong in saying that experiments on live animals are not necessary in toxicological education, 1463.

Limited
sacrifice of
animals
often necessary.

Dr. Sanderson's Handbook.—Thinks Dr. Sanderson's Handbook is an encyclopædia of reference for experts, not an educational handbook for the ordinary student, 1500; it is mostly used in England for anatomical preparations, the painful experiments are undertaken by very few, and then only in part by each experimenter, 1481.

Regulation of Vivisection.—Thinks public opinion has taken an exaggerated view of the matter, and that special legislative influence is unnecessary, 1382, 1384; if there be an evil, the remedy lies in the province of

conscience, not of legislation; has a strong objection to the latter, and cannot conceive a law for the protection of frogs, 1501.

Objects to
special legislation.

Objects to the special case of pain caused by the physiologist being separated from the vivisections and castration of animals by farmers, &c., for which the resulting human advantage is the only justification, 1502; refers to the preamble of Dr. Lyon Playfair's Bill, and thinks it altogether monstrous that society which slaughters animals for its food, emasculates them for its advantage, hunts them, wounds them, and kills them for its pleasure, should treat physiologists, who make these experiments for great human interests, as a dangerous class, to be licensed and regulated like publicans and prostitutes, 1491–2; is aware that several most eminent men had some part in the preparation of Dr. Lyon Playfair's Bill, and that the preamble might be easily modified, 1504–8; but thinks the eminent men from whom the Bill emanated have not quite considered what its operation would be, 1508. Points out that section five of clause three would enable physiologists to be held up individually to popular odium, 1509–10.

Dr. Lyon
Playfair's
Bill.

Believes that limiting experiments to certain places and certain persons would either drive investigators abroad or be a dead letter, 1488–9; it might be proper if any great abuse, which he does not believe, existed, 1490.

Is of opinion that an experimenter not having a reasonable prospect of attaining a result beneficial to mankind, or who was incompetent or negligent in the prevention of pain, would be liable under the Cruelty to Animals Act (1849), for "cruelly torturing" an animal, if it were a domestic one, 1385–6. If the Act does not include such cases, it might be made to do so, 1386; and its terms might be enlarged to include all animals, domestic and otherwise. Frogs and mice, the frequent subjects of experiment, are not, as it now stands, protected by it. Alterations of this nature would introduce a certain elasticity into the law, and it would then devolve on magistrates to decide as to the degree of suffering caused, 1388–95, 1503.

Suggests
enlargement
of "Cruelty
to Animals"
Act.

Mr. JOHN COLAM.

Secretary to the Royal Society for the Prevention of Cruelty to Animals, 1515–6.

Experiments in England generally.—Believes English scientific men generally try to prevent pain as far as possible, much more so than foreigners; does not know a single case of wanton cruelty, 1543–8. Believes that although anæsthetics are sometimes neglected when they might be used, 1530–2, their use is general in England, 1543; and as Dr. Brunton pointed out to witness animals would be narcotised to facilitate the operation if for no other purpose, 1568, *page* 81. In experiments producing protracted pain in which the use of anæsthetics has not been attempted, it would probably be found that their employment would have defeated the object of the experiment, 1540–2.

Anæsthetics
sometimes
wrongly
neglected.

Desires to point out that the society does not advocate the total abolition of experiments, 1567; but believes that many experiments are of such a nature that their object does not justify them, 1549; considers, for instance, the experiments of the Medical and Chirurgical Society on dogs, who were kept under water during various periods of time, then resuscitated, and half drowned again, was very cruel and, as in the end it proved, useless, 1533; and points out that in some of Brown-Séquard's experiments animals were kept suffering for weeks, 1544.

The Society
desires regulation, not
abolition.

Cruel experiments.

Admits that it is difficult to say when a truth has been established, 1551; but objects entirely to experiments for demonstration, 1551; they should be forbidden; incompetent persons should be prohibited from attempting experiments; and anæsthesia should be insisted on, 1569.

Experiments for
teaching
purposes
should be
forbidden.

Puts in statement quoting, with references, reports of various classes of experiments, showing that some were painful, others both lingering and painful, others performed to teach vivisection, and in one instance were undertaken by students alone, 1533–9; foreign experiments are omitted, except when recommended in English publications, 1569 (*Appendix IV.*).

Statement
of objectionable experiments.

Proceedings of the Society with regard to Vivisection.—Reads a letter from the society referring to witness as the society's representative, and stating what course the society has taken with regard to vivisection, what means it has adopted to get evidence, and what information it proposes to lay before the Commission, 1568.

Operations for acquiring surgical dexterity in France.

The society opposed several years ago with much success the practice of operations for the sake of acquiring surgical dexterity, which were then in vogue in the veterinary schools at Alfort and Lyons, 1568, *page* 79.

Vivisection thirteen years ago.

Thirteen years ago the society sent letters to the medical schools of England asking for information with regard to vivisection which was willingly given, and although the operations were very few in number, it was deemed advisable to offer prizes for the best essays on certain points; puts in the essays referred to, 1568, *page* 79.

Prosecution at Norwich.

Results of application to the medical schools for information.

Puts in a report of their proceedings against Dr. Magnan and others for cruelty to dogs at the Norwich meeting of the British Medical Association, 1568, *page* 80. Puts in a copy of the memorial presented to the society asking it to try to repress vivisection, 1568, *page* 80. On receipt of which a committee, the names of the members of which shall be given, 1701–2, was appointed to consider the matter, and after long and frequent meetings reported that they had the greatest difficulty in obtaining information on the subject; that they had addressed a circular to the hospitals and schools of the metropolis, asking the managers to supply particulars and to permit the secretary to be present when experiments were being performed; that from 14 institutions no replies had been received, in 30 no experiments were performed, from four evasive replies were received, and in five admission was refused, 1568, *page* 80.

The memorialists were also asked to supply particulars with regard to the vivisection of which they complained, but no specific information was obtained, 1569; believes this committee were of opinion that vivisection had not improved the treatment of disease, 1608–700.

Frequency, nature, and abuses of experiments in England.

Extent of Practice of Experiment.—Considers that the medical journals and works of physiologists prove that vivisection is frequent in this country; that anæsthetics are frequently not employed; that the object of these experiments is sometimes discovery, sometimes demonstration; that the same experiment is repeated many times; that painful experiments, where the suffering lasts days or even weeks, are not infrequent, and that students are being instructed by means of handbooks, &c. in the art, 1569; and the results of his own personal observation confirm this opinion, 1518–9; and make him believe that continental views are making rapid progress in this country with regard to vivisection, 1569; Dr. Hoggan referred to in support of this statement, *ib.*

Anæsthetics are used in demonstration.

From the evidence with regard to schools, it appears that there are few operations before classes, but many exhibitions of animals for demonstration after operation under anæsthetics, and some private vivisection by students; puts in a paper on the latter subject, 1569.

Guy's Hospital.

Witness gives detailed account of his visit to Guy's Hospital, in the course of which he ascertained that students are never permitted to perform experiments or to see them, except on insensible animals, 1568, *page* 80.

Learned from Dr. Pavy various details connected with experiments; saw some of the animals to be experimented on, and was informed that about 18 animals a term were used for purposes of demonstration under anæsthetics, 1568, *page* 81; and that Dr. Pavy's experiments were always conducted under anæsthetics and the animals usually killed before consciousness returned; even when consciousness is allowed to return pain is often nearly if not quite absent; instances a dog in which biliary fistula of the stomach was produced, and refers to absence of pain after amputation in the human subject, 1568, *page* 81; ascertained from Dr. Pye Smith that his vivisectional experiments were always conducted under anæsthesia, and that experiments with new drugs were being then tried on animals, 1658, *page* 81.

St. Bartholomew's.

At St. Bartholomew's saw Dr. Brunton perform several experiments in his private laboratory on cats and frogs wholly or almost entirely insensible, 1568, *page* 81; the object of the experiments was to ascertain the nature of the action of certain poisons, and to seek an antidote; the animals were destroyed, 1569.

Has inquired at every school in London, and has not found one in which the operation is performed before the students, the animal is operated on in the laboratory and then brought out for exhibition, 1550.

Method of obtaining animals.

The animals are obtained in a secret and confidential way, 1569, which was discovered by watching and inquiry from the vendors; gives some particulars, 1637–43.

Experiments by students.

Reads a letter from Mr. Mills, formerly student at Edinburgh and sometime lecturer at the New Veterinary College there, and now attached to the Royal Artillery, stating that he had himself witnessed vivisections by students at Edinburgh, made from idle curiosity and from reckless love of experimentation; details a most cruel operation on a cat to show the action of the heart; the victims in these experiments were dogs or more often cats caught by a poisoned bait or a "cat hunt;" in a few instances the animals were narcotised; these experiments were not known of by the professors, 1622, 1682–8; no case of experimentation by students has been found in London, 1683.

Section of a live lobster by a lady physiologist

A live lobster was cut up by a lady physiologist in the presence of a class of girls, and part of it afterwards crawled on the table; it was subsequently explained that the lobster, which had been brought in a bag with a dead rabbit, was believed to have been dead, 1569, 1614–21; at Billingsgate lobsters are killed instantaneously by being put into boiling water, 1644–6.

Popular lecture by Dr. Ferrier.

Alleged levity of description.

Professor Ferrier, in a popular lecture about six months since at the London Institution, described in humourous terms, amid great amusement, the involuntary contortions produced after the removal of a portion of the brain by irritation of certain parts of the cerebral hemispheres in the monkey, 1569, 1573–92, 1595; admits that the experiment which was not shown to the audience must have been commenced under chloroform, and that the subsequent portion, if the brain were in part removed, would also be painless; and does not refer to the incident as showing any cruelty on the part of Dr. Ferrier, but as showing the levity with which these experiments are regarded, 1596–613; believes there were no reporters present, but an account of a similar lecture was published in the north of England, 1593–4, 1609–10.

The Handbook for the Physiological Laboratory.

Regards Dr. Sanderson's handbook as an attempt to train students to the practice of experiments. It says in the preface it is for beginners, and describes in detail the appliances necessary, 1623–6; thinks that teachers of physiology advocate experiments being shown to students, and often their being taught to perform them; gives examples, 1627–31.

Puts in draf of Society's bill.

Proposes that *all* painful experiments should be forbidden

Regulation of Experimentation.—The society has embodied its wishes in a bill (*Appendix* III., § 5,) 1520–2, in which some of the provisions of Lord Henniker's and of Dr. Playfair's bills have been adopted, 1557–8; the society would like the bill passed unaltered; failing that, in as nearly as possible its present form, 1695; it differs from Dr. Playfair's bill mainly, in providing that *no* painful experiments should be allowed, 1559, 1561–2, 1565, even for purposes of investigation, 1527; the section of a polyp, if that causes pain, would be forbidden by the bill, 1659–60; it also provides if an experiment be performed under anæsthesia that the animal shall be destroyed before recovering consciousness, 1523–6; and forbids those which entail pain during recovery, even if performed under anæsthesia; the pain, for instance, after the ligature of an artery is often severe, 1528–9.

Such experiments as Bell's woul be forbidden.

The society as a whole disapproves of experiments for investigation when performed partially under anæsthesia; admits that of the individual members of the society some would go farther, some not so far, 1552–6. Hopes that such experiments as those on dogs with regard to suspended animation will be forbidden, 1655.

Also those which resulted in Palmer's conviction.

And those being made to discover an antidote to snake poison.

Admits that Bell's great experiment on the roots of the nerves would be stopped by the proposed bill, but points out that only one experiment was performed, and that only, Sir Charles Bell said, to convince people who did not use inductive reasoning, 1647–8; admits that any similar experiment would be forbidden by the bill, although it might be of equal importance, 1649–54; such experiments as those with regard to the action of strychnine, which had so much to do with the condemnation of Palmer, would be forbidden by the bill in its present form, 1692–4; as also those now being carried on by the Government of India, for endeavouring to find an antidote for snake bite by inoculating animals with the poison; quotes Sir W. Fergusson's opinion that these experiments are useless, 1689–91; will not affirm that an antidote can be discovered in any other way, but it might, 1697; believes the committee had the question of snake poison before them, 1696.

Also theoretically those on sheep-pox and choleraic discharges.

Admits that experiments in which sheep have been inoculated with the small-pox would, assuming them to be painful, be forbidden by the bill, 1656–8; and that the experiments with regard to choleraic discharges, which had such beneficial results, would technically come under the terms of the bill if they caused much pain to animals, but the society would not prosecute any more than in the case of a horse over-ridden to save life, 1678–81; points out that the Cruelty to Animals Act is liable to the same theoretical objection, under it the sheep-pox experiments might be argued to be "torturing" animals, but public opinion does not sanction such a view, 1663.

ut "cruelty" must be easonably nterpreted

Does not think demonstrating the circulation of the blood by the exhibition of the web of a frog's foot would come under the head of painful experiments, 1661–2; the society does not wish the wording of the bill to be qualified as regards experiments giving pain, 1676; but that a reasonable interpretation should be placed on the word "cruelty" as in other instances, 1671–2.

y the court, and experts alled if needed.

Does not wish to imply that the society should be empowered to act as judges on physiological investigators, 1665–7; but believes that the society could judge whether the results were useful without a knowledge of technicalities, 1669–70; or if not, whether a case would be a fit one for commencing a prosecution, and the court would determine whether cruelty existed or not, 1672–4; experts as in the Norwich case might be called on either or both sides, 1675.

The society would wish in any bill that the existence of what should reasonably be included under "cruelty" should be left to the court, 1677.

Record of all experiments

Licences.

The bill also provides for the record of all experiments, so that the value of the results might be known, and that by the publication of the results repetition might be made unnecessary, 1560, 1664–6, 1668. Like Lord Henniker's bill, it provides for licensing certain places, and extends this course to persons also, 1563–4, 1566.

Search warrants.

Has never known directly or indirectly an instance of cruelty to animals by medical men in their own houses, 1632–3; but believes that when licences have to be obtained, there will be a temptation to experiment without them; provision was therefore made for the issue of search warrants; has known many cases of strong suspicion in which they would have been asked for if the power of granting them had existed, 1634–6; in one instance at least there was reason to believe the law was being infringed, 1559–60.

ARTHUR DE NOÉ WALKER, M.D.

Was originally a military surgeon, served in the Crimea, and is now practising as a physician in London, 1703–6.

Was formerly a student in two or three physiological laboratories on the continent, and has kept himself acquainted with what has been published both at home and abroad, 1707–8; devoted especial attention to the subject in the years 1856–8, and again in 1872, 1709.

Has been in communication with the Secretary of the Society for the Prevention of Cruelty to Animals, but had compiled notes, in the hope of appearing before the Commission, beforehand, 1720.

Extent of Practice of Vivisection.—The practice of vivisection is a new phase of research, and is extending, 1717.

Draws a very marked line between pure physiologists and practical medical men, 1775–6; the former inquire into the functions of different organs irrespective of results, 1777; while many eminent medical men have never performed a single experiment, 1778.

Classifies experiments.

Divides vivisections into classes according to their objects, viz., those with physiological objects, those for the sake of ascertaining if a proposed operation is likely to be successful, those for acquiring manual skill in surgery, those for studying the effects of injuries, 1721, *page* 89, considers experiments under this head are very numerous and often aimless and ill-conducted, 1726, and has therefore made them the subject of a separate paper, and lastly, those relating to electro-physiology, to the artificial generation of morbid states, to the experiments testing the effects of drugs and other substances, and to these last experiments he wishes to specially refer, 1726.

Excessive amount of experiment.

Considers there is a great deal of wanton and unnecessary suffering in foreign laboratories, 1710, 1712; judging from the voluminous accounts of vivisections in the "Gazette Medicale de Paris," 1712–6; from the English medical press it would appear that some such experiments are performed in this country, 1711, and also that the number of experiments is very excessive, 1810; believes that a very large number of experiments involving pain are performed by incompetent persons, 1800.

Refers to the "liberal" supply of animals for experiment provided at the West Riding Lunatic Asylum, as evidence that the practice is extending, 4888, *page* .

Has made one or two attempts to get into laboratories in England, but found it impracticable, 4904, being known as having written against the practices going on

in laboratories, 4906; would rather not mention the names of the laboratories, 4905–9; gives another instance of refusal to allow admission to a laboratory, 4904.

Alleged value of Physiological Discoveries.—Physiologists base their justification of vivisection on the therapeutic value of their discoveries; without going so far as to say that physiology is of no use to the physician, witness maintains that it has never led to the discovery of a single remedy or to improved treatment, 1721, *page* 89. Science has made as many blunders as empiricism, 1770.

Sir William Gull.

Professor Rivière.

Quotes Sir W. Gull's opinion, that "the clinical "physician knows that the phenomena of disease are "not explained by the phenomena of healthy texture, "nor by the action of healthy organs," 1749; refers to Professor Rivière's assertion "that the discovery of the "circulation of the blood had not advanced medicine a "single step," 1721, *page* 90; and would agree with him if he had said "therapeutics" instead of "medicine," does not think he meant his observation to include surgery, 1789–90.

Physiology is of little practical value except to localise disease.

Contends that physiologists mislead themselves and their pupils, some even saying that "physiology is the "foundation stone of pathology," yet the physiologist knows nothing of the nature of marsh miasma, for instance, or its action on the frame, while the physician knows that quinine will cure the disease, 1721, *pages* 89, 90.

Maintains that a knowledge of the functions of an organ in health is no guide of itself to the treatment necessary in disease, 1721, *page* 89, 1738; the knowledge of the functions of the liver or kidneys is no use in diagnosing the diseases of those organs, 1739–42, 1758; refers to scurvy, diabetes, gout, intermittent fever, and other diseases, and points out that in each case the remedies used were discovered by the physician, and that none have been suggested by the physiologist, 1721, *page* 89.

Ringworm.

In ringworm many efficient remedies were known before the disease was discovered to be produced by a parasite; admits that this knowledge however was a great advantage, it placed a definite object before the pathologist,—kill the parasite and the disease is cured, 1762–3, 1769.

The sounds of the heart.

Was not aware that the exact cause of the second sounds of the heart had been ascertained by physiological research, but maintains that although anatomy and physiology may help to determine the seat of the disease, they are no further help, 1749; and points out that physicians were the discoverers of the action of digitalis, aconite, &c. on the heart, and that physiologists are unable to explain the causes of such action, or to suggest other substances which would act analogously, 1721, *page* 89.

Ligature of arteries and transfusion of blood.

Has not any personal experience with regard to vivisection for surgical objects, but admits the value of the knowledge of the possibility of the deligation of arteries and of the transfusion of blood; considers the latter however an invention rather than a discovery, and believes that these and all similar discoveries were made by physicians and surgeons, not by physiologists, 1721, *page* 90; thinks the discovery that nitrite of amyl was a remedy for *angina pectoris* was a lucky guess, any agent having an elective affinity for a given organ is generally tried for most diseases of it, 1781–2.

Angina pectoris.

Experiments for acquiring surgical dexterity.

Believes experiment for the sake of acquiring manual dexterity is never practised in England, 1793–5; Gooch excised the rib of a dog to see if it might be possible in the human subject, 1793–4; when a student abroad, a surgeon trephined the skulls of six goats with the object of discovering its effect by post-mortem examination; considers this purposeless, 1721–6, 1791–2.

Majendie's experiments in feeding animals exclusively on gelatine or on albumen were dietetic rather than therapeutic experiments; witness was present at the latter, 1783–4.

Small-pox.

Sheep-pox.

A *full knowledge* of the processes of small-pox and other diseases would be desirable if possible, but even this would not point out the remedy, 1761, 1764, 1767; physiology discovered the existence of the *acarus scabies*, but pathology that sulphur would destroy it, 1761; is aware that sheep-pox is the work of a small organism; thinks this knowledge important as pointing to the possibility of prevention rather than as suggesting a cure, 1765–6; admits that the knowledge of the processes of inflammation is due to experiment, but such knowledge has not led to the discovery of any remedy, 1760.

Localisation of disease.

Considers that etiology, pathology, physiology, chemistry, and a knowledge of the action of remedies all are of use in the treatment of disease, and that the part of physiology is to localise the disease, 4911–2; especially in nervous affections, 1739; it usually, but not always, effects this, 1759; but is of no further use, 1721, *page* 90; 1750-3, 1757.

Believes the eminent medical man who stated that he was generally greatly indebted to physiologists in his practice was wrong if he meant more than that physiology enabled him to localise a disease, 1779-81.

Pathological experiments are useful.

Admits that the practice of experiment by the induction of disease has been too recently introduced to bear much immediate fruit, 1785–6; and concurs in thinking that any further advance in pathology must be made by means of the microscope and pathological experiment jointly, 1787–8.

Admits that the functions of the various parts of the nervous system have been ascertained by experiment, 1754–6.

The abnormal conditions of these experiments interfere with the results.

Points out that experiments on live animals are constantly made under conditions which could not arise naturally, and that the results are often therefore useless, 4888, *pages* 243–4; has witnessed various experiments in which an inflammatory state has been artificially induced; the experiments were most painful and gave no rest day or night to the victims; maintains that any knowledge thus obtained is generally rendered valueless by the fact of its having been induced in a previously *healthy* tissue, and not being the outcome of a morbid state, 1727–8; instances many cruel methods of introducing poisons into the system of animals, and points out that these methods never occur naturally, and that an unnatural condition is introduced into the experiments by them, 4888.

Unnecessary prolonged suffering and useless cruelty.

Biliary fistula in cats.

Animals are kept alive much longer than they ought; instance the case of frogs who are kept nailed down until nature wears out under electric experiments, 1729–30. Has himself seen frogs kept for months in closed jars until ulcers formed around the mouth; even then the man in charge was not blamed, 1730–1; instances the injection of boiling water into the stomach and sand into the veins of dogs, as examples of wanton cruelty; will not say that the operators had no object in view, but certainly no demonstrative purpose was served thereby, 1802–8, 4888, *page* 244; "would have suspended from rank and pay" the physician who vivisected 16 cats with nearly the same results; considers such a man ought not to be licensed as an experimenter, 1810.

Puts in a statement of experiments which he desires to bring before the Commission, 4887–8. The descriptions are taken from the writings of Claude Bernard, Schiff, and others, from the "Medical Times and Gazette," the "Gazetta d'Italia," &c., 4888, *pages* 244–5; in these descriptions no reference is made to anæsthetics; in many instances they would be impracticable; in others, if used during the operation, the prolonged suffering afterwards remains, 4889–90.

Details of uselessly cruel experiments.

Refers to several of the experiments described; characterises as unnecessary and cruel experiments, starving rabbits until they ate dead frogs, 4888, *page* 244; 4898–9; in the experiment in which portion of a living frog is placed in the stomach of a live dog to show that the gastric juice will dissolve living substances; points out that dogs do not swallow live frogs; that the frog might have been removed as soon as the action was evident; that it might have been pithed; and that its repetition for demonstration was cruel and needless, 4888, *page* 244; admits that birds swallow frogs alive, but thinks this goes to prove the experiment unnecessary, 4902–3.

Non-natural food.

Starvation.

Starvation and wounding.

Injection of boiling water.

Cerebral fungus.

"Frightful struggles" of a cat.

Considers forcing Liebig's extract of meat into the stomachs of rabbits was an experiment against nature, 4888, *page* 245; but is aware that in Norway cows are fed in winter on a mixture of stockfish and cow dung, and has also known them fed for months on lemons, 4900–1; dogs and rabbits having the fifth pair of cranial nerves severed, and being thus unable to swallow are allowed every season to die of hunger instead of being killed, 4888, *page* 245; doves and pigeons starved until they were too weak to stand, and then subjected to amputation of one of the extremities which produced immediate death, showing, what every old dame in Europe knew, the combined effect of suffering, starvation, and wounds, 4888, *page* 245; considers placing, first, a frog with the medulla oblongata divided, and then a sound frog, in cold water and raising the temperature until they are killed, horrible; also the injection of boiling water into the stomach of a dog, 4888, *page* 244; puts in sketch of a frog nailed to a board and deprived of food, in order to test from time to time the effects of nervous exhaustion, 4888, *page* 245; refers to an experiment in which an animal was kept alive with the greater part of one of its cerebral hemispheres exposed for five days, during which it gave rise to a fungus; points out that under a proper system of control the necessity of thus keeping the animal alive would have been called in question, and that various other physiological problems might have been solved at the same time, and the necessity of further experiments avoided, 4888, *page* 243; refers to "one of the frightful struggles so common in physiological laboratories," in which meat, steeped in decoction of colocynth, was thrust down a cat's throat, only to be rejected, whilst another cat, in which the glosso-pharyngeal nerves had been excised, ate some, 4888, *pages* 244–5; 4891–7.

Anæsthetics.—Believes it to be literally true that "anæsthetics are used," but it is nevertheless often a pure delusion to believe that the animals are kept in a state of insensibility, 1810.

Curari not an anæsthetic.

Considers curari is not an anæsthetic; a dog whose respiratory organs are paralysed by it will try to wag his tail, and will follow you round the room with his eyes, 1731–5; quotes Claude Bernard, that animals under its influence "feel pain but cannot manifest it," 1735.

Vivisection for teaching purposes should be forbidden,

Admits the value of instruction conveyed by ocular demonstrations, 1731; and that anatomical demonstration is a necessity, but vivisection for physiological demonstration should be forbidden, the suffering entailed being so great and a verbal description being sufficient, 1701; most medical men have learned physiology, as anybody might, solely from description, 1809.

except under anæsthetics.

Vivisectors are not exempt from the law of habit; they are hardened both morally and physically; instances a case of the different way in which suffering is regarded by those experienced and those inexperienced in vivisection, 4908–9; would certainly not allow his son to see demonstrative experiments, except under anæsthetics; sees no objection to them then, provided insensibility be maintained to the end, 4910.

Legislation is necessary

Regulation of Vivisection.—Considers there is a decided necessity for State intervention which would not, he believes, interfere with research, but rather the reverse, 1718. Personally would like to see vivisection forbidden; it seems a necessary evil, however, and therefore witness advocates its legal restriction, 1737.

Licences.

Is of opinion that no person should be allowed to experiment unless licensed, except analysts in cases of poisoning, 1729; and that the number of animals allowed yearly to each experimenter should be limited; believes this would prevent experiments by unlicensed assistants, 1729; at present many cases have occurred in which 10 to 30 animals have been experimented upon with the same result, 1729.

All experiments on living animals not having a definite object should be forbidden; the advantage resulting from the prohibition of useless experiments, such as the injection of boiling water into the stomach or sand into the veins of dogs, 1801, would counterbalance the possible prevention of some useful ones, 1797–9.

Returns showing past and proposed experiments should be required.

A return should be required each year of the experiments intended to be performed, and another showing the results of those that had been performed; the first would prevent unnecessary and the second careless experimentation, 1729; is sure that scientific research could be conducted with greater economy of pain than it is, 1735–6.

Uses of these returns.

Believes that the list of proposed experiments would enable a competent inspector to say whether a given experiment had been performed before; if so, repetition should not be allowed unless there was reason to suppose it had been defective, 1796; the inspector in allowing the repetition of an experiment would be able to refer the experimenter to the previous experiment, and the cause of failure might thus be avoided, 1800; the second return should state whether anæsthetics were used, and how long the animal was kept alive, 1729, as proper supervision is required, not only as regards animals to be, but on those which have been, experimented on, 4888, *page* 243.

Surgical experiments.

Does not think any control is requisite with regard to vivisection for surgical experiment, except to provide that it should be done by clinical surgeons only; surgery is so perfect that very few experiments can be needed, 1721.

Suggests a board of control.

Inclines to the institution of a board of control for scientific experimentation on animals, comprising physiologists, physicians, surgeons, pathologists, and possibly a chemist, 1771–4.

LAWSON CAPE, M.D.

Formerly in practice in London, 1811–2; and for 10 years lecturer at St. Thomas's Hospital, 1820.

Has been out of practice 8 or 10 years, and has not attended to the subject since, 1833–4; and has no practical knowledge of the schools since the year 1847, 1837–9. Is not a member of the Society for the Abolition of Vivisection, but to a certain extent agrees with them, 1813–6; although not recommending the total abolition of experiments on live animals, 1832, deprecates their extension, 1819. Believes that the present surgeons and physicians are efficient practitioners, and have never had anything to do with vivisection, 1817–8; and does not see any educational benefit likely to result from its introduction in the schools, 1821, 1836.

Experiment for demonstration is not necessary.

Never saw vivisection in Great Britain; was more than two years in Paris; and then it was not recognized although done secretly, 1820, 1835.

Approves of the system of licence by the Secretary of State if accompanied by proper regulations, 1826–7, 1831. Considers that vivisection should not be allowed on the dog and other highly organised creatures, 1822–5, 1828; but does not think worms for fishing, and the lower animals feel much pain, 1829.

GEORGE MACILWAIN, F.R.C.S.

In practice in London until about four years since, 1840–1.

In early life was acquainted with Mr. Abernethy, 1845–6; and did a little vivisection himself, 1853; and having studied the writings of Hunter and Lord Bacon, witness became much impressed with the fallacies commonly accepted as facts by the medical world, 1845–6. On several occasions has offered to advocate his views in public, but has not had an opportunity afforded him, 1845–6, 1854, 1858. Puts in the first volume of a book printed by witness advocating the inductive method in medical science, 1845–46, and pointing out the uselessness of vivisection, 1847–9; this work has been re-published in America, 1859. Has communicated with the Society for the Prevention of Cruelty to Animals with regard to vivisection, 1842–4.

Advocates reasoning by induction.

Observations on the valves of the veins, not vivisection, led to Harvey's great discovery.

Denies that the circulation of the blood was discovered by vivisection. Fabricius discovered the valves in the vein, Harvey saw that they only allowed the blood to flow in one direction, and hence discovered that it circulated, 1845–6; besides, if in the dead body you inject a fluid into the arteries, it returns by the veins, 1845–6.

Aneurism.

Refers to Hunter's experiments with regard to suppuration and aneurism, describes the nature of the latter; the old treatment was to tie the artery above and below the aneurism, but this operation was often fatal, because, as Hunter said, the artery when tied was in a diseased condition. Points out that Hunter's experiments on animals to prove this were fallacious, as animals cannot have aneurism induced in them, 1845–6.

Fallacies in arguments, based on experiments on animals.

Refers to Mr. Travers' surgical experiments on the intestines of dogs, points out the error of the conclusions he deduces therefrom, and the want of analogy between the operation on the dog, and that for strangulated hernia in the human subject to which Mr. Travers' deductions were applied; witness believes that hundreds of lives have been saved by his having succeeding in getting the treatment recommended by that experimenter given up 1850–1. Quotes Mr. Stanley of St. Bartholomew's in support of this, 1852.

Loss of human lives therefrom.

Vivisection a fallacious method of investigation.

Considers it is demonstrable that vivisection has been not only useless but misleading, 1859; and that experiments on animals have led to serious practical errors, 1850, and have not conduced to the cure of human infirmities, 1847; prefers, however, to rest his condemnation of vivisection solely on the ground that it is demonstrably a fallacious method of investigation, 1855–8.

Rev. SAMUEL HAUGHTON, D.C.L., M.D., F.R.S.

Fellow of Trinity College, of the College of Physicians, and of the College of Surgeons of Ireland, Registrar of the School of Physic, and Honorary Secretary of the Royal Zoological Society of Ireland, 1860–6.

University of Dublin.

Puts in the regulations of the University of Dublin prohibiting vivisection, 1867, *page* 99, which may be taken

as expressing the views of the College of Physicians of Dublin, also, as both bodies are concerned in the management of the School of Physic, 1869. Describes the government, &c., of the School of Physic, 1868. Points out that the University of Dublin not only forbids vivisection itself, but will not recognise the certificates of schools in which it is permitted to the students or used for demonstration, 1867, *page* 99. In these regulations "vivisection" is understood to include all experiments on living animals, excluding the exhibition of animals freshly killed, by means of which, as artificial respiration can be kept up, very many experiments, such as Weber's on the pneumo-gastric nerve of the heart can be performed, 1917–8; these regulations were made when the physiology class was started a year and a half ago, 1898. Experiments on pithed frogs are permitted, but those on animals under anæsthesia are forbidden on account of the difficulty of securing that complete anæsthesia is maintained, 1919–23.

Certificates from schools in which vivisection is practised for demonstration not received.

No experiments for demonstration allowed except on pithed frogs.

Professor Purser conducts the classes in physiology and histology without experiments on live animals, and finds no practical difficulty on that ground, 1870–1, 1927–33, 1971; although as a matter of feeling he would doubtless prefer to be unfettered, 1903–7; the professors of the five extra-academical schools also agree that vivisection is unnecessary in lecturing, 1900–2.

Results arrived at by Experiment.—Considers that the value of these experiments is often exaggerated; instances the extensive series of experiments by Dr. Hughes Bennett with regard to the action of mercury on dogs' livers, which were of great value as regards sick dogs, but of none as regards sick men, 1875; and physicians therefore rightly adhere to the old practice which experience has justified, 1934–6.

Treatment proper for dogs not therefore good for men.

Biliary fistula.

Voted for the grant for the experiments on biliary fistula, but was disappointed at the smallness of the result, 1954; admits, however, that a negative result is often important, 1955.

As an instance that you cannot argue by analogy from one animal to another, refers to some experiments of his own, with regard to the action of strychnine and nicotine, by which he ascertained that a frog might be speedily killed by a bath containing either poison, but that in a bath containing both poisons in certain proportions a frog would live for weeks. On trying the experiment on dogs, it was found not to have the same effect. The result on frogs having been published. Nicotine was tried as an antidote for strychnia in America and proved, as it has since done in five or six cases, successful, 1875, 1911–2.

A mixture of poisons innocuous to frogs, poisonous to dogs, and an antidote for man.

Results often vitiated by the pain and the anæsthetic.

Points out that in these experiments animals are disturbed by pain, and also by the anæsthetic. The products of the blood and the secretions differ greatly from those of animals in health, 1894–7.

Limits of Experimentation.—The true use of vivisection and pathological experiment is original research, 1867, *page* 99; but so much nonsense is talked with regard to it that if nine tenths of it were stopped no harm would be done, 1960–1.

It should be limited to competent persons in pursuit of definite objects, and should be under regulations, 1946, 1964–6. It would not do to trust the enthusiastic dissector to keep up anæsthesia as long as he ought unless it facilitated the operation, which it does not invariably, 1892–5; and painful experiments should be forbidden if other means were possible, 1967.

Justifiable for definite objects only.

Considers experiments for the advancement of science are justifiable, but only when undertaken with a definite object, 1952–3; and thinks that if an experiment opens up further questions they in turn should be solved under proper supervision, 1956.

Considers that a man may qualify himself to conduct a physiological laboratory without experiment on live animals, just as surgery is learned without experiment on the living body, 1944–5.

Thinks the idea of shutting up the Zoological Gardens because confinement may produce disease in the animals absurd, 1937–8.

Experiments for demonstration.

Objects to experiments for purposes of instruction, whether under anæsthesia or not, 1872, 1876–7, 1963; both because, being performed under abnormal conditions, the results cannot always be depended on, and because they are unnecessary, 1897; is entirely opposed to Dr. Rutherford's extraordinary statement that vivisection is "essential" in teaching, 1943.

Evil effects on some characters.

Half a lifetime's experience has convinced him that the dissecting-room elevates some characters and degrades others, and he believes that accustoming students to the sight of animals under vivisection would produce in many cases young devils rather than men who would advance science, 1888.

The Norwich experiments.

The Norwich experiment was unnecessarily cruel, the absinthe should have been received through the stomach; besides, if the object was to ascertain its effects on men, one of the 2,000 men whose votes are purchasable might doubtless have been induced to get drunk on absinthe for a small consideration, 1874–75. Considers that the majority of the Medical Association would have forbidden such experiments, although the majority of those in the room were in its favour, 1874.

At Oxford the experiments proposed to be made by Professor Magnan were submitted to a committee beforehand, and some were prohibited, 1951.

Memorial to Society for Prevention of Cruelty to Animals.

Reads memorial signed by the most distinguished medical authorities in Dublin and by himself, addressed to the Society for the Prevention of Cruelty to Animals, setting forth the enormous extension of the practice of vivisection, even in England; its probable indefinite development; its liability to abuse; its deadening effect on public feeling, and suggesting that a sub-committee for the restriction of vivisection should be appointed; that prosecutions should be attempted under the existing law, and that if these failed a bill should be drawn up providing for licensing laboratories and persons, forbidding vivisections for demonstration, extending the time during which prosecutions may be commenced from one to three months, &c., 1867; reads a letter from the secretary of the Society for Prevention of Cruelty to Animals asking for assistance in collecting evidence with regard to the practice of vivisection, and reply from himself declining to assist in collecting possible evidence against scientific brethren, 1867.

Physiological Handbook.

Thinks Dr. Sanderson's Handbook valuable, but it unquestionably tends to encourage vivisection by students, and to extend the practice generally, 1939–42.

Often inefficiently administered.

Anæsthetics.—Has no difficulty in keeping an animal under chloroform himself, but from his knowledge of schools and students, believes any provision compelling their use would be laxly carried out, 1924–6; the practice is to use anæsthesia very imperfectly, and when the controlling eye is gone to drop it altogether, 1884.

Believes pithing a frog gives the means of making as many experiments as chloroform would, but some on the nerves cannot be performed with either, 1948–50; it is difficult to give anæsthetics to the lower animals without destroying them, 1947; has tried to produce anæsthesia in lions and tigers without effect, 1947.

Extension of the Cruelty to Animals Act useless.

Regulation of Vivisection.—Is aware that the present law protects certain animals only, 1885; but thinks an extension of it would be entirely insufficient; has been a frequent prosecutor under it, and finds the difficulty of getting evidence results in the acquittal of nine out of ten offenders; in fact, it tends to promote the suffering of animals, 1891; has not, however, come prepared to detail proposals for regulating research, 1962.

Reasons for restriction.

Considers that regulation is required, 1st, because young physiologists make clumsy and unnecessary repetition of foreign experiments; 2nd, because in many necessary experiments the method is often needlessly cruel, instance the Norwich experiments by M. Magnan; 3rd, because they are often undertaken without sufficient forethought and care; and 4th, and specially, because the importance of the results are over-estimated, 1874–5.

Thinks that vivisection for original research requires some such provision as that provided for anatomy by the Anatomy Act of 1834, 1873; experiments under anæsthesia should be equally under control with others, 1882–4.

Suggests similar law to the Anatomy Act.

The Anatomy Act of 1834 is an important guide in the whole question, 1878; and any controlling machinery set up should be similar to that at work with regard to anatomy, 1908–10, 1916, 1968; is inclined to think that if the inspector of anatomy were to act as inspector of physiological experiments in conjunction with a colleague appointed locally by the public it would be found to work well, 1886.

Necessity for the unprofessional world being represented.

Wishes to express his opinion very strongly that control should be partly under the direct influence of public opinion, so that the Legislature might know what was going on, 1969–70; the supervision should not be vested solely in a fellow physiologist, 1957–60.

Thinks experiments made by a medical practitioner to test the propriety of a certain course would be almost necessarily useless, and should be forbidden; if he wanted to enter upon a series of experiments he could obtain a licence, 1913–5; in the case of poison such cases must be purely hypothetical, 1972–4.

Lord Henniker's bill.

Objects to Lord Henniker's bill because it leaves out experiments for testing the effects of drugs; because it proposes to charge a fee for a licence, and thinks it goes too much into detail which should be left to the supervising body, 1880–1.

Licensed persons only should be allowed to experiment.

Dr. Playfair's bill.

Objects entirely to unlicensed assistants at these experiments being allowed; believes such assistants would actually perform the experiments, and that the practice would thus be extended among the students, 1881; thinks the clause in Dr. Playfair's bill allowing experiments to be conducted under the supervision of a licensed experimenter is intended to have the effect of enabling experiments to be handed over like the post-mortems at a hospital to the youngsters, 1887.

Licences.

Objects entirely to the clause in Dr. Playfair's bill empowering the presidents of the Royal Society and of the Colleges of Physicians and Surgeons to issue licences; there would be no control under such a system, 1881.

MR. ALFRED H. GARROD.

Prosector to the Zoological Society of London, 1975–6.

Did not go through a practical physiological course, 2020–1; but now performs experiments for his own instruction, but always under anæsthetics, 1976–8.

Experiments for demonstration not necessary.

Does not object to experiments for demonstration on pithed or decapitated animals, 2004; but has an instinctive aversion from demonstrative experiments before classes in special physiology, 1980, 1992; and objects to such experiments under anæsthetics because the animals can return to the normal state afterwards, 2005, and because an operator in public may often pay less attention to the feelings of the animal than is desirable, 1994; thinks, too, that the text-books for physiological students do away with the necessity for repeating experiments, 1995; and that enough can be taught without vivisection, 1986, 1993.

Believes the phænomena of the circulation of the blood are more easily learnt from description than from ocular demonstration, 2000–3.

Gives reasons.

Admits that in chemistry experiment is necessary for the student, 1996–7; but points out that the important subject of surgery is taught by text-books without experiment, 1998.

Admits that no one could understand the action of the living muscle from seeing the dead muscle only, 1999; but contends that although it is necessary to study its phænomena in original research it is not for demonstration, 1999.

Believes that the extra study required in getting up surgery or physiology from a text-book is quite balanced by the extra study necessary to enable a student to grasp the results of an experiment, 1998.

Surgical experiments.

Vivisectional surgery is objectionable, it used to be practised in France on a large scale, 1990; believes, however, that except in vivisectional demonstration abroad, and possibly at home, there is no abuse of the practice, 1989.

Anæsthetics.—Believes experiments under anæsthesia are painless, 1919; and is of opinion that by varying the anæsthesia according to the animal there is no difficulty in anæsthetising them; instances the giraffe, 2006–9; considers a disposition to move or make a noise a sign to repeat the dose of chloroform, 2023–6; and that perfect quiet during an important skin section is a sign of complete anæsthesia, 2022; has anæsthetised frogs with ether, the effects of which last a considerable time, but not with chloroform, 2028.

Animals at the Zoological Gardens.—More animals die of tubercle than of any other disease, the larger number from conditions which cannot be accounted for, 2010–6; the domestic animals live as long there as elsewhere, 2017; lions and tigers suffer from want of exercise, and live shorter lives, 2018–9.

Regulation of Vivisection.—Experiments for investigation by competent persons should be under no restriction whatever, 1980–1, 1991; experiments by incompetent persons would come under the Cruelty to Animals Act, 1982; admits, on the other hand, that the animals at the Zoological Gardens do not come under the Cruelty to Animals Act, 1983; but contends that two or three experiments will either make a man competent to perform such experiments or induce him to abandon the practice, 1984, 1987; there is no inherent attraction in vivisection, 1988.

FREDERICK WILLIAM PAVY, M.D., F.R.S.

Physician and Lecturer on Physiology at Guy's Hospital during the last 20 years, 2029, 2033.

Has always illustrated his lectures by experiments, 2108; but believes he was the first physiological lecturer in London who did so, 2033; a course of lectures lasts during the six months of the winter session, 2088.

Supply of animals.

For purposes of experiment uses dogs, rabbits, guinea-pigs, and frogs, 2089–90; which are bought in the ordinary way at Leadenhall Market, 2101–4; during the session about 20 dogs and 10 rabbits are used, 2096.

Performs another set of experiments for research, in which, unless there is a special reason, chloroform is always used, 2140–2; has always had in view ultimate benefit to the human race, 2127.

Humane feelings of students.

At the commencement of a course finds it necessary to tell the students that no experiments which will wound their feelings will be introduced, 2062–3.

Vivisection before a class without anæsthetics would not be tolerated in any large body of students at Guy's or elsewhere in this country, or by the profession generally, 2035–6.

All the experiments for demonstration are painless.

For lecture purposes *all* vivisection is performed under some anæsthetic, or in the case of frogs, on a pithed animal, 2033, 2141, 2091–2, 2097; when it is desirable not to weaken the action of the heart puff ball is used, 2097; in every case complete anæsthesia is maintained during the whole experiments, 2098–2100; a great number of the experiments, however, are chemical, 2118; instances an experiment to show the changing character of the blood by detecting a drug administered to the animal in the urine, 2093–5; has nothing to conceal, a member of the Society for the Prevention of Cruelty to Animals might be present at any lecture; everything was explained to the secretary of that society, 2105–7; has not found that these experiments have hardened the hearts of those who were formerly students in physiology, 2197–9.

Popular misapprehensions.

Extent of Practice of Experiments on Live Animals.—Much is done abroad which would not be tolerated here, 2060–1, and which might rightly lead in this country to regulative legislation, 2062; does not consider the practice has considerably increased in England of late, 2037; thinks the popular opinion to the contrary has arisen from "practical physiology" having been added to the list of subjects taught, the instruction given under this head really consisting, however, principally of microscopic work on the dead tissues, 2037; and from experiments, really entailing very small pain, being described in such a way as to appear horrible. Often the facts cannot be recognised from the description, 2106.

Does not believe experiments are conducted by incompetent or reckless persons, 2064–6; or by students in their rooms, 2066–7, 2086–7; or that purposeless experiments are made by scientific men, their time is too valuable, 2117–8; if experiments are being performed by incompetent persons or students they should be legislated against, 2066–8.

Does not know of any private laboratories, 2076–7.

In England the amount of suffering caused is very small.

The amount of suffering caused to animals in this country is almost insignificant, 2031; most experiments are under anæsthetics, and therefore painless, 2031–2; some experiments on the nerves are necessarily painful, but after the result is seen the animal can be at once put out of suffering, 2182–4; does not recollect any experiment on the liver which cannot be performed under anæsthesia, 2182; thinks that in resuscitating an animal poisoned by strychnia, by sustaining artificial respiration, the pain cannot be acute, the animal being under the action of the poison, 2148–9.

Anæsthetics.

Does not think any definite line can be drawn between experiments in which chloroform can be employed and those in which it cannot, 2177–9; has never found the slightest difficulty in keeping an animal under its influence even for three or four hours, 2205; apart from the question of feeling anæsthetics are of great help to the operator, 2099.

Not meant for ordinary students.

Physiological Handbook.—Considers Dr. Sanderson's Handbook meant for professors, not students, 2085; admits that the editors seem to have intended the book, as they themselves say, for beginners, but does not see how beginners could use it, 2150–1; the experiments are unfortunately described, but are not meant to be performed in the schools as would appear at first sight, 2081–2; many of them are not adapted for repetition, 2163–5, 2168–9; they are principally taken from continental sources, 2083, 2156; very likely most of the experiments have been tried by the editors, 2156; persons prosecuting physiology in the hope of benefiting mankind would be justified in undertaking experiments which would not be justifiable for ordinary medical students, 2167.

Frogs in hot water.

Argues that in the experiment of placing a frog in cold water and gradually raising the temperature the animal may not suffer much pain, 2159–62.

Recurrent sensibility.

Does not deny that the recurrent sensibility experiment is suggested in the handbook as one for the physiological lecturer, but it would not be performed at Guy's, 2152–4; it would not be tolerated, 2157, by any body of English students, 2193–6.

Does not think this book has tended to increase the practice of vivisection, 2084, 2155.

Physiology is the foundation of medical treatment,

Necessity for Experiment.—Physiology is at the foundation of the treatment on disease, 2128; the practice of medicine is based on it; before knowledge of diseased action is possible that of healthy action must be acquired, gives instance, 2122–4. Was much helped by experiments on animals in forming the views expressed in his books on dietetics, 2119–20. Has ascertained by experiments that the cause of death in poisoning by strychnine is the cessation of respiration, has performed the experiment demonstrating this under chloroform in lecturing, 2143–6.

and can be advanced and taught by experiment only.

Is convinced that physiology, like chemistry, can only be thoroughly taught by the help of experimental illustration, 2109–13; thinks the knowledge derived from seeing the action of a living heart and the peristaltic action of the intestines of inestimable value to a medical man, 2200–3. Cannot see how such knowledge can be otherwise obtained, 2204; the knowledge gained by hearing, reading, and seeing is likely to be more impressed on the mind than that resulting from hearing and reading only, 2125–6; besides seeing gives knowledge over and beyond that derivable from hearing and reading only, 2129; a medical student not only reads and hears descriptions of diseases but goes through the hospital and sees them, 2130–1. Physiologists untaught by experiments are like physicians without practical experience, 2132.

Considers that the difference between a limb in the living body and one in the dead body is not analogous to that existing between a dead organ and a living one in action; a man may have a thorough knowledge of the parts of a limb without any of the action of the living parts, hence the method of study which may suffice for surgery is not applicable to physiology, 2113–5. Denies that this argument proves that it would be desirable to experiment on human beings, to acquire thorough knowledge, on the ground that there is no essential difference as regards the great functions between man and the lower animals, in fact, setting aside the phenomena of consciousness the great physiological truths can be demonstrated on the dog, 2133–8; the higher functions would, however, be different, 2139.

Dr. Playfair's bill.

Regulation of Vivisection.—Sees no objection to experiments for demonstration being forbidden unless the operative part is performed under chloroform, 2191–2. Thinks the passing of Dr. Playfair's bill would pacify public feeling without interfering with legitimate vivisection, 2074–9; it would not alter a single practice in the London laboratories, nor he believes in those in the country, 2070–3; it would not restrict his own course, 2185.

PHILIP HENRY PYE-SMITH, B.A., M.D.

Assistant physician and lecturer at Guy's Hospital, 2038.

English experimenters are humane and Legislation is needless.

Practice of Vivisection.—Believes that in this country the practice of vivisection is pursued with due regard to humanity, and as far as possible under anæsthetics, 2048–9; has never known these experiments performed by incompetent persons, 2058; and thinks it is needless therefore to forbid such persons to perform them, 2059. Is not aware of the existence of any private laboratories, 2078.

Experiment is essential.

Considers that the study of physiology, in its full sense, is impossible without vivisection, 2040.

Sensational descriptions.

Agrees with Dr. Pavy in thinking that experiments are essential in physiology just as they are in chemistry; that there is no just force in urging that because practice on the living body is not necessary in learning surgery or anatomy therefore it is not as regards physiological education; that experiments are described in very horrible terms which really involve very little pain and that experiments by incompetent persons are unknown and improbable, 2108–21.

Anæsthetics are used in a large majority of cases, 2050–1, 2054; and complete anæsthesia is usually produced unless the anæsthetic be badly administered, 2052, 2054. Has himself never performed or seen a painful experiment on an animal without the use of anæsthetics, 2056. The number of necessarily painful experiments is necessarily small, 2057; and in them the most painful part is performed under anæsthetics, 2055.

The effects of drugs or of operations can only be judged of after recovering from anæsthesia, 2053.

Physiological Handbooks.

Agrees with Dr. Pavy in his opinion of Dr. Burdon-Sanderson's handbook (*see page* ,) 2170. Knows each of the contributors to the handbook, and has full confidence in their humanity and judgment, 2176; and while not agreeing with every line thinks there is no experiment which would not be justifiable in teaching students in pure physiology, 2175.

Legislation is unnecessary.

Regulation of Experimentation.—Is not prepared to deny that the practice of vivisection is abused abroad, 2044–5; but is not aware of any abuse of the practice in this country, and thinks therefore that legislation is unnecesary, 2042–3; thinks that the selection of experiments should be left to the teachers, assuming them to be competent, rather than be limited by law, 2171–2; believes that at present public opinion among physiologists is sufficient to check abuses, 2176. If, however, the practice in this country were assimilating to that of foreign countries his opinion as to the necessity of legislation might be modified, 2044–7.

Dr. Playfair's bill.

Agrees with Dr. Pavy in saying that if Dr. Playfair's bill were to become law it would not render illegal any of the present experiments now performed in the London schools, and he is sure that the gentlemen in charge of the laboratories out of London,—*i.e.*, those at Edinburgh, Manchester, and Cambridge would also say the same, 2075; but although it would not interfere with the leading physiologists thinks it would be an undeserved stigma on them. Sees no difficulty, however, in working it if it be required, 2186–90.

J. BURDON-SANDERSON, M.D., F.R.S.

Professor of Human Physiology at University College, London, Superintendent of the Brown Institution, and author of one part of the "Handbook for the Physiological Laboratory," 2206–7, 2209.

University College.

Necessity for experiment.

Is excluded from practice and restricted to lecturing and original research by the terms of his appointment at University College. Has been lecturer on physiology since 1866, and gives every year one course of 75 lectures, in which no experiments on animals are performed, 2651–4; each lecture lasting an hour, 2660; also goes through a practical course lasting about 11 weeks, of which five are devoted to chemical experiments; two or two and a half weeks to observations on the living tissues of dead animals, and the other two weeks to experiments on insensible live animals, 2654. Besides frogs, five or six rabbits, at the outside, are used, 2655–7; the lecture with the demonstration lasts about half an hour, 2660–1. Considers that the functions of circulation and respiration cannot be perfectly explained without these experiments, 2658–9.

The ordinary students attend both courses, 2714–5. Notice is given on the blackboard of all demonstrations forming part of the course. Experiments under anæsthetics are arranged beforehand and performed before smaller numbers, 2357.

Laboratory students.

Those who attend the physiological laboratory, are in a few instances persons of no profession, who intend to devote themselves to scientific research, 2716–22; the remainder are young medical men who stay on, after passing, to make themselves more efficient. The studies in the laboratories are connected with the bedside practice of the hospital as far as possible, 2723–5.

Animals used.

Occasionally uses cats as well as frogs, dogs, and rabbits, 2386–8. Will put in a statement showing the number of animals experimented on at University College, and the nature of the experiments, 2739–42.

Brown Institution.—Has the management of the Brown Institution under the University of London, 2623. It was founded with the purpose of investigating and of curing the diseases of animals. It has a hospital and a laboratory for research; numerous experiments are now being made there with regard to the nature of diseases, 2625–6. Considers that the knowledge thus gained must eventually tend to the diminution of both

human and animal suffering, 2626–33. The experiments with regard to sheep-pox have had the treatment of small-pox, of which it is a type, in view, 2629–33.

Sheep-pox.

Private experiments.

Vivisection is not a part of the business of the institution, some private experiments, however, are conducted in one of the rooms by Dr. Klein, 2796; others of a *quasi* private nature by Dr. Ferrier have been discontinued, 2797–800.

Supply of animals.

Keeps a small private stock of rats, guinea-pigs, and rabbits at the institution, from which Dr. Klein also takes his supply. These animals do not interfere with the reception of other animals for cure; in point of number, the rats and guinea-pigs are naturally more numerous than the patients, 2801–20. The rats and guinea-pigs are bought in the market; does not know how the cats and dogs are obtained, 2821–4.

Value of foreign research.

Extent of Practice of Vivisection.—Is acquainted with the foreign schools from the professional journals, and considers that nine tenths of our physiological knowledge has been derived from experiments performed in them, 2216–7. Believes there are more great men at work abroad than ever, 2211.

Admits that there are some things done abroad in connection with teaching and even with research which can hardly be justified, 2217; but does not think that the opinions held abroad will make progress here, 2759. Cannot give any information as to the number of animals consumed in Professor Ludwig's or in Claude Bernard's laboratory, 2735, 2743–4.

No abuses in England.

Considers that there is a general feeling of consideration for the lower animals in this country which prevents the infliction of unnecessary pain, 2285–7, and tends to secure the utmost pains being taken to get a definite result, 2227–9; does not believe there is any wanton or cruel experimentation in England, 2688.

During the last 20 years only three papers in the Transactions of the Royal Society, out of 18 or 19 on physiological subjects, have related to painful experiments on animals, 2605–7.

In all large schools there are pathological as well as physiological laboratories, 2727.

Research is becoming methodized.

Investigation, however, now goes on with greater rapidity, hence the comparatively large number of experiments, 2786–9; but it is a mistake to say that the "number of experiments will increase in geometrical progression;" on the contrary, knowledge is becoming methodized, and the number of separate facts on which it rests smaller, 2783–5. Believes that organization will tend to diminish the number of experiments, 2825.

Finds on investigation that not more than about 16 persons (names given) are now engaged in physiological research in Great Britain, 2607–12.

Useless experimentation.

Knows of one or two instances of useless experimentation by incompetent persons, gives details, 2615–7, but thinks such cases are very exceptional, and that the public performance of experiments tends to prevent their being undertaken privately, 2613–4.

Rabies.

Rabies in the dog could not be induced by starving the animal in sight of food and water, the attempt would be unjustifiable, not necessarily from the pain caused, but from the ignorance displayed; does not know of any foreign experiment so painful, 2764–70.

Starvation.

Chossat's experiments on starvation have given valuable results, but should not be repeated unless doubt arises with regard to them, 2774–6.

Needless prolonged suffering.

Thinks that in M. Paul Bert's experiment of dissecting out and irritating for six hours the pneumo-gastric and sciatic nerves of a dog, there was not a due regard to the infliction of the least possible amount of pain, 2778.

Baking and freezing to death.

Points out with regard to Dr. Legg's experiments on cats, that in no case was the temperature above fever heat, 2388. Considers that the animals baked and frozen to death by Delaroche and Berger soon ceased to feel, 2778–9.

Refers to the appointment of a French physiologist at the college for the medical education of women, 2790–5.

Is of opinion that the knowledge of the nature of processes of disease obtainable from dead-house observation may be considered nearly exhausted, it is now necessary to go to the living frame, aided by the microscope and other appliances, 2707–13.

Increased knowledge will result in better treatment.

Results obtained by Experiment on Live Animals.—Contends that the utility of physiological research is manifested not so much by its present direct applicability to disease, as by the certainty that the knowledge derived from it will ultimately be the guide to the best treatment, 2296, both in medicine and surgery, 2704–6. Refers at length to the knowledge obtained with regard to the mechanism of inflammation and of tuberculosis by painful experiments on animals, 2296–9; and thinks if as much were known of the mechanism of cancer and rheumatism, we should be in the way of doing a great deal more good. Instances, as improvements of treatment resulting from the increased knowledge obtained by experiment, the application of cold in certain stages of rheumatic fever; the abandonment of bleeding in cases of fever; the modified use of gelatine for invalids; the use of alcohol in cases of fever, *page* 8.

Tuberculosis.

Refers also to the knowledge of the causes which lead to the secondary diseases following injuries, and of the circumstances favouring the development of tubercular diseases as tending to make them eventually preventable, 2299.

Number of animals used.

In the investigations with regard to tuberculosis, extending over some years, 200 or 300 rabbits may have been used, 2737; refers to another course of research in which about 50 animals were used, 2738.

Limits of Experimentation.—The justification of an experiment depends on the importance of the object compared with the amount of pain caused, which should be considered beforehand, 2750–5; and the pain made as small as possible, 2763.

Pain inflicted for non-scientific purposes.

Coursing.

Refers to the lingering deaths of many of the rabbits snared and trapped, and contends that the amount of suffering caused by these means, and by the gunshot wounds of animals that escape is enormously greater than that of those put to death by physiologists, 2698–703. Would consider killing a hare for experimental purposes in the manner done in coursing unjustifiable, unless for an object of great importance, 2695–7.

Thinks physiological experiments should be discouraged, except in the hands of those who have been trained in a laboratory of research under skilled investigators, whom the students should assist, 2241–2.

Repetition will become less necessary.

Admits that during the last 25 years the introduction of more exact methods has necessitated a great deal of revision of results; but with the present exactitude, future repetition will be less frequent, 2781–2; it is, however, difficult to say when a fact may be considered established, 2223–4. The establishment of new facts should only be attempted by thoroughly competent persons, 2225–6.

No English physiologist would think of repeating very painful experiments which had been verified abroad, 2780. Painful experiments are only necessary in original research, 2289.

Private research.

Individual research is open to the objection that results cannot be so easily verified; it could not be well forbidden in England, and it has often produced valuable results, 2302, 2309.

Increase of practical instruction.

Experimental research has not changed its character, 2672–6, although schools of physiology are more completely organized than formerly, 2212–3; 30 or 40 years ago there was no such thing as a laboratory of practical chemistry for educational purposes in the country, 2677; there was no physical laboratory even 20 years ago, 2678; until lately the professor of physiology at Edinburgh was the only purely physiological office in Great Britain, now there are only three, 2301.

Tendency to centralization.

Believes that the growing refinement of research will necessitate the use of special laboratories and appliances, and the abandonment of private research, 2680–1, 2685; and also make experiment by students improbable, 2682–3. More efficient laboratories are desirable for the sake of producing men of higher scientific attainments, 2732–4.

Medical Education.— Thinks that medical students should be taught the application of chemistry and physics to the explanation of living phenomena, 2302; but as medical practice is at present based on clinical experience, does not desire that they should go through the physiological laboratory unless they wish to become consulting physicians or surgeons, 2728–31.

Thinks physiology should be taught to workers designing to be physiologists, not by lectures, but by association, for experiment and practical work, in schools under competent instructors and attached to public institutions; believes with such an amount of publicity and responsibility abuses would be very unlikely to occur and the verification of results would be secured, 2302–8.

Practical physiology is taught by experiment at Guy's and King's College Hospitals only, 2286–8. The practical nature of modern education is not confined to physiology, it is applied also to chemistry, physics, &c., 2679.

Experiment for demonstration.

Admits that physiologists differ in opinion on small points, but thinks there is a general agreement that painful experiments are not necessary or desirable for demonstration, 2618, 2756–62; thinks anæsthetics should be used whenever possible, 2221–2, always employs them himself, 2230.

Strychnia.

Thinks the action of the most important poisons, such as strychnia for instance, should be shown to students; no one can form a vivid conception of their action without seeing them, 2231–8; such experiments cannot be performed under anæsthetics, 2618; but the pain is of short duration; death by strychnine would be clearly shown in five minutes, 2620–2.

Not for ordinary students.

Some experiments are basis-experiments; others are for demonstration.

Handbook for the Physiological Laboratory.—The book is intended for "beginners" in research, not for students of medicine; always points this out to his class, 2239–40, 2243, 2638; the title indicates that it is for students in laboratories which are under the control of skilled physiologists, 2662–5, and makes no reference to experiments for education, 2639; it was designed to give a new start to physiology, 2214–5; and was intended as a statement of the objective basis of physiological doctrine, as well as a guide in experimentation, 2666–8. Some of the experiments are meant to be shown to students, 2640–1; others are intended, not for repetition, 2280, but as an exposition of the means by which admitted facts have been arrived at, 2642, and form the basis from which to proceed in further research, 2648–50; believes that the publication of the details of such experiments prevents their being repeated, 2643–7.

Admits that there clearly ought to have been a distinction drawn between these basis-experiments, and those intended for demonstrations and as patterns, 2372.

Regrets the mistakes made in the publication which have led to such an erroneous feeling on the part of the public, and would be glad to see them rectified, 2367–70; and his real views placed more clearly before the public than they are in the book, 2284.

Cannot say how many copies of the handbook have been sold; it is in the publisher's hands, 2364–6; the only edition is dated 1873, 2634–5.

Cannot say how many of the experiments are painful and how many not, 2748–9; very few of them can be conducted without exceptional instrumental aid, 2684; many of them have not been verified by himself, 2746–7.

Thinks the experiment in which two needles are inserted in a frog, one entering the cranium through the occipital bone and the other the spinal canal, and the heart then exposed, would give extremely little pain even if the animal were not curarised, 2253–4; bearing in mind that it was an experiment of research with a definite object, it would be absurd to use any other anæsthetic, 2256–8.

In experiment 74, in which the frog is directed to be "slightly curarised or rendered motionless by the section of the medulla," the brain of the frog should have been directed to have been destroyed, 2259–61.

Tying the jugular vein of a rabbit.

Refers to a painful experiment in which the jugular vein of a rabbit under curari is ligatured and severed, and admits that an anæsthetic also should be used, although not mentioned, 2262–5; the rabbit, however, showed no signs of subsequent pain, and would have lived if it had not been killed to observe the effects; would not dream of repeating such an experiment, 2371–6.

The use of anæsthetics is assumed.

Points out that the book was intended for physiological readers only, and that as it is generally agreed among physiologists that anæsthetics should be used whenever possible, their use was assumed; admits, however, that it would have been better to have been more explicit, 2265–73.

Vagus nerves

The division of the vagus nerves (Ex. 101) produces inflammation of the lungs, 2771; entails pain similar to that felt in that disease, and results in the death of the animal; the experiment is not intended for repetition, but as, irrespective of the handbook, some doubt has been thrown upon the alleged results, it may be necessary to repeat it, 2274–82.

Arterial pressure.

The experiment (*page* 176) in which a glass tube is introduced vertically into the aorta of a frog, is of very short duration and attended with very little suffering, as the whole of the blood is speedily driven into the tube and the animal dies, 2377–9.

Asphyxia.

The subject of asphyxia is now well understood; painful experiments to prove known facts should not be made, 2283.

Effects of curari on frogs

In ordinary doses curari does not affect the sensory nerves of any animal, but in large doses it appears to affect the central organ of the frog—gives reasons, 2380–1; refers to an experiment described in the handbook in which the heart of a frog is laid bare, and observes that the action of curari is not yet thoroughly understood; it is not treated in the handbook as an anæsthetic, as far as regards the higher animals, but is inclined to think that very little pain is felt by a frog under its influence, 2244–53, 2636–7.

Frogs.

Sensibility of Animals.—Is clearly of opinion that the sensibility of the frog is very small; admits that the evidence on the point is negative, but refers to the fact that a frog deprived of its hemispheres moves in the same way when touched as a sound frog; and points out that the smallness of its sensory system is an argument in favour of its low sensibility, 2379, 2382–5; the difficulty of getting frogs under chloroform is so great that it would be absurd to attempt it in many cases, 2378.

Deprecates legislation.

Regulation of Vivisection.—Does not consider legislation necessary, 2687, 2689; and although admitting it would not interfere with men of eminence if it ensured proper organization, fears it would stifle private investigation, 2826–7; original research by incompetent persons and some continental practices might, however, not improperly be forbidden, 2218–9, 2290–2.

Thinks that the head of a school of physiology might be held responsible for doing his best to prevent abuses, but he should not be held personally answerable for the acts of others, 2389–92. So far from it being necessary to restrain medical students from experimentation, the difficulty is to get them to work for themselves, 2669–71.

Inspection impracticable.

Does not object to publicity, 2353–6, 2358; but considers that inspection of laboratories would be impracticable, and keeping a list of the animals useless, and as regards frogs and such animals impossible, 2352; believes that the laboratories attached to public institutions would not be more beneficially controlled by law than they now are by the various managing bodies, 2311.

Dr. Playfair's bill.

Has had some conferences with Dr. Playfair, and assents to the general scope of his bill, 2220, 2293–5, 2312–3, 2686; but objects to some of the phrases as being of doubtful interpretation, and as likely to narrow the extent of investigation more than the advocates of the bill wish, 2314–6; does not object to the proposed licences, 2352.

Making a law on the subject would be [illegible]

Considers that it would be a slur on medical men to legislate against their inflicting pain on animals for scientific ends, whilst the sportsman is not controlled, 2690–3.

but would not interfere with science.

Believes that a law forbidding experiments for demonstration except under anæsthesia, but granting licences to competent persons to perform other experiments under reasonable restrictions, would not interfere with physiological science in England, 2619.

MICHAEL FOSTER, M.D., F.R.S.

Prælector of Physiology at Trinity College, Cambridge, and author of one part of the "Handbook for the Physiological Laboratory," 2208, 2210.

Concurs in the main with the evidence given by Dr. Sanderson, but would not go quite so far in the matter of legislation, 2317–9.

In England.

Extent of Practice of Experiment.—Twenty years ago there was very little physiology carried on in England; but from 1810 to 1820 there was probably as much done in England as abroad; since then there has been less, 2413.

Abroad.

Believes that research is principally confined in Germany to public laboratories, and in France to those of Paris and Lyons, 2399–400. Has no personal knowledge on the point, 2401–4; but considers that physiologists abroad are not so considerate towards animals as English physiologists, 2410.

No abuses in English schools.

Does not believe abuses exist in English schools, 2416; or in English physiological inquiry, thinks that the humanity of those concerned may be safely trusted to prevent them, 2320–1, and minimize pain, 2346–7; points out that the number of necessarily painful experiments is very small, and that generally the pain can be kept within small limits, 2343–5.

Extirpation of the kidneys.

Does not consider the extirpation of the kidneys, as described by Béclard, a useless experiment; thinks it distinctly likely to produce scientific results, 2406.

Effects of changes of temperature.

Declines to give an opinion with regard to the experiments referred to for ascertaining what temperature animals can endure, without knowing particulars, 2408–9. It is very difficult to judge whether an experiment is needless; there have, however, been such, 2405.

Objects of the book.

Handbook for the Physiological Laboratory.—The object of the book is to advance physiological science by experimental and other methods, 2414; does not believe it will tend to introduce continental methods; English good feeling will exist whether the investigators be few or many, 2411, 2415.

Use of anæsthetics assumed.

It being intended that these experiments should be performed under competent supervision, he was the less particular about inserting directions with regard to anæsthetics in each experiment, 2332; would be more careful about anæsthetics if he had to write the book again, and would state to others the additional precautions he himself takes to prevent pain, 2333–9.

The recurrent sensibility experiment was inserted to complete the section; points out that the language shows that it is on a different footing from the rest, and is not meant for demonstration, 2420; has never performed it himself, 2419.

The frog in hot water.

In putting the frog into water and gradually raising the temperature,—gives details of experiment,—directly the frog becomes uneasy, the object is attained, and the animal should be taken out, although the description does not specify this, 2421–4.

Curari.

Does not agree with Claude Bernard and the continental physiologists with regard to the action of curari; thinks there is tolerably conclusive evidence that it destroys the central nervous system; and, therefore, consciousness, gives particulars, 3123–6; at the time the handbook was written the action of curari on the central nervous system was more doubtful, 2327–9, 2331.

Uses morphia with it.

"Strong" frogs were selected in the experiment with curari, in order to better manifest the poison, 2330; witness has been accustomed to inject morphia with curari to prevent pain, 2332.

In experiments for demonstration witness performs all painful experiments, or parts of experiments, under anæsthetics, and thinks this course should always be pursued, 2338–42, 2417–8.

Legislation unnecessary.

Has no objection to legislation, but thinks it unnecessary, 2321, 2348; both as regards public and private experiments, 2393–4; and believes that the good feeling of Englishmen will prevent the necessity of legislation in years to come, 2415.

Licences.

Although physiologists might be able to indicate the *line* of their research, it would make investigation impossible if they were compelled to obtain special licences for each set of experiments; thinks, therefore, that licences should be general, not special, 2395–6.

Private experimentation.

Any legislation preventing private experimentation would be undesirable, although private experiments are necessarily growing fewer, from the growing necessity of having elaborate appliances, 2348–9; admits, however, that they should be restricted in the same way as those made in public, 2350–1.

JOHN ANTONY, M.D.

Was a pupil of Sir Charles Bell while at the Middlesex Hospital, and was selected to prepare dissections for his lectures, 2425–7, but never saw any of his experiments on live animals, 2428–32. Is now constantly occupied in dissecting with the microscope, 2490–1.

Animals vivisected and thrown on the floor to die.

Studied physiology at Paris in 1838; gives particulars with regard to the experiments (Appendix III. § 3.), 2435–40; has seen no vivisection "since the awful dose" he then had, 2582. Animals were there treated like so much inorganic matter, 2582; in some cases for instance, portions of their brains were exposed and torn or cut, and the animal thrown on the floor to creep into a corner and die, 2448. Nothing was taught by these demonstrations that could not have been learned from books, 2450; thinks nationality had something to do with the cruelty, 2583; all the English students were very indignant at it, 2437. With all their indifference to animal suffering the discoveries made by Frenchmen, even including Claude Bernarde, 2561–2, are not comparable in value to those of Sir C. Bell, 2457–9.

Private Experimentation.—Knows of some instances of experiments by young medical practitioners in private houses in the country on guinea-pigs, cats, and dogs, made from mere curiosity and without anæsthetics; believes this state of things has existed for years. Has remonstrated with the perpetrators, but cannot give details without breach of faith, 2509–28, 2532, 2592–5. Amongst others. Bischoff's experiments, tying one of the fallopian tubes and leaving the other in the guinea-pig, and cutting out the lungs of a frog without killing it, and inducing inflammatory action therein were performed; admits that Bischoff's experiment is less painful than "spaying" a sow, but considers the other experiment cruel, the web of the foot might have been used instead; the frogs might have been pithed but they were not; has seen none of those experiments himself, 2529–49; thinks legislation declaring such experiments illegal would prevent them, 2601.

Relative sensibility of animals.

Sensibility of Animals to Pain.—Believes that the capability of feeling pain and the amount of intelligence increase and diminish together, 2597–8; gives instances, 2603–4, both diminish as the order of animal becomes lower; refers to the indifference with which some animals part with their limbs, and gives various reasons and examples, and points out that the nervous systems of the lower animals consist mainly of the nerves of organic life, 2459–66, 2476–7.

Increased suffering consequent on civilization.

In the human being, in addition to a certain amount of inevitable pain, 2557, in cases of injury, a large amount of suffering arises from the hyper-æsthesia of civilization, 2550, and possibly from a delicacy of organization, resulting from successive generations of civilized life, 2553.

Has himself seen, for instance, savage women get up and carry burdens within an hour after parturition, while civilized women suffer many hours, 2552, and a similar difference exists between a racehorse and a carthorse, or an Italian greyhound and a bulldog, 2596–7; animals do not suffer from constitutional irritation, nor generally speaking from the pain of anticipation, 2554–6, although dogs appear to do so to some extent, 2602.

Signs of pain.

Crying, struggling, &c., often occur when there cannot possibly be any pain, 2558–9; considers that in the case stated with regard to the lobster cut up by a lady physiologist was no evidence to show that it felt pain, 2560.

Dogs and cats are principally used because they are high up in the list of mammals and are easily obtained; does not think they feel pain as acutely as men. Sir C. Bell's chief experiments were on a donkey, 2478–81; frogs are largely used by physiologists because their nervous system is relatively large; but it does not therefore follow that they feel pain acutely any more than it follows that a dead horse feels pain, because a recently killed and even decapitated horse will kick with great force if he be at once skinned, 2483–4; excluding intelligent animals from vivisection would exclude those most sensible to pain, but it would also exclude those most like man, 2599–600, and such likeness is often essential in experimenting, 2482.

Considers that observations of the vital processes in the lower animals are advantageous to human beings, 2443–4; and that the functions of individual nerves could only have been ascertained by vivisection; gives examples, 2445–6.

Licences.

Regulation of Vivisection.—Thinks that licences should be granted by the Secretary of State or some public officer but to those who have shown great aptitude in physiology or cognate sciences, 2444, 2472, and for some fixed period only, 2507, and also for some fixed place subject to inspection, 2500; is not prepared to go into details, but thinks there would be no difficulty in restricting the licensee in such a way as to stop cruelty, 2449, 2474–5, and believes that any one who ought to receive a licence would, in the course of acquiring the requisite amount of knowledge, become recognized as a fit person, 2470–3; believes that heads of schools would for the sake of the general good submit to receive periodical licences, 2558.

Objections to public laboratories.

A laboratory, whether at the licensee's residence or in a specially appointed place, should be open to the experimenter and the inspecting officer only, 2491, 2567–70; gives details on the subject, 2501–4, and thinks private workrooms are preferable to public laboratories, because discoveries are made in private, and discoverers are often jealous, 2447, although there are many exceptions, 2563–7.

Inspection of laboratories.

Thinks inspection by a specially appointed qualified official, generally approved of by the profession, would be preferable to any system of experiment before the public, 2444, 2454–5, the inspector to see that certain specified regulations were carried out, 2453; thinks the inspecting official should be a member of the medical profession, but not a skilled physiologist, and that such a man would have little difficulty in saying whether an undue amount of suffering were caused, 2496–9; admits that if an inspector should be of opinion that an eminent physiologist were cruel, professional opinion would probably be on the side of the physiologist, but still inspection would be effective in stopping the licences of cruel young men who might have obtained them, 2505–7.

Are unnecessary.

Experiments for Educational Purposes.—Does not consider experiments on live animals at all necessary for demonstration in medical education; the phenomena produced by strychnine, for instance, are so peculiar that they could not be mistaken even if never seen before; gives other reasons, 2450–2, 2485–93; admits that the fact of some few persons going to operating theatres from morbid curiosity is no argument against public operations, 2571–3; but thinks the dissecting room is a fearful trial to the young student, although getting hardened to it does not render them less tenderhearted, 2574–7: contends that frequently seeing experiments on live animals blunts men's feelings with regard to such experiments, although not rendering them less tender generally, 2578–81; considers that public demonstrations by vivisection are useless and demoralising, and should be forbidden, 2444, even under anæsthetics, 2450.

Demoralising effects of experiments for demonstration.

Sir C. Bell.

Miscellaneous.—Bell was said to be most kind and feeling, and to have limited pain as much as possible, 2430–4; he was very proud of his experiments, and insisted on them as evidence of his great discoveries; cannot reconcile any assertion to the contrary with his own observation, 2584–9; believes Sir C. Bell's remarks with regard to the wanton experiments made in his day referred to Majendie's, 2590.

Believes that vivisection is extending in England, 2441–2.

Anæsthetics.

The majority of experiments at present can be performed either wholly or partly under anæsthesia, 2456–8.

Does not deny that physiologists make wanton experiments, but believes that they are seeking good objects, 2591.

WILLIAM RUTHERFORD, M.D.

Professor of Physiology at the University of Edinburgh, and formerly at King's College, London, 2828–9, 2901.

Visited Berlin 11 years ago, and Leipzig 5 years ago, to gain physiological and medical knowledge, not then obtainable in England, 2968–75; went through a special private course of experimental physiology at Berlin, 2867, 2987.

Medical School at Edinburgh.—The medical school at Edinburgh is the largest in the kingdom, 2830; physiology is taught by a course of lectures, and by a practical course embracing microscopic and chemical studies, and a few experiments on living animals, 2831; the larger number of experiments are on the dead tissues, the chemistry of digestion, &c., 2869–70.

Expenditure of animals.

For the systematic course uses two dogs, three rabbits, and a considerable number of frogs; in the practical course not more than six rabbits and a considerable number of frogs, 2990–2, 3838; for research used last year about 40 dogs and 8 rabbits, no cats or guinea-pigs; the number of dogs is exceptional; in the whole five years when in London did not use more than 10 dogs, 2993–5.

In both courses the dogs and rabbits are put under chloroform, chloral, or opium, and the frogs, generally, not always, pithed, 2832–6, 3839; the frogs not pithed are used to show the action of poisons, 3837.

Number of painful experiments.

Experiments for original research are performed either by witness or his assistant, or by some one under their immediate superintendence, 2840; on the average about half the experiments are painful, 2841–6.

Vivisection by students.

Has never heard of private vivisection by students, 2855; and during his 10 years' experience has never known a student desirous of undertaking an original research; the only experiment students perform at Edinburgh is that of demonstrating the circulation of the blood under the microscope, 2997.

The new laboratory.

The new buildings at Edinburgh are designed to give greater convenience for experiment, but certainly not for any increase in the present amount of vivisection, 2890, 2892–3.

Vivisection has increased in England with the introduction of practical instruction in physiology and with the increase in original research, 2848.

Germany.

Experimentation Abroad.—Has no personal knowledge of inhumanity abroad, 2754; both at Leipzig and at Berlin vivisection was conducted most humanely, 2914, and the instruction thus conveyed ample, 2915. Did not see any reckless vivisection abroad, 2989; but thinks the Germans are somewhat less careful as regards the repetition of painful experiments than the English, 2983–6.

Noticed that in some parts of the continent there was less delicacy in the treatment of human patients than in England, 2851, 3001–4. Considers the tone of English feeling with regard to the infliction of pain higher than it is abroad, 2850–4.

Cannot give any information as to the number of animals consumed in laboratories abroad, 2998–3000.

Action of rhubarb on the liver.

Experiments for ascertaining the Action of Drugs.—Considers the experiment on the biliary secretion of the dog referred to as very important as demonstrating the effect of rhubarb and colchicum in stimulating the liver; in this experiment the animal was placed under curari, and a tube inserted in the bile duct to receive the secretion, respiration being maintained artificially, 2002–8, 2959, 2961, 2964; before these experiments the action of rhubarb was involved in doubt, 2955–6. Considers that the action of these drugs on the dog is a guide as to its probable action on man, 2959, 2962, 2965–7; points out the advantage of this knowledge in their treatment for a sluggish state of liver and for gall stones, 2956, 2958–9, 2963; and contends that the knowledge gained by such experiments is calculated to prolong human life and diminish suffering, 2960. The operation took about half an hour, and the dog was kept eight hours alive subsequently; the wound would be opened two or three times to inject some substance into the bowels, and then reclosed; does not consider the experiment involved great pain, 2917–31; chloroform or opium in any form would have retarded the action of the liver, and therefore could not be used, 2912–3; about 30 experiments were previously performed on the same subject under chloroform, 2932; such an experiment should not be repeated for teaching purposes, 2943.

Uses of the knowledge of its effects.

Calomel.

Whether the action of calomel on a man is similar to its action on a dog is a moot point, 3012–4.

Podophyline.

Was himself satisfied with the results of six experiments when investigating the properties of podophyline, 2945–6; the first experiment on a rabbit giving no result, 3020; is aware that it might be urged that the use of curari interfered with the result, but believes that the reasons against its doing so are valid, 3021; admits that another set of experiments resulted in different conclusions being arrived at, and that, assuming a doubt to exist, repetition might be desirable, 3016–7.

Repetition of Experiments for Verification of Results.—Experiments should be repeated if there be any doubt as to the result, 2847; but the number of times an experiment should be repeated is a difficult question, and can only be left to the discretion of the person licensed, 2945–7; a course which would, however, work very differently with different individuals, 2948.

Painful experiments are necessary for the discovery and verification of facts, 2856–7; but only highly qualified men are competent to undertake them, 2863–6.

Unnecessary in the higher animals.

Experiments for Demonstration.—Repetition of experiments is essential in teaching, and although painful experiments on the dog or higher animals are unnecessary, 2940–2, they are necessary on frogs in order to show the action of poisons, 2858–9, 2862.

Strychnine.

It is desirable that students should see the action of strychnine, which can be shown in five minutes on a frog, 2860–1; in order to recognise its effects on the human subject, and to understand tetanus, 2976–9; entirely differs from those who think that written descriptions of the action of strychnine, &c. are as good as ocular demonstration, considers, on the contrary, the latter essential, 2980–2.

It is even more essential that some of the experiments demonstrating the mechanism of the body should be shown, 2981.

Students.

Believes that students would not tolerate experiments unless anæsthesia is produced and maintained; gives instances, 2898–900.

Points out that under an organized course of practical instruction the number of experiments is minimized, 2867–8; as one animal is sufficient for the whole class, 2891, 2894–5.

Bernard's opinion.

Curari.—Bernard believed that curari only affected the motor nerves, recent experiments, however, seem to indicate that it affects the sensory system also, 2939; and is to some extent an anæsthetic, 2909–10, 2934.

Is not aware of any facts tending to show that curari affects differently different classes of animals, 2911; there are accounts of the death of human beings poisoned by curari, but cannot give details, 2937–9.

Admits that curari might be given to prevent manifestation of pain and expressions of disapproval by the students, but thinks that an anæsthetic, about which there is no doubt, should always be used in experiments for demonstration, 2933–5; in some cases chloroform and curari might be used conjointly, 2935*a*–6; opium, chloral, and chloroform all destroy pain if given in sufficient doses, 3010–1.

Sensibility of animals.

Infers from observations—gives details—that the sensibility of animals differs greatly, and that of frogs is much below that of higher animals, 2871–2.

Legislation is unnecessary.

Regulation of Experimentation.—Believes there are no such abuses as are popularly reported to exist, and thinks legislation unnecessary, 2953; and that the present popular feeling is based on ignorance of what is really done, 2849; but believes that legislation, not interfering with physiological education or with research by competent men, would not be objected to by physiologists, 2916.

Vivisection of the domestic animals.

Forbidding the vivisection of the domestic animals would be a great blow to medical and physiological education; many experiments could not be shown on frogs, the experiments relating to the pressure of the blood for instance, 3005–9; thinks both dogs and rabbits are necessary as well as frogs, 3018–9.

Licences inspection.

If legislation is undertaken, thinks granting general licences to competent persons the best course to adopt, 2882. Would not object to the Inspectors of Anatomy being empowered to inspect these experiments, 2951–2, and report to the licensing body what the experimenters were doing, 2874, 2883; does not consider a lay element necessary to the licensing or inspecting bodies, medical men are as humane as others, 2953.

Record of experiments.

There would be no hardship in requiring a record of all experiments to be kept; most experiments are already recorded at Edinburgh, 2884–7, 2949–50; such a record would tend to stop the proceedings of incompetent persons, 2888.

Dr. Playfair's bill.

Objects to Dr. Playfair's bill because it forbids all experiments for demonstration under pain of fine and imprisonment, 2877–9; even if those under anæsthesia were allowed it would still be objectionable not to be able to show the action of poisons on frogs, 2881; except in this respect the bill would not interfere with things at Edinburgh, 2882; but passed as it stands it would drive students abroad, 2880; considers that Dr. Playfair's bill is complex and indefinite, and prefers Lord Henniker's, 2882; which would have prevented experimentation by unfit persons without interfering with competent men, except as to the licensing, 2875–6. Objects to the power of granting licences being vested in a Secretary of State; thinks placing it in the hands of the councils of the Royal Society and of the Colleges of Physicians and Surgeons preferable, 2873–4.

Lord Henniker's bill.

WILLIAM TURNER, M.B.

Professor of Anatomy at the University of Edinburgh, 3022; one of the editors of the Journal of Anatomy and Physiology, 3099.

Has been lecturer on anatomy at Edinburgh for 21 years, and has the largest class in Great Britain, the number of students in it being upwards of 450, 3069–71; from one third to one half of whom also attend the lectures on physiology, 3072–3; there are no experiments on live animals in the anatomy class, 3023.

Practical demonstration necessary.

Necessity for Experiments on Live Animals.—Points out that although the structure of the body may be both verbally and graphically described, dissection is essential, 3092–5, and that there is a general agreement among scientific men that similar practical teaching is absolutely necessary as regards physiology, 3097–8; most recent discoveries have been made in Germany, where the best practical instruction is given, 3100–1.

Humanity of physiologists.

Resorts to experiments on living animals himself only when it is of paramount importance, believes other experimenters are equally careful to avoid giving pain, 3079–80; refers to the reluctance with which Sir Chas. Bell and Dr. Jones performed experiments, 3025, 3079; and believes English experimenters generally are humane, 3139–40.

Objections were made to investigation by vivisection in Harvey's time, 3025, 3089.

Extent of practice of experiment.

There is more vivisection now than formerly, because teaching is more thorough and practical, and there are more workers in the field, 3090–1; but although practical physiology has only been taught in the schools of late years, many eminent men experimented for themselves in their early days, gives instances, 3141–5.

Functions of the spinal nerves.

Results arrived at by Experiment.—The supposition that the functions of the anterior and posterior roots of the spinal nerves were respectively of a motor and sensory character, although originally suggested by an anatomist, Mr. Alexander Walker, of Edinburgh, was

Turner, William, M.B.

definitely proved by Sir Chas. Bell's experiments, and the respective attributes of each ascertained by his and Majendie's experiments, 3025.

mproved method of igature.

Refers to the improved curative methods which have been resulted from vivisectional observation, instances the knowledge derived from Dr. Jones' experiments on horses and dogs as to the best form of ligature and the best means of avoiding secondary hæmorrhage and of obliterating arteries, quotes various authorities on the point, 3025–7; refers to the value of Dr. Hunter's experiments with regard to aneurism, 3027; also to the experiments by Drs. Hope and Williams with regard to the sounds of the heart by which certain diseases can now be recognized; refers at length to the discovery of vaccination by Jenner, and the important discoveries which have followed Galvani's investigation on frogs with regard to animal electricity; all instances of the valuable results flowing from experiments on animals, 3027–8.

Aneurism.

Sounds of he heart.

Functions of the periosteum.

Asphyxia.

Diabetes.

The reproductive functions of the periosteum and method of the repair of bone have in great part been arrived at by experiment, 3125; the whole subject of asphyxia has been investigated by experiment on the lower animals, and results obtained which could not otherwise have been arrived at, particulars given, 3121–4; that diabetes is not a disease of the kidneys, was ascertained by experiment, 3126.

Increased knowledge of the various vital functions, &c.

Circulation of the blood.

Lymphatic system.

Our present knowledge of digestion, of the secretions, of the action of the nerves on the glands, of the action of the vaso-motor and of the inhibitory and other nerves, of the functions of the kidneys and reproductive organs could only have been ascertained by vivisection, 3081–8; with regard to the discoveries which have followed from vivisection, refers to the discovery of the circulation of the blood, &c., observes that although the discovery was led up to by the structure of the different parts of the circulating system being made known by anatomists, the functions of the heart and arteries were discovered by vivisection, by Harvey, and that of the capillaries by Malpighi, 3025. The discovery of the lymphatic system by Asellius in 1622, and of its course and objects by Pecquet in 1649, were results achieved by vivisection, and the matter is being further pursued by Dr. Sanderson and Dr. Klein, 3025.

Considers that the foregoing results, like many others not mentioned, could only have been obtained by means of vivisection, 3057–64.

Most experiments can be rendered painless.

Use of Anæsthetics.—A large proportion of experiments, including those for research, can now be performed either wholly or in part on animals deprived of sensation by anæsthetics or otherwise, 3029, 3034–6; nearly all the experiments performed by Harvey would be now rendered painless, 3065–8.

Extent and Limits of Experimentation.—Experiments before classes should be very limited in number and might generally be performed under anæsthetics, 3030–3.

Experiments on the nerves

Some experiments, such as those on the nerves, must cause pain, but anyone with ordinary humane feelings will endeavour to minimize it, 3138; it is indeed necessary that experiments should be performed with as little disturbance to the animal as possible, from undue disturbance experiments by incompetent persons may easily give misleading results, 3153–5; painful experiments should be performed by competent persons only, 3039–40; and those demonstrating facts about which there is no doubt should not be repeated, 3037–8.

Vivisection by students.

Here and there a student may occasionally vivisect animals, but it certainly is not a practice among them, 3074–6; knows nothing of the experiments by students referred to by Mr. Mills, 3130–3; as a rule medical practitioners have neither time, nor he believes inclination, to vivisect, 3077–8.

Alleged vivisection of a lobster.

No one knowing anything of elementary physiology would have raised a charge of cruelty on the ground that whilst a lobster was being cut it moved its limbs, 3108–9.

Regulation of Vivisection.—Admits that there is nothing now to prevent any man from hiring a room and performing any experiments he chooses, as long as he does not contravene the Act for the Prevention of Cruelty to Animals, 3115–7.

Difficulty of defining competency.

Fears lest legislation may check the progress of discovery, 3055–6; and is of opinion that no restriction should be placed on competent persons, but does not see how or by whom competency is to be determined, 3127–8; although the professors of the various medical schools would know most, they would not know all, who were competent to undertake such experiments, although there are very few such, 3041–7; is not prepared to admit that experiments on living animals are performed by incompetent persons in this country, 3129.

Turner, William, M.B.

Objects to class legislation against physiologists.

Deprecates legislation with regard to experiments on living animals, on the ground of the difficulty of defining whether an animal is or is not "living," 3048–9; a pithed frog, for instance, although virtually dead might, as regards some of its organs, be considered "living" for some time, 3113–4; also objects on the ground that it would restrict vivisection for scientific ends, but not castration, tail-clipping, ear-notching, and other vivisection on the farm, 3050; admits that it is to the interest of the farmer to injure the animal as little as possible, but points out that it is also of the greatest importance to the physiologist that his experiment should cause as little disturbance as possible to the animal, 3134–7; does not advance the argument as to what is done on farms as tending to justify vivisection by unskilled persons, 3156–8, but as desiring to point out that any legislation restraining scientific experimentation would be class-legislation, and leave untouched similar things when done by unscientific persons, 3102–3.

Legislation might prevent the speedy detection of poison.

Limiting the right to perform experiments to competent persons would prevent a country practitioner, for instance, from testing substances suspected to contain poisons by giving them to animals, although he might not be able to detect them by other means, 3051–4; thinks such questions should be dealt with at once and on the spot, 3118–20.

Inspection of laboratories undesirable.

Deprecates the inspection of physiological laboratories not because it might be looked on as a slur on the profession, but because it might impede scientific discovery, 3146–52; and points out that if placed under the inspection of unscientific persons of the most undoubted respectability and good faith, they would probably make most unfounded charges from ignorance of the subject, 3110–1.

Inspector anatomy.

Considers that there is no analogy between the duties of the inspectors of anatomy and the suggested inspectors of physiological laboratories. The duties of the inspectors of anatomy are simply to see that the subjects for dissection are properly come by and properly buried when done with, 3104–6; an inspector of anatomy could not control the dissections, it would be placing a man without special knowledge to be a judge over experts, 3107; an Act for the physiologist parallel to the Anatomy Act would provide and regulate the supply of live animals for experiment, and see to their removal after being used, but would not interfere with what experiments were performed on them, 3159.

JAMES CRICHTON BROWNE, M.D.

Medical Officer of the West Riding Lunatic Asylum, 3160.

Nitrite of amyl.

Pycrotoxine

Has for 10 years given attention to the subject, 3161; has performed two series of experiments, one not involving destruction of life to ascertain the action of nitrite of amyl, and one with regard to pycrotoxine, the essential constituent of coculus indicus, 3163; 46 animals in all, gives details, were operated on, 3164–6; was successful in discovering an antidote, chloral, for this poison, 3163; no opportunity of testing it on human beings has yet occurred, 3167–8; witness has been denounced for this cruelty, although pycrotoxine is much used for poisoned wheat, in each case the animal dies in convulsions, 3218.

Dr. Ferrier's experiments.

Dr. Ferrier's first series of experiments were performed at the asylum, and comprised the removal of a portion of the top of the animal's skull, and the application of electricity to the convolutions of the brain. They were all under anæsthetics, and the animals were killed directly afterwards, except three, in which the hemispheres had been removed; the longest experiments lasted six or seven hours, 3169–78. In all, 29 animals were experimented on, of which five died from the anæsthetic, 3178; gives details as to publication of the results, and repetition of the experiments, 3179–82. All the experiments were painless, 3221–2; including one in which a partial recovery was permitted to solve a doubt, 3193–4.

Their objects and uses.

The object in view was to throw light on the brain and nerve diseases with which the asylum, which contains upwards of 1,400 patients, is concerned, 3195–6, and the knowledge thus obtained has been found of great service in the diagnosis of brain diseases, 3217.

Excitation of the brain of a cat.

It is true that a cat under experiment "uttered long continued cries, screamed, and gnawed its own legs, &c.," but it was under deep narcosis, and only performed these movements when the brain was excited by the electrodes, 3189; chloroform was given far more freely than would be safe in the case of human beings, 3190–1; entirely denies that it was insufficiently administered, 3184–5.

The statement that another cat was "only partially narcotised" is correct, but deep narcosis is all but death; chloroform acts first on the sensory nerves as in cases where it is inhaled for neuralgia, 3192.

Signs of pain.

Refers in detail to the important and interesting results arrived at, 3213–6; and points out that actions, usually regarded as manifestations of pain, can be produced in the human being under mesmerism without the patient knowing anything of the matter on awakening, 3197–204. Refers to cases in which consciousness has been retained during fits, and in cases of paralysis—gives details,—although no sensation of pain accompanied the signs which usually are manifestations of it, 3217–8.

Paralysis.

Mesmerism.

Is aware that it is argued that both under mesmerism and under chloroform sensations may be felt, but forgotten, but points out that such an argument is not capable of proof, 3206–7; refers to the diverse effects of chloroform on human beings, and contends the phænomena produced are independent of sensation, 3208–12.

Does not know of any other private laboratory or private experimentation, 3182*a*–3.

Thinks inspection of private laboratories would be impracticable, but would be glad of some legal sanction for experiments, 3218–24.

DAVID FERRIER, M.D.

Professor of Forensic Medicine at King's College, and Assistant Physician at King's College Hospital, London, 3225–6.

Has been a lecturer to classes for about five years, 3296; the number in the class is about 40 or 50, 3298.

The practice of experimenting on animals does not harden men's hearts. Some experimenters are among the most humane men witness knows, 3249–50.

Painful experiments should be made under anæsthesia either wholly or in part, except as regards those portions in which it would interfere with the result, 3245–8; always endeavours, himself, to minimize pain, 3230–3.

Experiments for Demonstration.—Practically, all experiments for demonstration can be performed under anæsthesia, 3234–5, 3244; as they are at King's College, 3297, 3353.

Action of poisons.

Thinks it essential that the action of poisons should be shown to students so that they may recognise their action in the human subject; anæsthetics cannot be used, but the experiments are of short duration, 3237–42, 3244, 3299; such knowledge cannot effectually be communicated by oral instruction, 3303.

In experiments with poisons, witness only uses those which have marked characteristics and act speedily, such as charcoal vapour, strychnia, &c.; uses frogs, rats, and sometimes birds, rabbits, and guinea-pigs, 3401–5.

Induction of disease.

Experiments for the induction of disease are from their duration unsuited for demonstration, 3243–4.

Neither at King's College nor elsewhere in England are animals "crucified sometimes for several days," 3333–8; has never seen any "frightful" experiments before students, or anywhere else, 3351–2; the students in either London or Edinburgh would not tolerate such things, 3339, 3356–7; on the contrary they have a repugnance to experiments even under anæsthesia, and would rebel if painful experiments were used for demonstration without it, 3340–4, 3354–5. Thinks it is libellous to say that medical students as a body have "maleficent tendencies," 3345–50.

Good feeling of medical students.

Experiments at the West Riding Lunatic Asylum.—Details the method of performing the experiments at Wakefield with regard to the functions of different portions of the brain, 3227–9. In these experiments æther or chloroform was in all cases administered at intervals, but no signs of pain were observed during the time its administration was suspended, 3363–4; in some cases in seeking to keep the animal unconscious it was overdosed and killed, 3383–6.

Anæsthetics were always used.

Experiment on the dog.

In the experiment on the dog in which one hemisphere had been entirely removed, witness was ultimately inclined to attribute its barking to a sense of pain, 3365–6; observes that such an experiment need not be repeated, 3374; but points out with regard to this and other experiments referred to, that a certain apparent consciousness is not inconsistent with entire insensibility to pain, 3367–72, 3375–7, 3379–82; and that just as a frog deprived of both hemispheres, and therefore incapable of feeling, may be made to croak by stroking its back, so the dog may have been caused to bark without feeling any pain, 3413–8.

Signs of pain.

In the human being a part of the skull may be removed and yet not the slightest pain be felt, 3377; assuming that pain was caused by the stimulus it would

cease when the stimulus was taken away, and would be as transient as the pain caused by a pinch to a man asleep, 3408–12.

Experiment on the cat.

The experiment on the cat "with and without chloroform" was the means of proving that the non-manifestation of pain was not caused by the action of the anæsthetic, 3378.

Does not believe that in these experiments pain was produced by the action of the stimulus, although it induced the signs which usually imply pain, 3406–7; adverts to the fact that under chloroform consciousness returns before sensibility to pain, and often remains while sensibility is lulled, 3427–30; and that it is said that patients observe what takes place during an operation, and yet feel no pain, 3431–2; refers to the fact that in a mesmeric state persons will rationally answer questions, and yet be entirely insensible to bodily pain, 3419–23.

Consciousness may exist with complete insensibility to pain.

Validity of argument by analogy.

In arguing from experiments on animals as to the functions of the human brain thinks that if pathology is in harmony with physiology the results may be trusted, 3387–8; and adverts to the fact that the brain in the monkey tribe is very like the human brain, 3381. Believes that in the main his conclusions are accepted by physiologists, 3387.

Experiment on a human being.

An experiment of a like nature was tried on an Irish servant whose brain was exposed, and the results were found to be the same; the patient died, it is said from disease, 3390–1; entirely objects to experiments on human beings but, for the sake of accumulating facts and advancing knowledge which may mitigate human suffering, thinks the sufferings entailed on animals are not excessive, 3392–7.

Extent of Practice of Experiment.—There are very few physiological investigators in England, but they are for the most part eminent men, 3256–9. Besides the laboratory at Wakefield, where much good work has been done, and his own, does not know of any private laboratories, 3327–31.

Experiments on living animals have sometimes been necessary to convict criminals, 3300–2.

Lectures at the London Institution.—Entirely denies that at the lecture with regard to the functions of the brain delivered at the London Institution and referred to by the Secretary of the Society for the Prevention of Cruelty to Animals there was any attempt to give a comic aspect to the phænomena described, gives details, 3274–95; there is no foundation for such statements, the managers of the institution were very anxious for him to lecture again in the same season, and subsequently, 3314–9; cannot recollect anything in his lecture that could be construed into "a joke" about the stupidity of the monkey, 3358–9; nor can he recollect saying that he was afraid to say on how many cats he had repeated the experiment, 3360; he had then experimented on about 100 animals altogether, 3381; nor did he observe any laughter in the audience, 3362.

No attempt was made to give a "comic" aspect to the facts described.

Regulation of Experimentation.—Experiments by ignorant or incompetent persons are not likely to be of any use, 3260–1; would, himself, have the greatest abhorrence of any man undertaking painful experiments without being convinced that good would result, 3426; but thinks that the majority of mankind may be trusted not to make such experiments unless some good can be got by them, 3400.

Experiment by incompetent men is unlikely.

"Cruelty to Animals" Act.

Approves of the law for the prevention of cruelty to animals; thinks any infringement of its provision, under the plea of scientific experiment, should be judged on its merits, 3269–71, and that a law drawing a clear distinction between experiments for research and the "cruelty to animals," aimed at by Martin's Act, is desirable, 3273.

The licence system

Considers that granting licences on the certificate of responsible and known persons would, perhaps, be the best compromise, although it would be objectionable in witness' opinion, 3311; believes that there is no law at present to prevent a man, whether competent or not, practising medicine or surgery, if he likes, 3312.

would not greatly interfere with research.

Thinks a system of granting licenses to competent persons would not interfere to any great extent with original research, if not coupled with inspection and the necessity of keeping a record of experiments, 3272.

Objects to inspection of laboratories.

Objects to any kind of inspection, 3272; is strongly opposed to the proposed inspection of physiological laboratories, 3320–1; considers that inspection by competent men even would be unsatisfactory, and by incompetent men it would be useless, 3323–5; experiments would often be left unperformed, on account of the trouble of communicating with the inspector, 3322.

Ferrier, David, M.D.

Contends that a knowledge of facts which might appear to an inspector most insignificant may be, and often are, pregnant with results of world-wide importance; for instance, Galvani's observation, that a frog's legs twitched under certain conditions when touched by two metals, 3424; and thinks for this reason the control of an inspector very undesirable, 3425.

Objects to licensing places.

Objects to experiments being allowed only in licensed places; many investigators live in the country, and would be debarred thereby from experiment, 3326–7.

Difficulty of deciding as to competency.

Is not opposed to a restriction being placed on incompetent persons, if it can be decided who is incompetent; but thinks there should be no restriction placed on men of ability, although not prepared to say how ability could be tested, 3262–8; considers a student of psychology, for instance, should not be restrained from such experiments, 3264; although possibly not up to a medical standard of competency, 3395, 3399; it would seem, however, necessary to have some special examination as a test, if the system of licensing persons were adopted, 3304–10.

Thinks the difficulty of fixing a standard of competency insuperable; that legislation limiting experimentation to competent men would consequently be a restriction on original research, 3251–4; has no other objection to legislation, 3255, 3262.

GEORGE HOGGAN, M.B.

Late H.M. Indian Navy, 3433, and formerly Demonstrator of Anatomy, 4198, 4206; and subsequently engaged in teaching separately in Edinburgh, 4204.

Has acted as assistant in a laboratory, 3434, and tried three or four experiments himself, but never got further than giving the anæsthetic, as the animals died under the anæsthetic, and witness consequently resolved not to try again until he had learned how vivisection was done, 3471; knows nothing from personal observation of London physiological laboratories, 4101–3.

Testing a new theory by experiment.

Having devised a new mechanical theory for respiration, when demonstrator of anatomy at Edinburgh, witness was advised by Dr. Handyside and other medical gentlemen to test it by experiment on the live animal, 3472–8, 4022–5; he was then within about a year of taking his degree, and had received all the instruction in anatomy and physiology which was necessary for taking a degree, but felt, nevertheless, incompetent to undertake such experiments, 3490–506; although as his theory was based on the contention that the action of the muscles in respiration was generally misunderstood, 4024, it could have been tested under chloroform, 4026–33.

Will not pronounce an opinion as to whether it was usual at Edinburgh for the professors to encourage young men who showed an aptitude for original research, although aware that they award gold medals for some of the theses which are sent in before attaining the M.D. degree, and which very usually treat of some matter of original research, 3507–27.

Vivisection by students.

Believes other students beside himself were advised to experiment on live animals, and that there was in the years 1867–72 considerable private experimentation among the students at Edinburgh, with the concurrence of the lecturer, Dr. Handyside; refers to a student who had made 10 or 15 experiments on cats, 3479–85; thinks anæsthesia was not used, as he was recommended to avoid cats, because they squalled and scratched when cut, 3480, 3486–9, 4209–24; believes that the students at Edinburgh undertook experiments whenever they thought any theory could be proved or advanced by that means, 4104–6; they would also get animals and open them, in order to see the heart beat, 4212.

Cats.

Cannot recall names and details, but references to private experimentation were made to him in conversation, dozens of times; fears it would be as useless to seek information on the point as to inquire into freemasonry, 4225–8.

Experiments with strychnine.

In one instance saw galvanism tried in a cellar, but not by a medical man, as a cure for poisoning by strychnine, it failed, 3462, 3465–9.

Requiring original remarks in theses tends to increase vivisection.

Deprecates the practice at Edinburgh of requiring theses, containing original remarks, before taking a degree, as a fruitful source of vivisection; thinks the great frequency of the practice in France is due in part to the University of France requiring such theses, 4238; but admits that number of theses at Edinburgh on experimental physiology is relatively small, 4242–3.

Prolonged experiment on the nerves of a curarised dog.

Reads a translation, giving in detail Paul Bert's observations in 1869, 4111, 4235, on a curarised dog, in which several of the principal nerves were successively laid bare and tied, and subsequently excited by galvanism, 4111, the dog being meanwhile kept alive by artificial respiration. After 10 hours the temperature fell, and the next morning the dog was found dead, although the artificial respiration machine still worked. The results arrived at were chiefly that contraction of the bladder was effected through the intervention of the sensory nerves, and that sugar was found in the saliva as well as the urine, 4109–11, 4140–1.

Hoggan, George, M.B.

possible reflex action of the muscles.

Admits that according to the received theory with regard to reflex action the results obtained by the action of the sensory nerves may have been unaccompanied by sensation, but has doubts as to the correctness of the received theory, 4143–53.

"Atrocious" sufferings of the dog,

Considers that tying the nerves would cause great pain, 4229–30; refers to the difficulty of getting at the splanchnic nerve, and the intense pain it must, he believes, have caused; is of opinion that in one form or another experimentation was being carried on during the 10 hours, 4165–9, and that during the whole of that time and until death surpervened excruciating suffering must have been endured by the animal, 4231–2; and if it had not died no doubt the experiments would have been resumed on the next morning, 4196–7; considers that the intelligence and sensitiveness of the animal remained intact, and that its sufferings were the most atrocious that the imagination of man could conceive, and the result trivial in the last degree, 4111–4. Does not for an instant suppose that Paul Bert considered curari an anæsthetic, 4154–6.

insignificance of the results.

Points out that in many persons contraction of the bladder may be produced by the pain given by a pinch; thinks obtaining the same result by such an immensity of pain a wholly inadequate result, 4163; does not consider the appearance of sugar in the saliva in induced diabetes a fact of much importance, 4162; considers the fact that the pneumo-gastric and sympathetic nerves remained intact as an instance of the previously known but curious action of curari in attacking one nerve after another as the dose is increased, 4157–61, and that it proves that the dose of curari in this experiment was small, an important fact, considering what its action, when given in small doses, is, 4161.

Thinks the translation is perfectly fair, and that his own remarks on it would be justified if ten times stronger, 4170–1.

Extent of Practice of Experiment.—Professor Rutherford's experiments show the progress experimental physiology is making in England; formerly one had to go to the continent to get a knowledge of the practice, 4203, 4205.

Considers that the course of experiment in England and abroad is much the same, 3459–61; and that such experiments as Paul Bert's will certainly be undertaken in England unless they are forbidden; it is for the British people to say if such things shall be done here, 4164.

Has found physiologists, especially abroad, among the kindest of men, but with an entire want of feeling with regard to the infliction of pain, 4244.

"Criminal" repetition of painful experiments.

Thinks the repetition of the experiment, described by Claude Bernard,—in which it is shown that irritation of the chorda tympani produces an increase of secretion in the glands,—criminal, although in the first instance it was a valuable, as well as a severe and painful, experiment, 4034–51; has never seen it performed under anæsthetics, believes they might affect the result, 4048–9.

Action of certain drugs on the livers of dogs.

Considers the experiment in which a glass cannula was inserted in the bile duct of dogs, who had been kept fasting for 18 or 19 hours, in order to arrive at the action of certain drugs, useless and cruel in the extreme; refers to the intense pain produced in the human being by gall stones, 3464; and thinks this experiment on the dog would, for the first six or eight hours under curari, give even greater pain, 4052–61; adverts to the experiments by which it was proved that mercury had no effect on dogs, and points out that the result is simply the knowledge that the effects of a certain drug on a dog are *not* the same as its effects on a man, 3464.

Knowledge of tubercle not advanced by experiments.

Doubts, in spite of the number of experiments, whether any better knowledge of what tubercle is, has been gained during the last 30 years; points out that M. Crocq has proved that M. Villemin's theory, which was previously accepted, is erroneous; thinks the experiments prove rather what tuberculosis is not, than what it is, 4134–9.

Bernard complains that vivisection is undertaken by incompetent persons, and that science is thereby encumbered with a mass of contradictory results, 4238, 4241.

Often insufficiently given.

Anæsthetics.—Thinks anæsthetics do more to lull public opinion than to mitigate the sufferings of animals; first, because often if given thoroughly they would kill the animals before the experiment could be concluded; and secondly, because they require an assistant to be specially told off to keep them up, 4107–8.

Morphia.

Quotes Claude Bernard's opinion that the action of morphia is "stupéfiant," and produces immobility, but does not suspend sensibility, 4133. Maintains that under morphia or opium pain is felt as long as the cause remains, but forgotten when it is withdrawn, 4184–93. Points out that the dog referred to by Bernard cried when pinched, quotes passage on the point, 4133, 4194–5.

Curari.—Is as fully convinced that curari is not an anæsthetic as he is of any point of physiology, 4115–6; and contends that its action is so well known that further experiments are not needed, 4199.

Bernard's opinion.

Considers that it is sufficient to bring forward Bernard's opinion that curari is not an anæsthetic, without referring to other scientific men who differ from him, 4172–4.

Bernard although the introducer of curari deprecates its present excessive use, 4239–40; and expresses himself as if it were "horrible beyond all conception" to be under its influence; puts in the book containing the passage referred to, 4117–8.

Not considered an anæsthetic abroad.

In the laboratory in which Paul Bert's experiment was performed, has frequently heard it remarked, "Look at that animal, you would suppose it does not feel, but it feels doubly," 4234–7. It is scarcely possible that any man in France can believe that curari is an anæsthetic, 4177.

Human experience of curari.

Refers to particulars of two instances of human beings who had been put under curari, and of one who had been operated on under it; the latter complaining of "*douleurs atroces*," and the former speaking of the loss of power to move, while preserving entire consciousness, 4119.

Quotes Vulpian.

Quotes Professor Vulpian's statement, that "curari does not act on the sensory nerves, or at least does not abolish their function;" it "in no way annuls sensation, at least in ordinary doses," 4126–9; whilst in larger doses it destroys not only the power of the sensory nerves, but life itself, 4175–83.

Refers to Yule's experiments.

Thinks Mr. Yule's experiments with curari on a frog, assuming him to be correctly described in "*Nature*," must be disregarded, as the results obtained are opposed to those arrived at by the greatest authorities in experiments which he has himself seen, and considers the arguments based on them are invalid, gives details, 4121–31.

Limits of Experiment.—Advocates the regulation, but not the abolition of vivisection, 3435–7; wishes to stop abuses, but not to hamper investigation, 4065.

Repetition of experiment.

Considers the repetition of painful experiments demonstrating admitted results an abuse, gives instance of an experiment of this kind, 3454–8; but does not approve of limiting painful experiments to investigations with regard to new discoveries, as it would be a useless restriction and be easily evaded, 4244.

Demonstration on living animals unnecessary.

Does not consider demonstration on living animals necessary, mechanical contrivances, such as the sphygmograph, will show all that is necessary, 4244; thinks class demonstration on living animals should be forbidden under heavy penalties, 4244.

Vivisection should only be allowed in a theatre open to the public.

Regulation of Experiment.—Has discussed the question with numerous other persons, and they agree in considering, 3442–52, 4083, 4088, 4100; that private vivisection should be forbidden under penalties of imprisonment, and that experiments on live animals should only be allowed in places fitted with proper appliances, and containing a gallery to which a small number of the public should be admitted by ticket; gives details, and points out various advantages arising from such a course, 3438–41, 4084–7.

Experiments on certain animals should be forbidden.

Such institutions should also contain accommodation for animals on whom pathæological experiments were being tried; the use of anæsthetics should be compulsory, and that of curari forbidden in the operations, and such animals as horses, monkeys, dogs, and cats, exempted from experiment, 3441.

Does not think that the animals being used for pathological experiments would be disturbed by being seen by the public, who should not be able to touch them, 4096–9.

Public spectators.

Thinks five or ten students and a like number of the general public, who should be admitted in the same way as they are to the British Museum reading room, 4067–9, should be allowed to go into the gallery. Admits that the public would not be competent to appraise the scientific value of experiments, but thinks the plan would prevent misrepresentation as to the objects and nature of experiments, 4070–3; although the public spectators might, for instance, mistake reflex movements for signs of pain, just as they might not be aware of intense suffering when there was no movement, 4091.

Admits that the spectators might resort to manifestations of repugnance or disapproval; but thinks that under proper regulations such proceedings could not be repeated, 4092–5; and points out that the scheme proposes that complaints may be made to the superintendent, 4092.

Admits that vivisectors would object to be over-looked, but the prevention of carelessness and cruelty would outweigh this disadvantage, 4074–8; would advocate the same course in regard to operating theatres, if it should appear that in England the instruction of students was placed before the feelings of the patients, 4079–81.

"Cruelty to Animals" Act.

Does not desire to see any alteration in the Cruelty to Animals Act, 3441; and does not advocate licensing persons, 4082.

EMANUEL KLEIN, M.D.

Assistant professor at the laboratory of the Brown Institution, Lecturer on histology at St. Bartholomew's Hospital, and author of part of the Handbook for the Physiological Laboratory, 3528–30, 3620.

Studied pathology and histology at Vienna under Prof. Brücke and Prof. Stricker for four years, 3690–4, 3737; has subsequently practised in Vienna, 3545, and in London, 3556–7; and is now attached to the Brown Institution, 3688; has tried very few physiological experiments in England, 3610, 3594, 3596; the most painful experiment with the knife in his special researches has been injecting into the jugular vein, 3686–7, 3652–3; his experience of vivisection has been principally abroad, 3582; performs no experiments in his lectures at St. Bartholomew's, 3558–9.

Experiments for demonstration.

The Brown Institution.—Has, with Dr. Sanderson, the direction of those that come to the Brown Institution to make special researches, 3620, 3572; and performs certain experiments in his private room for teaching purposes, 3612, before students who come from the hospitals in order to go more thoroughly into microscopic or histological anatomy, 3626.

Anæsthetics.

There is no class, 3663, but experiments are performed it may be for one student only, 3675; there are in all three or four painful experiments, principally on frogs, which are killed as soon as possible—gives details, 3664–70; considers, however, that the frog's sensibility is of a low order, 3583–9, 3707–16; anæsthetics are only used for convenience' sake, 3631–3; and the frogs are not pithed, 3681–3; about 10 or 12 animals, mostly frogs, no dogs, and possibly one kitten, are consumed during the year, 3676–80; has performed these experiments for about three years, 3665; the animals are obtained by private purchase, 3575–8.

Witness' position with regard to the University of London is not clearly defined, but he has received a title and works in the laboratory, and lives in the Brown Institution, but receives no remuneration, 3574, 3612, 3615–7.

Nature of the investigations for Government.

Investigations made for the Government.—The experiments made at the request of the medical officer of the Privy Council, 3621–5, 3649–51, have been very few, 3609; and have all been of a pathological nature, 3611; and have related to tuberculosis, typhoid fever, sheep-pox, and pyœmia, 3581; the only operation involved has been making an incision for the purpose of making an injection, 3590–3, 3595, 3652–3, 3662; no anæsthetic was given to the dogs because of the pain being small, but the cats were chloroformed for the sake of convenience, 3597–8, 3636–43; some of the diseases induced, 3630, 3634–40, have been painful, but chloroform could not be used, 3656–9; monkeys, dogs, cats, frogs, and other animals, bought at the Government expense for the purpose, have been used, 3590–3, 3684–5, 3576–7; the total number used during 1874 was 14 or 15, 3614;

Mr. Forster's Minute.

Mr. Forster's minute did not apply to these experiments as there were no severe operations, 3645–8, 3746–9; with the exception of these experiments witness has performed no private experiments, 3579–80, 3607–8.

Use in demonstrations.

Anæsthetics.—Never uses anæsthetics for the lower animals, but thinks that in experiments on the higher animals for demonstration they should be used out of deference to the feelings of those present, 3538, 3542, 3563; cannot say whether their use affects the efficiency of the demonstration, 3555.

Klein, Emanuel, M.D.

Use for convenience' sake.

In investigation anæsthetics should not be used, 3562–3, except for convenience' sake, 3660–1, 3538; gives instances, 3599, 3562, 3602–5; an investigator has no time to consider the sufferings of the animal, 3538–44, 3542–4.

Dogs.

Some experiments,—gives instances,—can be done in less time and with less suffering to the animal without chloroform than with, 3560–1, 3741–5; giving chloroform to dogs is a long and probably painful operation, 3601, 3654–5.

Disregard of animal suffering abroad.

In painful experiments when chloroform cannot be used witness asks whether any of the students object, 3550–2, 3627–8; on one occasion the experiment on the cornea of a frog was objected to as being useless, and was not performed, 3671–5; believes on the continent the feelings of the animals are entirely disregarded, 3546, except when interfering with the convenience of the operator, scientific men being of opinion that animal suffering is of little importance compared with scientific research, and the general public abroad not claiming to criticize scientific matters, 3549, but the English public appears to have a considerable regard for the feelings of the animals, 3547–8, 3554; believes, but cannot say definitively, that investigators in pure physiology, 3700–6, share his views with regard to anæsthetics, 3606, and thinks there is little difference of opinion between English and foreign physiologists on the point, 3553, but admits that Dr. Sanderson does not agree with him, 3750–2.

Considers curari an anæsthetic.

Curari.—Considers curari an anæsthetic, 3718–20, because it stops breathing and therefore diminishes the vitality, and also because Dr. Schiff's experiments at Florence—gives details—appear to prove that it acts on the sensory nerves. Is unaware of Claude Bernard's reasons for holding the reverse opinion, and also of the experiments on two human beings, 3753–74; its action on the respiratory organs prevents its use in some instances, 3600.

Use of anæsthetics taken for granted when possible.

Handbook for the Physiological Laboratory.—The book is intended as a guide to students working under proper rules and competent supervision, 3533, 3537, 3565; it is assumed that anæsthetics will be used whenever possible; considers that this rule is accepted in English laboratories, 3566–7; has performed the experiment on the mesentery of the frog about twice for teaching and several times for pathological purposes, 2723, under curari, which witness considers an anæsthetic, 3719–26; has not performed experiments on the injecting small mammalian animals, 3727–8, nor on arterial pressure in England, 2729.

In England.

Extent of Practice of Vivisection.—Physiology is much more attentively studied in England now than when witness first came over, 3567–70; does not think there is any private experimentation out of a laboratory, 3571.

Abroad.

Prof. Stricker's laboratory at Vienna is intermediate in point of size between Prof. Brucke's and Prof. Ludwig's, 3734–8; there are no lectures given there, 3693, but the amount of work done is very great, and the number of animals used, principally frogs, dogs, and rabbits, very considerable, 3695–9, 3730–3.

EDWARD ALBERT SCHÄFER, M.R.C.S.

Assistant Professor of Physiology at University College, London, 3765–6.

Was educated in England, but has studied abroad, 3793. Has been a lecturer for two years at University College, and studied there for eight years previously, 3803–4. Witness' teaching is entirely on the tissues, 3775.

Believes that both professors and students desire to minimize pain, 3772–3; and that from feelings of humanity, anæsthetics are given whenever possible, and that almost all experiments are under them, 3770–1, 3779, 3807–9; very few painful experiments are performed, 3774.

In the practical physiology section there are two divisions; one for demonstration on the living animal, and another, of recent origin, in which advanced students, or medical men, perform experiments in conjunction with the professors, 3778.

A large number of frogs, about half a dozen dogs, and a somewhat larger number of rabbits are consumed in the year, 3782–3.

Frogs.—At University College, during the last 10 years at least, 3805–6, anæsthetics have been seldom given to frogs, 3788–90; either for research or demonstration, 3809–10, 3866–7; they are, however, often otherwise made insensible, 3837, 3865; with the view of facilitating the experiment, rather than from consideration for the feelings of the frog, 3798–800, 3864. Many of the experiments, however, are very severe, and would be very painful if the frog had proportionate sensibility, 3795–6, 3801.

Schäfer, Edward Albert, M.R.C.S.

Thinks chloroform would be a great irritant to the skin of a frog, 3816; and has doubts whether the anæsthetic would not cause as much pain as the operation, 3797.

Does not consider the frog as sensitive as the higher animals, 3791; besides it cannot reason about its sensations, 3859. Thinks a frog's restlessness when irritated by a parasite due in part to reflex action, 3860–3.

German frogs are preferable to English, because the parts are larger, 3784–7.

Extent of practice of Experiment.—The number of laboratories in London is very small; the largest are attached to University College, Guy's Hospital, King's College, and the Brown Institution, the number of workers in them could be easily arrived at, 3820–3. Is unaware of the existence of any private laboratories, 3817–9. Does not believe in the existence of any private experimentation by students, 3824–7.

Experimentation Abroad.—Was at Professor Ludwig's laboratory for three months during the past summer, 3850–1; the annual consumption of animals there would amount to more than 100 dogs and rabbits, besides frogs, 3852–5. Foreign physiologists endeavour to minimize pain, 3856–7. There are not many painful demonstrative experiments abroad, but more than in England, 3858.

Experiments for Demonstration.—Has seen formidable, but not "frightful," experiments performed before students, but always under anæsthesia, 3828–32, 3836. A painful experiment without anæsthetics before a class would be likely to cause a commotion among the students, 3833–5.

Experiments should only be repeated when there is doubt as to the result, 3813; results arrived at by such men as Professor Ludwig would be at once accepted in England without repetition, 3814–5; experiments in his own research have been entirely novel, 3812.

Curari.—Is doubtful as to how far curari is an anæsthetic, and therefore, when possible, frequently gives opium as well, 3780–1; instances experiments in which opium is also given, 3838–45, 3848–9.

JOHN GRAY M'KENDRICK, M.D.

Lecturer on Physiology at the Extra-Academical School of Edinburgh, 3869–72; and at the Edinburgh Royal Veterinary College, 3876.

Is also in private practice in Edinburgh, 3868; and was formerly also assistant to the Professor of Institutes of Medicine in the University, 3873, 3875; has been connected with the Veterinary College for a year, no experiments on live animals are performed there, 3970–3; the lectures at the school qualify for graduation at the university, 3872.

Necessity for Experiment.—Thinks that little additional knowledge with regard to the nature and origin of disease is to be hoped for from anatomy, or from the old methods as to the action of drugs; thinks further results must be gained by experimental methods, 3987–92; for instance, although anatomy did something in tracing the nerve fibres of the spinal cord it was reserved for physiology to show that the fibres on each side were connected with the opposite side of the brain, 3878–9.

Considers that a correct diagnosis of many diseases would be impossible without the knowledge obtained by the experiments of Bell and Brown-Séquard, 3878; and points out that the more we know of the nature and origin of disease the more possible prevention and cure become, 3992–4.

Probable decrease of vivisection.

Believes that as science advances vivisection may become less necessary; instances the investigation into the changes produced by light on the retina, in which it was thought necessary at first to excise the eye for experiment, but the facts can now be demonstrated all but painlessly on the live animal, 3905–6.

Increased knowledge.

Results obtained by Vivisection.—Reads portion of an article contributed to the British Medical Journal by himself, entitled "What has Vivisection done for Humanity?" Contends therein, i. *That it has advanced* *physiological knowledge;* instances 22 discoveries with regard to the functions of the nerves and nerve centres, the physiology of the circulatory, respiratory, and digestive organs, the action of light on the retina, and the phenomena included under electro-physiology. ii. *That it has aided medicine and surgery;* refers to the transfusion of blood, artificial respiration, knowledge of the causes of the sounds of the heart, the cure of aneurism, the knowledge of the phenomena of inflammation, and of the nature of zymotic diseases as supporting this view. iii. *That it has advanced therapeutics,* by the discovery of anæsthetics, of the action of many drugs, and of antidotes, 3916, 3878. The article enumerates the more important advantages obtained only, 3917–21.

Improved treatment.

Refers to the abandonment of the section of seventh nerve for neuralgia, 3879; and also, in some detail, to Dr. John Reid's experiments on the cranial nerves; to witness' own, in conjunction with Dr. Dewar, on the effects of light on the retina; Dr. Hope's on the sounds of the heart, and those by Dr. Hughes Bennett and others with regard to the antagonism of certain drugs and the discovery of antidotes or probable antidotes for strychnia and the Calabar bean, 3879.

Antidotes for strychnia and Calabar bean.

Points out that accurate knowledge of the action of most drugs has been reached by experiment, 3921; and that scientific results often produce practical, though unforeseen, benefits, 3795.

Chinoline.

Refers to his investigations, in conjunction with Dr. Dewar, resulting in the discovery of the anæsthetic effects of chinoline, which has the remarkable effects of acting on the nerve centres rather than on the nerve terminations, and of not interfering to any great extent with respiration; refers to experiments with it, 3906–14; this discovery was made in 1874, and has awakened much interest, but as it produces nausea in the human being is of little practical value at present, except for the lower animals; further investigation is needed with regard to its power and capabilities, 3922–8.

Sir James Simpson.

Believes Sir James Simpson experimented on animals with chloroform and many other substances, but he also tried them on himself and his assistants and was often ill after his experiments, 3945–2.

Cannot recollect details of Dr. Hughes Bennett's experiments with mercury on the liver of dogs, 4001–2.

Does not experiment on frogs as if they had no sensibility, but kills them first, 3915.

Had relief of pain and cure of disease in view in most of his physiological experiments, those on the eye were for purely scientific ends, 3972. The infliction of pain is an evil justifiable only when a great advantage is obtainable, 3895–6.

Believes that the progress of discovery is likely to be rapid, judging from the results of recent years, 3930. Discoveries are always promptly announced to the profession, 3984. Thinks the advance of physiological science will tend to mitigate the sufferings of animals, 4020–1.

Anæsthetics.—There is a considerable latitude of opinion among physiologists as to anæsthetics, 4009; considers that, except with regard to the action of drugs, nearly all experiments in original research may be rendered, either wholly, or in part, painless, 3899–905, 3933; thinks even if an anæsthetic affects the organ experimented on, an allowance may be made for its effect; points out that a severe operation introduces abnormal conditions, especially if pain be felt, and the physiologist often has to choose between the disturbance caused by pain and the disturbance caused by an anæsthetic, 3934–8.

Allowance for the effects of anæsthetics.

In experiments on the nerves,

Believes that some of the facts connected with the nervous system might possibly have been ascertained under partial, though not under complete, anæsthesia; the most painful experiments were, however, done so carefully, and are so well corroborated that they need not be repeated, 3941–4; anæsthetics were used in the experiments with regard to the effect of light on the retina; had the experiment failed it would have been tried once, with all possible precaution, without anæsthetics, 4015–6; considers these experiments his principal work, 3985–6.

and on the retina.

Effects on the liver.

Curari.—Believes that Dr. Rutherford first tried the effect of curari on the liver, and then when trying the action of other drugs on animals under the influence of curari made an allowance for its effects; cannot say whether it would have been possible to have used an anæsthetic and then made an allowance for it, 4003–8; in such experiments the pain, which in this instance would be severe, and the probable scientific gain should both be considered, 4017–8.

Experiments for Demonstration.—For demonstration only painless experiments are necessary, 3898, 3933; is content to show students the physiology of the nerves, muscles, and heart, and certain facts regarding respiration on decapitated frogs, 3897; but does not think it necessary to show students the action of strychnine, or the action of hydrate of chloral as an antidote, except in the case of advanced students, 3886–92, 3966–9; the experiment, moreover, is not very painful and is soon over, 3880–5.

Painless experiments only needed.

Action of poisons.

Experiments by Students.—Except under the superintendence of the lecturer, students are not encouraged to attempt private experiments at Edinburgh, which is probably the most active school in the kingdom, 3893–4, 3996–9.

In one instance, witness heard of a student experimenting on a cat, but having pointed out to him the useless nature of his proceedings they were abandoned, 3915; as a rule, neither students nor young medical men practice experimentation, 3974–9, except under supervision or when preparing their theses, 3979–81; very few experiments can be performed out of a laboratory, the appliances now used are very expensive, 3982–3.

On a cat.

Regulation of Vivisection.—Does not see how legislation can regulate vivisection, 3953–4; and fears it would retard the advancement of science, 4012; has a private laboratory himself, 3960–5; and disapproves of licensing places as likely to inconvenience competent and earnest men, but does not object to licensing persons, 3955–8, 4012, 4019.

Licence system.

Persons who avow that the sufferings of animals are beneath the concern of the physiologist should be restrained; is entirely of an opposite opinion, 3959.

Considers that experimenters should either be competent physiologists or under one, 4010; but would rather dissuade them from the practice than restrain them by law, 4011.

If there were great abuse of the practice of experiment would approve of adequate restriction even at the risk of retarding the advancement of the science, but does no believe such abuse exists, or is likely to exist, 4013–4.

MR. ROBERT SAWYER.

Is a member of the legal profession, and of the Committee of the Society for the Prevention of Cruelty to Animals, 4246–7; also of the society's sub-committee on vivisection, but does not think he concurred in the report referring to the "grim" character of Dr. Ferrier's experiments, 4278–81.

Heard the whole of one of Dr. Ferrier's lectures at the London Institution, and considered it very interesting, 4249–52, 4270. From 50 to 70 persons were present, 4264–5; Mr. Colam, Mr. Reid Thompson and Mr. Thomas Allen were also there, 4259, but not in the same part of the room, 4260–1.

Dr. Ferrier's lectures at the "London Institution."

Understood Dr. Ferrier to say that the animals were rendered insensible to pain, 4253–5, 4257–8. Thinks that the experiments on the brain of an insensible monkey went to show that exciting particular portions produced movements usually denotative of pain, 4267–9; did not understand that Dr. Ferrier had performed a single unnecessary experiment, and did not think that the lecturer covered the "grim character of the experiment by his humour," 4276; but he seemed to witness to consider that he was justified in taking the lives of any number of animals for his investigation, 4256, 4272.

Experiments on the brain of a monkey.

Was sorry to hear Dr. Ferrier's smiling remark, 4274–5, that he was "afraid to say how many cats he had" experimented on, received with a titter, 4253, 4266; believes the reference to the stupidity of the monkey must have occurred at the other lecture, at which he was not present, 4282–3.

JOSEPH LISTER, M.B., F.R.S.

Professor of Clinical Surgery in the University of Edinburgh, surgeon in ordinary to the Queen in Scotland, 4284–5.

Results obtained by Experiment.—Refers to the advantage he has derived from his first experiments, 4338, made when he was first appointed a teacher, 4303–6. These experiments eventually led to the development of the antiseptic system of treatment which was tested in the first instance on calves and horses, 4350–8; considers that these experiments caused very little pain after the operation, as a properly applied ligature causes little or no pain even in the human being, 4359–62.

Discovery of the antiseptic method of treatment.

Considers that experiments on living animals is one of the most important means of increasing knowledge, 4291–2.

Experiments for Demonstration.—Attaches very great importance to demonstration as a means of instruction, 4339–43, gives instances of its advantages, 4312–3; has only employed it on one occasion himself, many years ago, as his subject, practical surgery, does not from its nature require it, 4308–9, 4317–23.

Anæsthetics should always be used.

Considers that in class demonstration anæsthetics should be used, not so much because it saves the animals pain, but because it prevents the demoralizing effects of seeing unnecessary pain inflicted, 4328; has never seen or heard of any levity at demonstration before classes, 4311, 4314, and does not believe that properly conducted experiments demoralize students, 4367–9.

Experiments should be performed for definite objects only.

Limits of Experiment.—Considers that in these experiments the prospect of a probable gain to the human race should over-balance personal reluctance to undertake them, 4329; but experimenters should always have a clear and definite object in view, 4328, and regard it as a duty to give chloroform in severe experiments whenever possible, 4321–2; is of opinion that in the higher animals experiments can nearly always be rendered either wholly or partly free from pain, 4293–4; and although thinking that painful experiments will be necessary, believes the pain will be usually short, 4296–7, 4315–6.

Effects of rhubarb on the secretion of bile.

Placing a frog in hot water.

Thinks the eight or nine hours suffering entailed on the dog while experiment was being made as to the effect of rhubarb on its liver justifiable, it would not suffer as much as a rat caught in a gin, 4404–7; placing the frog in water and raising the temperature would be painful, but does not consider the sufferings of the frog worth serious consideration, 4420.

Difficulty of giving frogs chloroform.

Anæsthetics.—The inflammatory action of chloroform on a frog's skin renders it inconvenient to use, 4379, 4416; believes that ether would be open to the same objection, and that even taking the frog in the hot hand would be very painful, 4417–19. It is a laborious process to chloroform a horse, and hardly necessary except for severe operations, 4331–7.

Sensibility of Animals.—Entertains no doubt that the lower the organization, the lower the sensibility, gives various reasons and examples, 4374–8. Cannot compare the sensibility of a rabbit and a dog or guinea-pig, or of a wild fox and dog, 4432–4.

The frog "the physiologist's animal."

Does not think the frog capable of suffering much pain, gives reasons, 4390, 4411–4; considers that it has become "the physiologist's animal" on account of its tolerance of severe treatment, and from the fact of its comparative insensibility, 4391; and does not consider it necessary to give them anæsthetics, 4415; believes that it would be very difficult to prove that frogs *suffer* from parasites, 4392; points out that the kicking and struggling of a frog or of a worm on a hook goes on after the brain is removed, and cannot therefore be an evidence of pain, 4380–1, 4384–5, any more than the unconscious movement of a paralysed limb or of the sensitive plant, 4386–9.

Convulsive movement not necessarily a sign of pain.

Vivisection and pheasant shooting.

Non-scientific vivisection.

Believes that the pain caused by vivisection in Great Britain during a whole year would be exceeded by the amount produced in a single day's pheasant shooting, 4328, 4401; and points out that popular feeling acquiesces in the removal of a blemish from a horse, 4408–10; and in the castration of animals for human comfort and convenience, and contends that operations for scientific ends stand on far higher ground, 4328.

Humanity of English vivisectors.

Believes experiments in England are only undertaken by competent men, 4426, and that there is no danger of inhuman vivisection here, 4401–3. Considers that medical men are the most humane class in the community, they know more accurately what suffering is, 4330, and see more of it, 4395; admits, however, that this does not apply to the increasing class of pure physiologists, 4396–8; contends also that the inevitable pain inflicted before the discovery of chloroform by operations, did not render surgeons hard-hearted, some, such as Mr. Syme, were among the kindest of men, 4370–1.

Mr. Syme.

Difficulty of defining competency.

Regulation of Experimentation.—Admits that painful experiments should, in the interests of science, be undertaken only by most competent persons, 4297*a*–8; but points out the difficulty of saying who are competent, 4301; and doubts whether legislation would not retard research, 4299–300; would himself consider a thoroughly instructed medical student "competent," 4303; believes men unknown to fame undertake such experiments, and thinks it would be as undesirable to check them as it would have been to have stopped Davy's and Faraday's experiments when they were unknown, 4324–7; has never known of any experiments by incompetent persons, 4302; thinks it would be undesirable to exempt the domestic animals from experiment, 4421–4.

Exemption of domestic animals from experiment.

Objects to limiting experiment to public laboratories, and considers that admitting an uninstructed public to see experiments would be most undesirable, gives reasons, 4348–9.

Does not think that nine-tenths of the original research going on could be stopped without loss, but if it could, no one can say which tenth should be retained, 4399–400.

Legislation would be a slur on the profession.

Thinks prescribing by law that experiments for demonstration should only be allowed under anæsthetics unnecessary, and considers that it would be a slur on the profession, 4344–7, 4393, it would imply a fault where none exists, 4430; does not think the case of the Factory Acts analogous, 4394.

Objects to legislation as being superfluous, 4425–9, although admitting that if it did not interfere with private investigation it would not be objectionable, 4431.

Analogy between man and other animals.

Miscellaneous.—Considers that as the phænomena presented by horses and calves is similar to those observed under similar circumstances in the human being, it is reasonable to suppose that the results will be analogous, 4363–6; has not attended lectures or experiments by others since he was a student, 4310, and has conducted all his investigations in private, 4307.

Mr. Syme's experiments were very important, and though before the discovery anæsthetics were performed as humanely as possible, 4286–90.

MR. GEORGE RICHARD JESSE.

Honorary Secretary of the Society for the Abolition of Vivisection, 4437, and formerly a railway engineer, 4436.

States that the society regrets that a Royal Commission has been considered necessary, and is of opinion that what is needed is the enforcement of the Cruelty to Animals Act, 5556; regrets the supineness of the Society for the Prevention of Cruelty to Animals, and deprecates any legislation short of forbidding vivisection, 6475; the society's object is to show the demoralizing effects upon the experimenters themselves, especially as boys and girls are now to be taught physiology, *page* 271. Believes that Professor Huxley's "Elementary Physiology for Boys and Girls" is an evidence of the spread of vivisection, and shows that in place of mercy, cruelty, "the most devilish" of vices, is being taught instead, 6465–7; objects to enter into technical questions, but decidedly considers that the passage quoted (*see* 6511,) from that work is a distinct encouragement to vivisection by young students, 6506–23, and that the other passages quoted have some tendency in the same direction, 6524–7.

Juvenile vivisection.

Is prepared to establish that the cruelties of vivisectors are not surpassed by any recorded in the history of mankind, 4439; considers vivisection itself a moral ulcer, and proposes to read extracts from various works to prove it, 4438–9; protests vehemently against it, giving various reasons and arguing that it tends to divorce intellect from moral principle, 6475.

Discusses various reasons advanced in support of vivisection, and rejects them all, considers that it entails degradation of mind as proved by the examples of Spalanzani, Brachet, &c.; quotes various writers in supporting this view, 6475; and observes that the society has abstained as much as possible from referring to continental practice, 6473.

Human vivisection.

Believes it not impossible that physiologists would vivisect human beings if they dared—criminals were vivisected at Montpellier, baby farms in England have not escaped suspicion, whilst in America a poor and friendless woman was actually made the subject of experiment, 6475.

Insignificance of the discoveries made by vivisection.

Discoveries wrongly ascribed to vivisection.

Maintains that the discoveries made by this means are utterly inadequate to justify such cruelty. Begs that the Commission will ask each vivisector to specify his individual discoveries by this means, and then obtain the opinion of impartial men with regard to these alleged discoveries, 6475; points out that Harvey's, Hunter's, and Bell's great discoveries were not made, as is too often erroneously supposed, by vivisection, and that high medical authorities, such as Gall, Curwin, Bell, &c., have said that vivisection is not only useless but misleading, 6475.

Hospital funds.

Witness admits that he was unaware that the funds of the hospital as a charity are entirely distinct from those of the medical school, but contends that the prospectuses of the hospitals seem to him to favour the view, 6528–49, that the money given to cure the sick is misappropriated for torturing animals, 6475.

States that thousands of letters have been received by the society from educated persons urging in strong terms the abolition of vivisection, and contends that such deep feeling could not have arisen without sufficient cause, 6475.

Complains of the "unfair conduct" of certain newspapers in the matter, 4439.

Was not aware, but is glad to know that the Council of the College of Surgeons in London expressly states that vivisection is not required, 6550–1.

Dr. Hughes Bennett's experiments on a cat.

Reads a variety of extracts to prove the correctness of the views held by the society, commencing with a detailed account of Dr. Hughes Bennett's experiment in which theine was injected under the skin of a cat, the results being intense cerebral excitement, gradual loss of power in the spinal chord, and death, 4443.

Dr. Ferrier's experiments.

Alleges insufficient use of anæsthetics.

Reads papers referring to Dr. Ferrier's experiments giving details of experiment on a guinea-pig which was narcotized, experimented on, allowed to recover from stupor, and then again narcotized, *page* 220; adverts to Dr. Ferrier's observation that "before and throughout "all the following experiments ether or chloroform was "administered," and infers that the experiments preceding this remark on guinea-pigs, rabbits, and cats, were not under anæsthesia, *page* 230; also draws attention to the expressions "the animal was only partially narcotized," "with and without chloroform," &c., *page* 220; refers to an experiment on a rabbit in which, during the five days intervening between the first and second operations, the exposed part of the brain became *the* seat of suppuration and fungus cerebri, *page* 220.

Growth of fungus cerebri during an experiment.

Functions of different parts of cats' brains.

Reads in detail a series of observations made during an experiment lasting four hours on a strong cat in which the right hemisphere had been laid bare, referring to "signs of pain," "cries as if of pain or rage," "spitting," &c. under the action of electricity, *pages* 220–1.

Of dogs' brains.

Refers to various experiments on dogs and specially to one lasting eight hours, during which various portions of the brain were successively removed, and the various phenomena observed, some of which Dr. Ferrier considered apparently attributable to retention of consciousness, and a distinct sense of pain; amongst other things, "when called to it would struggle to get up," "it ran against furniture, walls, and jammed its head into impossible corners," *page* 221.

Correspondence with Dr. Ferrier.

Reads correspondence between Dr. Ferrier and himself, in which the former complains of not being fairly quoted, and also of the erroneous inferences drawn with regard to the use of anæsthetics, *pages* 221–3; reads, from the pamphlet in which this correspondence is published, remarks on the subject, *page* 222.

and Dr. Crichton Browne.

Reads letter which appeared in the "Times" from Dr. Crichton Browne, complaining of certain statements with regard to Professor Ferrier's researches which appeared in the advertisement of the Society for the Abolition of Vivisection, characterising it as "skilfully misleading," and the conclusions deduced from the "adroitly selected," quotations "as unwarrantable," also reads reply from himself which did not appear in the "Times," contending that the manifestations which usually imply pain, implied it in these instances, and that many of the expressions used in the descriptions indicate that this view is correct; also asserts that chloroform is given to narcotize the public mind, and make the patient helpless, *page* 221.

Partial consciousness of animals under experiment.

Read various extracts from the proceedings of the Royal Society relating to Dr. Ferrier's experiments, many parts of which are described in detail and desires to draw particular attention to the fact that in some of them the "animals were rendered only partially insensible during the process of exploration," that in others it is stated that the animals did not "apparently lose consciousness completely," and that in one instance the animal was allowed to remain quite conscious during stimulation, *pages* 275–7.

Reads passages from Hansard, giving details of experiments which excited great disgust and sensation when brought to the notice of the House of Commons, in order to prove that the cruelties of vivisection are not exaggerated, *page* 223.

Refers to Drummond's "Rights of Animals," and various passages in Dr. Wilson's "Life of Dr. John Reid," and reads passage referring to the remorse expressed by Dr. Reid for having inflicted such sufferings on animals, *page* 223.

Edinburgh University.

Reads particulars with regard to the recent plans for

a physiological laboratory at the Edinburgh University, *page* 223.

Quotes from the "Scotsman" a passage by Dr. Rutherford, referring to the practice at Edinburgh University of performing experiments without narcotics when necessary for the purpose of research or verification of results, *page* 223.

Huxley's Elementary Physiology.

Reads extracts from Professor Huxley's "Lessons in Elementary Physiology" for "boys' and girls' schools," describing various experiments on the spinal chord, sympathetic nerve, &c., and also quotes the preface to the effect that the organs and tissues of the common domestic animals afford sufficient material for acquiring a practical knowledge of elementary physiological anatomy, *pages* 223–4.

Alfort.

Refers to passages in various works relating to the subject, and among them one describing the horrors formerly perpetrated at Alfort, and another relating to experiments by students in their rooms; does not know the writers of these works or anything beyond their statements, 4446–55.

Dr. Chalmers.

Puts in a reference to a sermon by Dr. Chalmers against vivisection, 5553–6.

Majendie.

Quotes various passages from Dr. Elliotson's "Human Physiology," tending to show that Dr. Majendie was in the writer's opinion unnecessarily cruel, also others referring to the unsatisfactory nature of the experiments made by various eminent men, and to Gall's and Sir C. Bell's objections to vivisection, *page* 271.

Experiments on the brain of dogs.

Quotes from a work by the editor of the "Veterinarian" an account of experiments on birds, in which various portions of the brain were removed, and the phenomena presented under exposure to violent pain, to heat, to burning by red hot irons observed, also of experiments on dogs, in which a gimlet was forced into the brain of a dog and its doings observed for six days, and of another in which the experiments lasted 16 days, during which it retained in part its docility and intelligence, *pages* 271–2; and reads further extracts, one giving Sir Charles Bell's opinion against men causing protracted suffering, and another referring to mental and moral attributes of animals, *page* 272.

Sir Charles Bell.

Reads extracts from the "Quarterly Review," Mayhew's "Dogs, their Management," Blaine's "Canine Pathology," the Memoirs of Dr. Wilson of Edinburgh; the Life of Sir Astley Cooper, the last, witness contends, showing the demoralising effects of vivisection, *page* 272.

Dr. John Reid's experiments.

Reads passages from Dr. John Reid's Researches, to show the contradictory opinions of various eminent vivisectors as to the cause of death, and nature of asphyxia, *page* 273; quotes various passages from Dr. Reid's works, showing the painful nature of some of his experiments on the nerves, and giving full details in several instances; also refers to Dr. Reid having abandoned several experiments on account of their painful nature, *pages* 273–5.

Sir Walter Scott's opinion.

Quotes Sir Walter Scott to show the demoralising effect of vivisection on vivisectors themselves, reads passages from the "Personal Recollections of Mrs. Somerville," *page* 275.

Reads extract from his own "History of the British Dog," *page* 275.

Biliary fistula in cats.

Reads extracts from St. Bartholomew's Hospital Reports 1873, containing an account of Dr. Legg's experiments on biliary fistula in cats, and giving details with regard to some of the experiments, which lasted from 5 to 29 days, and also adverting to the differences of opinion existing with regard to the cause of death, *page* 277.

Effects of aloes and rhubarb on the liver.

In order to show that painful experiments are being carried on, reads extract from the British Medical Journal, giving details of experiments for testing the effects of aloes and rhubarb on the biliary secretion of the dog, *pages* 277–8.

Gives references to passages in numerous works to which he desires to direct the attention of the Commission, 5566.

Vivisection a source of error.

Sir Charles Bell's discoveries were not made by it.

Quotes Sir Charles Bell's opinion that vivisection has "never been the means of discovery," and "has done more to perpetuate error than to enforce just views," and that his own discoveries were deduced from anatomy not experiment, and stating that he only resorted to experiments because his utmost powers of persuasion were lost without them. Reads from Pichot's Life and Labours of Sir Charles Bell, an extract from one of his letters showing his reluctance to experiment on live animals, reads also the author's account of Sir Charles Bell's reasons for objecting to vivisection. Reads further passages with regard to the necessity of pain, and the littleness of man, and his greatest discoveries, and contends that they bear on the subject with which the Commission has to do, 6423-5; refers to Collier's Translation of Celsus with the same object, 6425-7.

Spalanzani.

Reads extract from Macilwain's "Memoirs of Abernethy," in which Abernethy censures Spalanzani for having "tortured animals in vain," and says that the nature of some of his experiments prove him to have been a "filthy minded fellow," 6436; reads other passages from the same work, 6436–40.

Mr. Erichsen's experiments with regard to asphyxia.

Reads extract from an article by Mr. Erichsen in the Edinburgh Medical and Surgical Journal of January 1845, on the pathology and treatment of asphyxia, giving details of an experiment (one out of several) in which the circulatory systems of three dogs were connected by tubes and one of the dogs suffocated, and the phenomena observed, 6453–8; was aware that the results of these experiments were reported to the British Association, but not that the Royal Humane Society had awarded Mr. Erichsen the Fothergillian gold medal for these researches, 6459; puts in reference to a passage with regard to vivisection in Bayle's Dictionary, 6459–6460*a*.

Private experimentation.

Refers to Baker's edition of Kirke's Handbook of Physiology as a book "calculated to make young men "try these so-called experiments in their lodgings or "elsewhere," 6460*a*, and as containing records of very cruel experiments, 6466–70.

Contrary opinions held by vivisectors.

Reads passages showing differences in the results arrived at by different vivisectors, and referring to the contradictory opinions held, 6460*a*, 6473; reads accounts of experiments showing the condition of the brain in various forms of death, 6460*a*, the intensity of the respiratory efforts when the animal is deprived of air, and the various phænomena observable during apnœa, 6465; the extremes of temperature which animals can endure, the phenomena attending starvation, the digestion of the stomach after death, the reproduction of the extremity of the tail after amputation in the salamander, also reads passages relating to the functions of the pneumo-gastric nerve, &c., 6473.

Tenders an extract from the "Sporting Magazine" of 1825, and regrets that the Commission will not receive it as evidence, 6427–36.

Witness desires to read passages showing Mr. Macilwain's and Sir W. Fergusson's opinions, but is informed that the reading of those passages may be dispensed with as both those gentlemen have appeared as witnesses, 6438–43.

Inquires whether the Commission could supply the society with Dr. Ferrier's Croonian lecture, but is informed that the office of the Commission is simply to receive information, 6443–53.

Contends that the Commission having power to "call for, have access to, and examine" all documents giving "information on the subject," have power to receive letters addressed to the society, and regrets that they have decided not to do so, 5551–2, 5556–61.

Refuses to give particulars with regard to the society.

Can give a list of the members of the society with the exception of some who do not wish their names known, 4440–2.

Has made these statements on behalf of the society, but declines to state whether they were adopted at any meeting of the society, or whether there has been any general meeting, declines to state in what manner the committee was appointed, or whether there is a treasurer, or whether the accounts have been audited; considers such questions inquisitorial, and that the constitution of the society is a private matter with which the public has nothing to do; states, however, that the society was formed during 1875, that there had been a meeting of some of the members of the committee (but will not say of how many), and that the names of the committee had been advertised in the papers, 6478–6502.

ROBERT McDONNELL, M.D., F.R.S.

Examiner at the College of Surgeons, surgeon to Steeven's Hospital in Dublin, and formerly for many years Lecturer on Anatomy and Physiology, 4456–9, 4499–51.

Irish feeling on the subject.

Popular opinion on the subject of vivisection is very strong in Dublin, although no abuses exist in the Irish schools, 4460–1, calling for legislation, 4522; sentiments of humanity are strong both in the medical profession and among the students at Dublin, 4462–3; and also among both English and American physiologists generally, 4519–21.

Anæsthetics.—Both for demonstration and for original research anæsthetics can nearly always be employed, at any rate during the most painful portion of the experiment, 4466–9, 4585–6; always uses them himself when possible, 4518; they cannot, however, be used in experiments with regard to the nerves of sensation,—gives instances, 4486–8; but these experiments are very few in number, 4470–1, 4587; renders frogs and even tadpoles speedily insensible by putting them into water containing a little chloroform, 4563–5.

Anæsthetising frogs.

Necessity of experiment, including some painful ones.

Experiments for Demonstration.—Contends that certain experiments are essential in medical education; instances the exhibition of the circulation of the blood in a frog's foot, 4505–8, and Weber's experiment on the pneumogastric nerve, which can be done painlessly but cannot be adequately described in words, 4528; with very few exceptions does not approve of painful experiments before classes, 4568.

Improved method of surgical treatment.

Chloroform.

Artificial epilepsy.

Results of Experimentation.—Contends that the difference between the surgery of the present day and the horrible practices described by Ambrose Paré is largely due to vivisection, and urges that if the experimental method had been introduced sooner a hundred years of torture might have been saved. The discovery of chloroform, 4537–8, and of the present method of treating aneurism were arrived at by experiment, 4539–40; as also Dr. Brown-Séquard's peculiarly valuable discoveries with regard to the conducting channels of the spinal cord and the artificial production of epilepsy, 4491–8; refers in detail to the benefits resulting from the knowledge of the practicability of the transfusion of blood, and of the torsion, in lieu of the ligature, of arteries as examples of the advantages obtained by experiments on animals, 4541–62*a*; and observes that he believes so strongly in the benefits to be got by experiments, that in common with other medical men he has himself submitted to considerable pain in experiments on himself, 4575.

Experiments on himself.

Experiments by Students.—Never knew of any experiments by students; believes they would not tolerate an experiment without anæsthetics except for some good reason, 4464–5, 4512–3; if students attempt any painful ones they should be stopped, but they should be allowed under supervision to watch the circulation of the blood or the effect of a grain of mustard seed on a frog's foot, 4572–3; and those who desire to advance science should begin as soon as they have passed their final examination to experiment independently in the laboratory; did so himself, and has also made a very few experiments at his own house, 4514–7.

Experimental research has been long and humanely pursued in Ireland, 4511; but there is no practical course in any of the schools; has sometimes permitted some of his more intelligent students to be present at some of his private experiments, 4484–5, 4502–4; the total number of animals witness has used has been very small, 4509–10; considers that if a class is impressed with the idea that good will result from the experiment the effect is humanizing, whilst passively witnessing suffering would be brutalizing, 4480–3; the method of giving scientific instruction is much better now than 20 years ago; then even chemistry was not taught practically, 4524-6.

Difficulty of legislation and of inspection.

Regulation of Vivisection.—Believes that the position of physiologists would be improved by legislation on the subject,—gives reasons, 4476, 4479, 4490, 4523, 4588; but thinks the matter surrounded with difficulties, 4475; neither of the bills of last year appears to very well meet the case, 4477; a system of inspection could hardly combine the two essentials of being really efficient and of not interfering with the progress of science, 4478, 4589; the importance of an experiment is a matter of opinion, and often no one can foresee the result. Dumas could not have dreamt that he was on the eve of discovering such a blessing to mankind as chloroform when he was distilling ants to get formic acid; nor Galvani the electric telegraph when observing that electricity made a frog's leg move, 4575–8, 4580; the suggested admission of the public to laboratories would be useless, for physiologists would then experiment in private, and many delicate experiments would be rendered impossible, 4530, besides many movements,—gives instances,—really due to reflex action would be ascribed by the uninstructed to the presence of pain; recollects being once assailed for showing the pulsation of a frog's heart after its removal from the body, 4531–4.

Experiments in public.

Repetition of Experiments.—When a fact has been once thoroughly established the experiments demonstrating it if painful should not be repeated, although while the matter is *sub judice* they may be rightly undertaken, 4472–3, 4587; believes Brown-Séquard's experiment, which witness has himself performed, in which epilepsy is artificially produced, is painless,—gives reasons, 4571.

Duration of painful experiments.

Miscellaneous.—Classifies experiments, and states that setting aside experiments as to the action of drugs and a few others, 4490, protracted agony is never necessary, 4474, 4488, 4587. Cannot state the longest period which it is necessary to keep an animal in a state of pain, but the amount of suffering is often small and the time always brief, 4489, 4570.

Qualification necessary for discovery.

Disputes the assertion that nine tenths of these experiments are made by men incompetent to discover anything, 4579–84; the converse would be more accurate, 4581–3; what is required is painstaking carefulness and an exact record of facts; believes the discovery of galvanism, for instance, was really made by Galvani's wife rather than by the philosopher himself, 4575, 4580.

T. HAYDEN, M.D.

Professor of Anatomy and Physiology in the Roman Catholic University of Ireland, and Fellow of the College of Physicians, 4591–5.

Experiments for educational purposes;

Considers that experiments for demonstration are as a rule unnecessary and of little use except perhaps to senior students, 4598–9, 4603; when witness first became professor he performed some experiments,—gives details,—before his class to satisfy himself, but has performed none since. The higher animals used were always anæsthetized, 4595–7, 4600; considers experiments necessary in new ground even if painful,—gives instances, 4601–2. The feeling among educated Irish people is very strong on the subject of animal suffering, 4604–5.

for research.

MR. JOHN CLELAND, M.D.

Professor of Anatomy and Physiology at Galway since 1863, 4607–8, 4633–4, and formerly demonstrator at the great Anatomical School of Edinburgh, 4645–6.

Cruelty to Animals Act.

Regulation of Vivisection.—Has not seen the proposals made on the subject, but thinks the Cruelty to Animals Act, which might be widened to include all mammals, 4621–5, sufficient without further legislation, 4620; qualified teachers should be unrestrained as regards their choice of experiments, 4618–9, and as to the use of anæsthetics, 4611, on which point all physiologists must judge for themselves; believes, however, there would be less pain given if physiology were as well studied here as abroad, 1660.

Anæsthetics.—Ought to be used whenever possible in painful experiments on mammals, 4611–3, 4617, 4643, 4647–8; part, if not the whole, of an experiment can generally be done under anæsthetics,—gives instances, 4644; witness always piths or decapitates frogs instead of giving chloroform, 4632, 4658–9.

Frogs.

Sensation of Pain.—The extent of a frog's sensibility to pain is a matter of speculation, 4614–6, but from the low organization of its brain and other reasons,—given in detail,—considers that probably frogs feel no pain comparable to that felt by the higher animals, 4626–32.

Medical Education.—Is of opinion that the curriculum of medical education is very heavily weighted, 4640; and that students have not time for the experimentation necessary for thoroughly studying physiology, nor has a professor, who has two such subjects as anatomy and physiology, time to devote to it; in fact his own teaching produces medical men, but not physiologists, 4639, 4641–2, 4655–6; thinks that in physiology as in anatomy direct observation is desirable whenever possible, 4638; and that experiments, including some painful ones, 4657, are most important, 4635–7.

Necessity of experiment in teaching.

Physiological handbook.

Thinks the experiments in the Handbook of the Physiological Laboratory should be shown for purposes of demonstration, but to a select class only; students generally have too little time to go thoroughly into the subject, 4650–2; does not vivisect much, and has never exhibited experiments on any animal higher than a frog, but has seen vivisection in Professor Ludwig's laboratory at Leipsic, where the majority of experiments are for research, although there are some for class demonstration, 4653–4; also knows what is being done generally, and of what great advantage experiment has been to physiology, 4609–10.

MR. CHARLES DARWIN.

Although he has never had anything to do with experiments on live animals and does not claim to be a physiologist, 4670, witness has taken a warm interest in the subject, 4661, and is fully convinced that physiology can progress by the aid of experiment on live animals only, the position of the valves in the veins gave rise, for instance, to many surmises with regard to the circulation of the blood, but certainty was only arrived at by vivisection, 4668.

Importance of physiology.

Believes that physiology is destined to confer great benefits on mankind, and that the prohibition of experiments would be a great evil, especially as all but an extremely few experiments can be performed on insensible animals, 4669–71; but at the same time any unnecessary infliction of pain, either by omission to give anæsthetics when possible, or otherwise, deserves detestation and abhorrence, 4672.

Resolution of the British Association.

Cordially approves of the resolutions of the British Association in 1871 with regard to vivisection, 4662–4, and also in the main of Dr. Playfair's bill on the subject, 4662–4.

FRANCIS SIBSON, M.D., F.R.S.

Member of the Senate of the University of London, and consulting physician to St. Mary's Hospital, 4673–5.

The Chloroform Committee.

Has taught the practice of medicine and clinical medicine for some 20 years, 4696–7; performed a few experiments many years since, and subsequently some others with regard to the action of opium, chloroform (as a member of a committee of the Medical and Chirurgical Society,) and as to the movements of the heart, the last of which have been much referred to, 4676–4727–8.

Physiology the foundation of pathology.

Necessity of Vivisection.—Considers physiology is the true basis on which pathology rests, 4706–8, and has found his own investigations with regard to the respiratory organs of use every day of his life since, 4709–10; refers to the importance of various discoveries made by means of experiments on animals, with regard to the antagonism of certain poisons, 4783.

Experiment essential to medical education

Believes it to be as impossible to teach physiology by lectures only as it would be to teach medicine without clinical practice, 4705; considers that the introduction of the practical method of teaching in the various branches of medical science, including physiology, has been the vital improvement of recent years, 4698–704, 4713; the old teaching was no teaching, and the knowledge of anatomy even required by the examiners was very slight indeed, 4711–2.

Anæsthetics.—Considers that the fundamental facts of physiology can be demonstrated on insensible animals, 4714, and that the number of very painful experiments for research is very small, 4744.

Has never heard of anæsthetics having been given for the convenience of the operator only, such a spirit should be repressed, 1718–20.

Anæsthetic effects of disease.

In pathological experiments either anæsthetics can be given or the pain is too trivial to require their use, 4678–80, and in most instances the disease itself acts as an anæsthetic, especially in cases of fever; has observed this effect of disease among the patients in hospitals, 4681–3; observes that the more painful ailments, such as carbuncle, are never made the object of experiment, 4684; and refers to the absence of pain in erysipelas and tuberculosis, 4684, 4730–2.

Schiff, Kolliker, and Claude Bernard.

Mr. Yule's experiments.

Curari.—Does not admit that the desire to believe curari to be an anæsthetic has led to its being so regarded, 4761. Although its first action is on the motor nerves, witness agrees with Schiff in thinking that in sufficient doses it also affects the sensory system, 4752–9. Believes that the conclusions of Kolliker and Claude Bernard go beyond the premises on which they are based, 4762–77; considers that the present position of the question is that there are no grounds from which either the consciousness or unconsciousness of the animal to pain can be logically deduced, and that Mr. Yule's experiments appear to show that curari deadens the reflex action of the spinal chord, 4778–82.

Admits, however, the subject is a moot point amongst physiologists, 4752, and that some to avoid doubt give opium also, 4758, 4779.

Useless experiments.

Regulation of Experimentation.—Is not prepared to admit that abuses have increased in the same ratio as the practice of vivisection itself, 4733–6; does not regard Sir Charles Bell's assertion that a large number of useless experiments were performed in his day as of any weight, as he carried his reluctance to experiments so far that he left some of his great discoveries to be confirmed by the experiments of others, 4737–9.

Sibson, Francis, M.D., F.R.S.

Incompetent experimenters.

Considers that experiments by incompetent persons are useless, but does not believe that abuses are frequent enough to require legal repression; fears that restriction would clip the wings of science, and prefers to leave matters to men's consciences and public opinion, 4688–95; does not, however, object to the Cruelty to Animals Act, 4692.

Humanity of physiologists.

Believes that physiologists endeavour to give as little pain as possible, and "spend no end of pains in planning and preparing" for them, so as to get as much out of them as possible to avoid repetition, 4685–7, 4716–7. Thinks one result of leaving English investigators alone will be to show foreign physiologists that great scientific results can be reached with greater humanity, 4739.

Majendie.

Miscellaneous.—Majendie had an object in every experiment he did, but was a ruthless repeater of them, he seems never to have considered that he was dealing with sentient animals, he might have obtained the same results with less suffering, 4739, 4743.

Mayo.

Mayo's experiments although not great experiments were certainly justifiable, 4740–2.

Exposing animals to high temperature. Boiling frogs to death.

Thinks the suffering caused by raising or by reducing the temperature of the body until death supervenes cannot be very severe, as in either case the animal would soon become unconscious, 4745–9. When the temperature of an animal is raised to 112° or 113° it practically dies, and when it sinks to 70° or thereabouts it loses consciousness, 4784–6. Goltz's experiment of boiling a frog to death is horrible, 4750.

Chossat's starvation experiments.

Considers that Chossat's starvation experiments were carefully planned, and have shown the necessity of a certain temperature being maintained in order to preserve life, 4751; is inclined to think no great pain was given, starvation induces discomfort, extreme cold (which causes pain), and then deadening of the nerves, which stops pain, 4787–9.

Biliary fistula in dogs.

Does not think that the experiment of establishing a biliary fistula in dogs under curari would be very painful even if curari were not an anæsthetic, gives reason, 4752, 4760.

"Frightful" experiments.

Considers medical students like other young men are fond of animals, and that any "frightful" experiment would certainly be stopped by them, 4721–6.

JOHN MALLET PURSER, M.D.

Professor of Institutes of Medicine at Trinity College, Dublin, 4791.

Was educated partly in London, partly in Dublin, 4829; has been a professor at the school of physic two years, but taught in a private school for seven years previously, 4811.

Gives two courses of lectures, one on practical histology, the other on physiology, 4792, and has about 50 or 60 students each year, 4821.

Ill effects of prohibition of demonstration on live animals.

Is considerably impeded in his physiological course by the regulation of the university forbidding experiments on live animals, 4792–3, even when under anæsthetics or when pithed, 4839–43, 4847–9, 4867, 4886; the prohibition does not extend to experiments on animals just killed, 4839–43, 4868, or to tying out a frog's toes to show the circulation of the blood, although that must cause the frog some inconvenience, 4841; if the regulation were repealed, the control of the University authorities would prevent abuse, 4834–5.

Regrets not being able, for instance, to show the influence of the pneumo-gastric nerve on the heart on a pithed frog, 4843, 4851–4; believes the authorities would allow him to cut off a frog's head and then demonstrate it, although he has not asked permission to do so, 4855–76.

Some painful experiments are desirable for demonstration.

Considers that experiments on live animals are necessary for demonstration, the greater number would be under anæsthetics, 4793–99*a*, but some painful ones on the sensitive nerves of warm-blooded animals are desirable, 4793–9*a*, 4832–3; does not believe students would object to any properly conducted experiment even if painful, 4804.

Occasionally gave painless demonstrations himself on live animals whilst teaching privately, and saw a few in the same school when a student, 4826–33, 4881.

No experiments are performed by students at Dublin; thinks they should not be allowed to undertake them except under the direction of the professor, 4812–6, 4835–8.

Private experimentation by students.

Does not believe private experimentation exists among students, and there is no need therefore to forbid it, 4817–20; students have no taste for experimentation, gives various reasons for the fact; believes any desiring to undertake them would come to their professor; believes that the more students know of experimentation the less likely they are to attempt it without due care, 4884–5.

Purser, John Mallet, M.D.

Farm vivisection.

Is aware of the extent of farm-vivisection, but considers its existence is not a valid argument in favour of scientific vivisection, and any law against needless infliction of pain should, however, have a general bearing, 4877–80.

Teachers should not be subject to restraint as to experiments.

Objects to legislation, however, altogether, and thinks that teachers of physiology should be the sole judges as to the propriety of an experiment, 4800–9; his own experience of university restriction causes him to deprecate any further restraint, 4850.

Believes that professors are humane, and that students will not experiment, and considers legislation would be unnecessary and harmful, 4825.

MR. JAMES B. MILLS.

Veterinary Surgeon in the Royal Artillery, 4913, and formerly at college in Edinburgh, 4915.

Anæsthetics.—Chloroform was used at the Veterinary College for very painful experiments, and also for delicate operations to secure steadiness, but not for firing and minor operations, 4962–3, 4967–8; uses it, however, himself for firing if the surface is large, 5046–52; there is no difficulty in giving chloroform to a horse if it be done with care, 5050.

Lithotomy performed on the live animal for demonstration.

Vivisection for Veterinary Demonstration.—Horses and donkeys bought for dissection were kept in a paddock until wanted, 5120–1; and sometimes first used for demonstration, instances a horse in which lithotomy was performed under chloroform and the animal allowed to live 12 or 14 hours before being killed, although he must have suffered considerable pain, 4969–92, 4996; dogs were also used for such experiments, 4981; lithotomy is very rarely performed on the horse, and might have been learnt from the dead subject, 4993–5; also instances a case in which the jugular vein of a pony was opened and the blood blown back and the animal allowed to live for some time afterwards, this experiment was not under chloroform, 4997–5008; in a third instance tenotomy was performed as an experiment on a horse, not under chloroform as the operation is not very painful, 5034–44.

Blowing back the blood.

Tenotomy as an experiment.

These were the only experiments on live animals at the college during the time witness was there, 5053–4; they were all performed by Mr. Williams, the present principal, 5104–6.

Experiments by Students at the College.—Knows that experimentation by students has been going on for five years, and believes it will be continued until put a stop to, 4952–5.

A dog sent by its owner to be destroyed, 5165–6, had some prussic acid given it, 5136–49; it was then partly restored by ammonia, and finally had its brains knocked out by the students, gives details; the professor did not know of this, 5009–31, 5150–5.

Unnecessary painful experiments.

Believes that many unnecessary operations involving great pain are still performed there, refers to an instance which occurred last winter of which all the students, and, he believes, the principal must have known, 4936–46, 4957, 5125–6. A horse suffering from disease of the sensitive laminæ of the feet, 4965, was bought for dissection, 4957, 5122–4, and kept for at least a week in a paddock attached to the college, 4957, 4964, 5108–9, and during that time was experimented on all over, 4966, by the students without, as far as witness is aware, any sort of control, 4950–1, 5107. The horse was neurotomised on both fore feet (which would, however, relieve him from the pain of the disease), 5110–7, 5129–32, then subjected to tenotomy of the perforans and perforatis, and to various minor operations, 4960–1, 5118–9, 5127–8. Anæsthetics were not used and the horse must have suffered a great deal, 4958–9, 5179–80. These operations were done openly in the quadrangle attached to the college (which is not, however, overlooked), 4947–9, 5133–5. Except in this instance witness believes such experiments are not known of by the authorities, 4956–7. Declines to give the name of the gentleman from whom he received the information, 5102–3.

Uncontrolled experimentation by students.

Experiments by Students in their rooms.—In his first term was admitted to at least two meetings for private experiment by students at their lodgings and subsequently to a great number, 4916–23; is sorry to say he operated himself, 4924.

Mills, Mr. James B.

Cat hunts.

Cats and dogs hunted down or caught by a poisoned bait and then painfully restored by antidotes, were used, 4927–31; these experiments were simply to demonstrate known things, 4928; and were due to idle curiosity and a reckless love of vivisection only, 4932.

Poisoned baits.

Was present at five private experiments by students, they usually consisted of opening the thorax to see the heart beat, and of picking up blood vessels and nerves, gives details, the authorities knew nothing of these experiments; about 6 or 8, including two medical students, were present; did not know of any other students making such experiments, 5055–100, 5181–3; in two cases the animal was partially narcotised by the poisoned bait, 5167–72; the house in which these experiments were performed was occupied by three students and the landlady and her husband, 5156–7; the cries of the cat (the landlady's) disturbed the neighbourhood, and witness and the others nearly got into a great deal of trouble about it, 5158–9. Subsequently was lecturer on materia medica, but the time being summer there was no dissection, and consequently no animal to experiment on, 5173–8; has not experimented on animals since he obtained his diploma, 4935; knows nothing of the proceedings of the Veterinary Society of London or of the Medical School of Edinburgh, 5160–4.

The statement in the paper sent by witness to the Royal Society for the Prevention of Cruelty to Animals is an unadorned statement of facts, 4914, 4933–4.

WILLIAM BENJAMIN ARCHIBALD SCOTT, M.D.

Licentiate of the College of Physicians at Edinburgh, and of the Faculty of Physicians and Surgeons of Glasgow, 5184–5.

Has been in communication with Mr. Jesse, 5186.

Circulation in the mesentery of a frog.

Has attended class lectures on physiology and pathology and read standard works on the subject, but has witnessed two or three experiments only, 5188; one of these formed part of the practical course at Edinburgh in June 1871, and was performed by the professor or his assistant, witness was so disgusted by it that he never went again, 5188–91, 5200–3, 5227–8, 5233–4; the experiment—describes it in detail, was the demonstration of the circulation of the blood by the microscopic examination of the mesentery of frogs under curari but not pithed, and which therefore, in his opinion, were suffering considerable pain, 5190–3; admits, however, that it would have been very difficult to have kept the frog under chloroform without curari for so long a time, 5235–6.

Theory as to inflammation.

Considers that the object was in part to illustrate a particular theory of the demonstrator's, 5192, with regard to the formation of the pus cells in inflammation, refers to the theory in detail, the practical importance of the question was simply nothing, except as showing the nature of inflammation, 5221–3; admits, however, that the discovery of the circulation of the blood although at the time a matter of pure science has since borne practical results, 5224–5.

Physiological Handbook.

Curari.

Refers to various experiments in the "Handbook for the Physiological Laboratory," showing that curari destroys or suspends the irritability of the motor nerves but not, as the book observes, that of "the sensory nerves or central nervous system;" also adverts to the absence or insufficiency of directions with regard to anæsthetics, and to the long duration of some of the experiments, 5194; Dr. Sanderson's work shows that curari is used in England although it has been publicly stated that it is not, 5226.

Thinks that the exposition of the movements of the heart, the demonstration of the lymphatic circulation, and similar experiments ought not to be undertaken without an anæsthetic in addition to curari, either in the case of frogs or the higher animals, 5195–9.

Adverts to the duration of pathological experiments, gives examples, 5193.

Experiments by students.

Believes that experimentation by the more zealous students in their own rooms is common, judges that this is the case from their common conversation, but can name only one student who used to so experiment, he operated on frogs, 5238–51.

WILLIAM LEGG, M.D.

Demonstrator of Morbid Anatomy at St. Bartholomew's Hospital, London, 5315–7, 5252.

Method of obtaining the animals for experiment.

Experimentation at Saint Bartholomew's Hospital.—There is no physiological laboratory at St. Bartholomew's, but a private room belonging to Dr. Brunton is used for experiments, 5257–8; knows nothing of the manner in which live animals are obtained for experiment, and has not inquired, 5318–9; before performing an experiment reads up all that has been written on the subject, 5286; and invariably employs anæsthetics where practicable, 5331, 5342; has performed four or five series of experiments, 5255; all of which have had reference to diseases which may occur in man, 5320–2.

Legg, William, M.D.

Ligature of the biliary duct in cats.

Ligature of the Biliary duct in cats, and other Experiments.—Performed a series of experiments on 16 cats as to the effect of the ligature of the biliary duct, 5256, 5259. The number of animals used was larger than in any other series, 5313, but not too large he considers for securing accurate results, 5265–7; believes, judging from human experience, that neither the disease nor the wasting away, nor the other symptoms could have been very painful, 5269–80. The results could not have been obtained by killing the animals after three or four days, 5277; the experiments were however conducted as humanely as possible, 5290–1.

Diabetic puncture.

The uncertainty with regard to the date of death except as regards one cat which strayed away, 5274–5, was caused by his absence on Sundays, he saw them every other day, 5287–9. The diabetic puncture was made under chloroform specially to see its effect on a diseased animal, 5281–3.

The main results were the increase of the connective tissue and the discovery that glycogen is not produced after ligature of the bile duct, both new and important points, 5260–5, 5284–6, 5292.

Observes that liver diseases are very common, some of them are very obscure, and great difference of opinion exists, and it is therefore a subject which it is very desirable to elucidate, 5322–4.

Bile pigments in the urine.

Attempted innoculation of cancer.

Exposure to high temperature.

Witness' other experiments have comprised an investigation as to the cause of bile pigments in the urine in which four animals were experimented on under chloral and killed afterwards, 5311–2; an unsuccessful attempt to produce cancer by innoculation under chloral, 5309–11, 5314; an investigation into the effects of hydrochlorate of cotarnamic acid on three or four dogs and, to his considerable discomfort, gives details, on himself, 5303–8; and some experiments in which the temperature of the body was raised artificially, to ascertain the changes effected in the various organs during fever: the animals, four in all, were under chloral hydrate all the time, and were killed afterwards, 5295–302.

Necessity for Pathological Experiment.—In an examination of the dead human body you have only the débris of the disease, but by experiment on animals and by killing them at different stages you get the complete natural history of the disease, 5325–30; considers that the artificial conditions by which disease is produced do not affect the results, 5339–41; the innoculation is often performed under chloroform, and the pain of the disease, the common lot of most things living, is the only suffering, 5332–4; some diseases bring but little pain, tuberculosis is sometimes painful, but often not, 5335–8.

Believes that experiments are conducted as humanely as possible in this country, 5343–5.

ARTHUR GAMGEE, M.D., F.R.S.

Brackenbury Professor at Owen's College, Manchester, Examiner in physiology at the University of Edinburgh, Examiner in forensic medicine at the University of London, 5346–9, 5372.

Has studied for a short time at the Universities of Leipsic and Heidelburgh, and also in Paris, 5416.

Never performs painful experiments for demonstration.

Gives two courses of lectures, one on systematic physiology for first year's students, in which a few experiments, always under anæsthetics, are performed, 5356; and a practical course for second year's men, in which every painless method of investigation is illustrated, 5356; there are about 40 students in each class, 5384. Details his method of instruction; never performs a painful experiment for demonstration, which he considers may be avoided without detriment, not even those showing the action of poisons, and always destroys the animals before sensibility returns, 5356–7, 5373, 5397–400. Performs experiments before his class with regard to arterial pressure, but always under anæsthetics, 5401–2, and is of opinion that experiment on live animals is essential in teaching, 5386–7.

Has entire control over the physiological laboratory, 5373; never allows experiments unless sanctioned and superintended by himself, 5373; does not allow students as students to perform physiological experiments, 5373.

Properties of vanadium.

The present Platt scholar, Mr. Priestley, a highly competent man, has however made under witness' direction

Gamgee, Arthur, M.D., F.R.S.

a series of experiments as to the action of the poisonous compounds of vanadium, 5374–5; gives details, 5388–92; the results are embodied in a paper which is to be read before the Royal Society, 5385.

Does not believe that a custom of experimenting in private exists among medical students, 5370.

In research the great majority of experiments may be rendered painless, but anæsthetics cannot always be used, gives instances, 5358–66. Observes that in cases of induced disease the animal is usually killed before the malady has run its course, 5367–9.

Private experimentation will decrease.

Vivisection is much too infrequent.

Extent of practice of Experiment on live Animals.—There are very few experimenters in English laboratories, fewer still out of them, 5370; the great delicacy of experiment and the elaborate nature of the appliances now necessary prevent much good resulting from private experiments, 5377, which will decrease, 5421–3; but he hopes that the number of laboratories will increase, 5420, as vivisection is now much too infrequent, 5383. Details the special efforts made at Owens College to encourage physiological research, 5385; and points out that all the most valuable physiological facts have been ascertained by experiment on animals, 5351–2.

Foreign physiologists.

Knows many foreign physiologists, including Professor Ludwig and Claude Bernard, and considers them as humane as any English experimenters; believes anæsthetics are used when possible, although on the other hand unnecessary experiments are performed, 5418.

Not an anæsthetic except in large doses.

Curari.—Gives details of an experiment with curari on two children, 5407–8, and gives reason for believing that in its early stages it is not an anæsthetic, but that in larger doses it does, although possibly not invariably, affect the sensory nerves, 5403, 5407, 5411. Considers that it should not be treated as an anæsthetic, 5413; it is used to eliminate certain disturbing influences, 5412.

Claude Bernard had not performed any experiments on human beings, 5409–10.

Choloforming frogs.

Frogs.—In experimenting on frogs always renders them insensible by decapitating them, in which case he also crushes the head lest sensibility should remain, as is possible in it, 5414, but points out that placing frogs in a very diluted solution of chloroform renders them rapidly insensible, 5415.

Claude Bernard has recently published a book on the use and effects of anæsthetics, 5419.

Incompetent experimenters.

Regulation of Experimentation.—Considers that experiment is in the hands of competent men, and is done humanely and for definite purposes, 5371, and whilst admitting that vivisection by incompetent persons should be forbidden, 5376, believes that there is no abuse in England, 5383, and does not think that legislation as to physiological laboratories is needed, 5376.

Difficulty of saying who is competent.

Cannot consider that reasonable regulations would be a slur on the profession, 5425–7, and is of opinion that a properly worked system of licences would be a great advantage, 5381–3; but is not prepared on the spur of the moment to propose details, 5382, 5396*a*. Hesitates to say where the line should be drawn, certainly ordinary students should not be allowed to experiment; on the other hand any hard and fast line, such as requiring the possession of a medical diploma, would exclude many men who ought to be licensed, especially now that physiology has become a separate study; gives instances, 5378–80, 5393–5, 5404–5.

GEORGE JAMES ALLMAN, M.D.

Emeritus Professor of Natural History in the University of Edinburgh, 5428.

Has never used experiments on live animals for demonstration, 5448, and for private experiments frogs only, 5448.

Action of poisons.

Thinks experiments necessary for the advance of science, 5430–1, but not, if painful, for teaching, 5443; is of opinion that demonstration of the action of poisons is not necessary, the symptoms are so marked and the descriptions are so clear, 5449–51.

Considers that painful experiments should not be repeated unless there be any doubt as to the result arrived at, 5444–6; that anæsthetics should be used whenever they would not interfere with the results, that is in the large majority of cases, 5432–4; that in other cases the operations should be performed under chloroform, even if the subsequent portion of the experiment cannot, 5435–8; and that the animals should be killed after the experiment, if recovery would entail severe pain, 5455–6.

Allman, George James, M.D.

Record of experiments.

Approves in the main of Dr. Playfair's bill, considers it better than Lord Hartismere's, 5439–40; and thinks that a record of all experiments should be kept, 5441–4.

Thinks licensing laboratories and providing for inspection by a competent board would prevent abuses, 5457.

SIR WILLIAM WITHEY GULL, M.D.

Consulting Physician to Guy's Hospital, formerly Fullerian Professor at the Royal Institution, &c., &c., 5458–61.

Transfusion of blood.

Necessity for Experiment.—Considers that experiment is necessary for the progress of surgical and medical science, 5464; points out that our knowledge of dropsical affections, pulmonary apoplexy, engorgement of the liver, and the whole category of such affections, and the possibility of transfusion of blood, flowed from Harvey's great discovery. Refers also to the method of cure for aneurism, and the treatment of spasmodic affections, &c., as results obtained by experiment on living animals, 5525–8.

Vaccination.

Refers to the advantages derived from a knowledge of the processes of inflammation and of tubercle and vaccination, 5529, and observes that the latter has saved more lives than Napoleon's wars destroyed, 5530–2.

Extent of practice of Experiment.—Is glad that the acquaintance with physiological facts is extending over a wider area, but doubts whether there is more vivisection in the way of research than formerly, refers to the time of Harvey as an instance, 5487.

Experiments for Demonstration.—Considers that a certain amount of demonstration by experiment is desirable, but that students ought not to be allowed to perform experiments, 5476–8; the number of painful experiments necessary for demonstration is few, 5505, 5507. Does not consider it often necessary to show the action of poisons by experiments, but every teacher should be allowed to decide such a point for himself, 5507–10, and also whether such experiments as those of Sir Charles Bell on the nerves, which are not now performed before classes, should be repeated; believes, however, that their repetition would excite reprobation both among students and teachers, 5471, 5511–5. At Guy's Hospital about two students only out of 300 attend the physiological demonstrations, 5516.

Anæsthetics.—The great majority of experiments are, as far as possible, performed under anæsthetics, 5466–7; observes that the removal of sensation is very generally necessary to the success of an experiment, 5467; pain complicates and modifies the result, 5469; knows of no experiments in which suffering is a necessary part, with the exception of the very few which relate to the nerves of sensation, 5470.

Believes that careful instruction in a laboratory tends to minimise the infliction of pain; ignorance, not knowledge, is cruel, 5498.

Objection to legislation.

Regulation of Vivisection.—Objects to legislation on the ground that abuses do not exist, and that it would be therefore an undeserved slur on the profession, also because he fears it would check scientific inquiry which would be deplorable, 5472–5, 5484–6; the tendency to rest content with present knowledge is already too strong, 5476. Admits that the existence of an Act against cruelty to animals is not regarded as a slur on the reputation of England, but contends that highly educated men will be restrained by their own humanity where it may be necessary to control the more ignorant by law, 5481–3, although the law applies to both, 5503–4.

Vivisection justifiable on its merits.

Instead of basing his opinion on the fact that legislation in this instance would be class-legislation against physiological investigators, because other sections of the community, who perform operations on live animals, are left untouched, prefers the higher ground, that vivisection is justifiable on its merits, 5495–7, and that there is no abuse of the practice, or experimentation by incompetent persons, to restrain, 5478–80, 5494.

The alleged "maleficent" tendencies of medical students.

Miscellaneous.—Has lived for 15 years in Guy's Hospital with medical students, and been occupied with them for 30 years, and considers the statement with regard to the "maleficent tendencies of medical students" a serious libel on them, and the reference quoted with regard to "frightful" experiments not founded in fact, 5500–2; any cruelty would be scouted by medical students, 5468. Believes that there is a sentiment of humanity among English experimenters, 5465; and doubts whether pure

Gull, Sir William Withey.

physiologists are more careless of suffering than medical men, 5522–4.

The method of research, including the use or non-use of anæsthetics, must be left to investigators, 5506, 5517.

Thinks that a man avowing that the sufferings of an animal under experiment were a matter of indifference to him should be morally controlled, if possible, by the influence of his friends, 5489–92; such indifference is worthy of detestation, an animal's sufferings should be considered as well as human suffering, 5493, and if public opinion should be insufficient to restrain such experimentation, legislation might be needed, 5518–21.

Popular feeling based on insufficient knowledge.

Is of opinion that the popular feeling against vivisection is based on insufficient and incorrect information as to what is really done in England, 5463; and partly caused by a belief that the "mysteries" of life are beyond the pale of legitimate inquiry, 5499.

'Relicta of ancestral relations."

His observation that "should advancing knowledge " show that we have superfluous parts or organs, and " especially if these are liable to disease, what a land of " promise for operations!" made when president of the Clinical Society, was purely hypothetical, and refers to parts which appear to be "relicta of ancestral relations," and which appear to have no direct use; the whole subject of hereditary transmission is very important, and experiments with regard to it should not be forbidden, 5533–44.

WILLIAM BENJAMIN CARPENTER, C.B., M.D.

Registrar of the University of London, formerly Lecturer on Physiology at the Bristol Medical School and at the London Hospital; Author of the "Principles of Human Physiology," the "Principles of General and Comparative Physiology," &c., &c., 5578–82.

Sir C. Bell.

Necessity for Experiment.—Experiment is essential to physiology, the science is based on it, 5601; physiological deductions from anatomical observations can only be tested by experiment, Sir C. Bell, for instance, trusted too much to anatomy, and fell into errors which had to be corrected by other physiologists, 5601.

Practical all knowledge.

John Reid's experiments.

Limits of Experimentation.—Experiments which are likely to increase physiological knowledge should be performed even if severely painful, as for instance, those of Dr. John Reid, with regard to the functions of the *par vagum*, 5603, 5615; no one can forecast the practical advantages which results from the discovery of physiological teaching. Chloroform for many years was merely a scientific curiosity of no practical importance, 5615.

Brown-Séquard.

Even protracted suffering may be justifiable, instances Brown-Séquard's experiments on 40 or 50 animals of various kinds, proving recovery possible after the section of the spinal cord, a thing previously considered impossible; adverts to the practical benefits conferred by this discovery, 5604–5; gives particulars, 5605–14.

Considers that the experiment in which two animals were made to grow together by removing portions of the skin of each and binding them together, might have an important bearing on the treatment of ulcers, 5621–2.

Action of rhubarb.

Thinks the experiment of introducing rhubarb and other drugs into the intestines of a dog and observing its effect on the secretion of the bile a most important experiment, and one which it would be necessary to perform both with and without anæsthetics, 5632–4.

Has known Dr. Brown-Séquard for 30 years, his great reputation for the treatment of nervous disorders was due to the skill he had acquired by means of his acquaintance with physiology derived from experiment, 5635–41.

Painful experiments unnecessary.

Experiments for Demonstration.—Considers that experiments for demonstration are most desirable if painless, but that painful ones, except in teaching toxicology when they are essential, are unnecessary and morally injurious, 5602, 5620.

Frogs.—Frogs have extremely little perception of pain, 5613; believes there is no evidence that they suffer any pain after the section of the spinal cord, 5614.

Purposeless experiments.

Majendie.

Indifference to animal suffering.—Has himself seen a perfect callousness to animal suffering, this was prior to the introduction of anæsthetics, 5627, and is sure that many experiments have been, and are, made simply to see what will happen, 5624–5, 5627; very many of Majendie's were of this nature, 5626–7. Believes, however, that experiments are humanely conducted in this country, 5583–4, and that English physiologists look upon it as a duty to give chloroform when possible, 5628–9.

Carpenter, William Benjamin, C.B., M.D.

Dr. John Reid.

Was intimately acquainted with Dr. John Reid, thinks he may have used some such expressions as those attributed to him with regard to his intense sufferings being a retribution on him, but witness has letters written by him within a few weeks of his death in which he expresses his thankfulness at having been permitted to so far complete his physiological researches, 5630–1.

Doubts the possibility of regulation.

Cock fighting.

The Maine law.

The Anatomy Act.

Regulation of Vivisection.—Effectual regulation without undue interference with the liberty of the subject would be desirable, but its possibility is doubtful, 5585–7, 5596; does not see how any line can be drawn between permissible and cruel experiments, 5594–5, 5644; or how private experimentation can be regulated, 5594; it cannot be put down like cock fighting which must attract attention, 5598, and even that, is more likely to be stopped by a healthy public opinion than by law, 5600; refers to the analogous case of the Maine law which witness thinks rather increases than prevents drinking in private houses, 5598–9, and points out that, although under the Anatomy Act dissection of human bodies except in licensed places is forbidden, cats and dogs and parts of human bodies are constantly dissected in private houses, 5597.

Dr. Playfair's bill.

Considers that it would be most undesirable to forbid experiment on living animals when deprived of sensation, 5642–3; has not studied the details of Dr. Playfair's bill, but fears there would be grave difficulties in working it, 5594.

Introducing boiling water into the stomach of a dog.

Miscellaneous.—The narration of an experiment in which boiling water was introduced into the stomach of a dog in witness' work on physiology was not inserted by witness, but was introduced in a late edition, would not like to offer an opinion about it without knowing its object, 5620.

The Brown Institution.

The funds of the Brown Institution are held in trust by the University of London for maintaining it as an institution for the study and cure of maladies and injuries of animals serviceable to man, 5645–6; and, under a recent legal opinion, for the performance of pathological experiments, 5648–50. Some other points connected with the administration of the trust have been inquired into, but will not be dealt with by the senate until the Commission have reported, 5647, 5651–4.

THOMAS LAUDER BRUNTON, M.D., D.Sc.

Lecturer at St. Bartholomew's Hospital on materia medica, 5655, 5692.

Gives about eight or ten lectures a year, 5702–3. Makes considerable use of experiment in investigation with regard to the action of drugs. Invariably destroys animals after the experiment, 5801–2; his laboratory is too small to allow any spectators except the man who is helping him, 5799–800.

Calabar bean.

Pharmacological Experiments. — Pharmacology has developed rapidly of late, 5667, and many valuable remedies have been discovered by experiment on animals, 5666, 5668; chloral, atropia, apomorphia, and the Calabar bean have been added to the stock of useful drugs, 5670.

Carbonic oxide and carbonic acid gas.

Investigation as to the action of drugs.

Refers to Bernard's discovery by experiment of the difference in the cause of death in poisoning by carbonic oxide and by carbonic acid gas, and to Hüter's employment of this knowledge and consequent saving of life, 5670–4; also to the mitigation of suffering in angina pectoris, consequent on his own observations with regard to the action of nitrite of amyl, 5669; operations are often necessary to discover on what part of the body and how the drug acts, they are nearly always done under anæsthetics, 5660–2; pain resulting from the operation seldom occurs, 5687–8, and is probably of small amount, 5687–80; the chief pain is from the action of the drug, 5664–6, which often has to be observed after anæsthesia has passed away; sometimes, however, the drug can be allowed to act under chloroform, and an allowance made for its influence, 5675–7.

Supply of animals.

The number of animals required in experiments for research varies enormously, 5720; has himself used in all about 150 animals of different kinds, 5748–9, chiefly cats, because they are a convenient size and cheaper than rabbits, dogs cannot be got, asks no questions as to how the cats are obtained, 5731–5; has never had occasion to sew up their lips, 5736.

Nitrite of amyl.

Next to the snake poisoning experiments, his investigation with regard to nitrite of amyl and other substances rank in point of importance, but the number of animals consumed was much smaller, the results being of a marked character, 5751–2; chloroform was not used as it would have very much complicated the experiments, even if used only during the actual operation its residual action would have interfered with that of the amyl, 5753–9.

Cholera.—Used 90 cats in the first series of investigations with regard to cholera, describes the method pursued, and gives reasons for it, 5721–7; no beneficial discovery has yet been arrived at; the experiments are still proceeding, 5728–30, is now at the third series of them, 5747.

Snake-poison.—Believes that an antidote can only be discovered by experiment, and has recently been engaged with Dr. Fayrer in investigations with this object, 5689, and used a large number of rabbits, guinea-pigs, frogs, dogs, pigeons, and fowls, but not so many animals in all as in the experiments with regard to cholera, 5746–7.

The animals were innoculated with the poison, 5750, which witness considers the most powerful thing with which he has experimented, it kills an animal in about half an hour, describes the symptoms, 5681–4; doubts whether the pain caused is as great as in poisoning by strychnia, although the convulsions in the latter case may possibly not be painful, 5685–6, anæsthetics cannot be used, 5683.

Hydrophobia. Has never experimented with regard to hydrophobia; it would be a difficult subject, 5690–1.

Experiment essential. Prussic acid. **Experiment for Demonstration.**—Considers that ocular demonstration is essential in teaching, gives reasons, adverts to poisoning by prussic acid and to the facility with which any one who has seen its action on an animal can recognize and deal with it in a human being, 5716–7; at present this is the only experiment witness performs without chloroform, 5704–7; uses rabbits and cats for these demonstrations.

Poisoning by carbonic oxide or carbonic acid gas is believed to cause no pain, the pain is in the recovery, but is not probably great, 5710–5.

Picrotoxine. Strychnine. The action of organic poisons such as strychnia and picrotoxine is at St. Bartholomew's shown by the lecturer on forensic medicine, 5708–9; the pain is much the same under either, 5814; does not think the action of strychnine can be properly shown under chloroform, which has in fact been used as an antidote to it, 5803; gives further reasons, 5813.

St. Bartholomew's. **Anæsthetics.**—In the reply to the Commission from St. Bartholomew's Hospital with regard to the use of anæsthetics, it should have been stated that anæsthetics were used whenever possible, 5693, 5700–1.

Chloral. It is not safe to give human beings chloral in sufficient quantities to produce anæsthesia, animals do not recover from large doses of it, 5769–70.

Opium. Opium is an anæsthetic, but it disorders the system to a much greater extent than chloroform, and is therefore more dangerous, 5771–5.

Insensibility under narcotics. The assertion, that "in the deepest narcotism produced by morphia or opium, there is no real insensibility to pain," is certainly erroneous, 5804–8; nor is there anything tending to prove that pain is felt but forgotten, or that narcotized animals are sensible to pain, 5809–10.

Describes the means by which cats, dogs, and frogs are put under chloroform, and observes that there is no difficulty at all about doing it, 5737–9.

Curari.—Considers curari is to some extent an anæsthetic, 5693–5, gives reasons, 5696, and refers to Von Bezold's, Schiff's, and Lange's experiments as tending to show this, 5791–5, and observes that after recovery from its effects animals do not show signs of having felt pain, 5796; but admits that its exact properties are involved in uncertainty, 5693–7. In demonstrating employs it for frogs only, 5698–9.

Its effect on the sensory nerves. It can be demonstrated on frogs, especially in spring, that curari affects the sensory nerves, 5699, 5791, and it is probable it also affects the higher animals, 5796. Cannot say whether the scientific world generally takes this view, 5797–8.

Its uses. Considers that curari, in addition to keeping the animal still, also eliminates some of the sources of disturbance resulting from the irritation of the nerve-centre, gives example, 5742, 5744; believes that in some experiments curari is necessary, in others chloroform, administering both is often impossible, gives reasons, 5741–5; doubts whether chloroform could have been used in Dr. Rutherford's experiments on the liver, it would probably have produced nervous congestion in that organ, the curari used would somewhat affect the secretion and would have to be allowed for, 5760–8.

Handbook of Physiology.—Refers to several experiments in the handbook of physiology which are stated to be

"suited for demonstration," and observes that with the exception of two in which there is slight pain, and one in which anæsthesia is allowed to partially pass away that these experiments are painless, 5776–90.

In the experiment demonstrating the functions of the chorda tympani, chloroform prevents some points being demonstrated which could be shown under curari, 5811.

ALEXANDER JAMES SINCLAIR, M.D.

Fellow of, and Examiner at, the College of Physicians in Edinburgh, and formerly Demonstrator of Anatomy at the School of Medicine in Edinburgh, 5817–8.

Has never demonstrated on live animals, 5849–50 but in the course of his investigations witness has used about 30 or 40 frogs, which being only to a small extent capable of feeling pain, were not anæsthetised, 5900–4; they were, however, pithed when possible, 5847–8; some of his pupils were present at these investigations, 5851–3.

Witness' experiments as to the inhibitory action of the nerves on the secretions were tried with and without anæsthetics, but the results being the same, anæsthetics were used subsequently to save pain, 5829–32; they would have been used from the first had there not been a doubt as to the result, 5905–6. Inhibitory action of the nerves.

Anæsthetics.—More frequently uses anæsthetics than not, finds them convenient, 5825; believes that pain is minimized and anæsthetics used as far as possible at Edinburgh, 5833, 5839–40; the general opinion would be against any departure from this course, 5909–10; has generally used anæsthetics in experiments, they keep animals quiet as well as save them pain; believes they are generally used on the latter ground, 5826–8.

Knows nothing of the Veterinary College at Edinburgh, 5834–5.

Experimentation by Students.—Had a casual conversation in 1869 with Dr. Hoggan, then a student, with regard to a theory, *primâ facie* not a very probable one, that the distention of the lungs was caused by the force conveyed by the blood from the heart, in the course of which witness recommended him to test it practically by experiment, 5854–77; considers that as a teacher he was simply doing his duty in giving such advice, 5885–92. Cannot say whether chloroform would have interfered with the experiment, 5899, but if the result were against Dr. Hoggan he would very likely have thought it had, 5907–8; witness would not have considered it wrong to perform it without, 5898, but did not make any reference to the use of anæsthetics in his conversation with Dr. Hoggan, 5821–4. Advised him not to use cats because it is difficult to manage a strange cat before it is chloroformed, 5822, 5893–7. Dr. Hoggan's theory of respiration. Cats.

Young men who display ability and a desire to investigate are encouraged to do so, and embody the results in theses, and for these, when meritorious, medals are awarded, 5878–84. Encouragement of research by talented students.

Regulation of Vivisection.—Objects to inspection on the ground that it is needless, 5836–8, 5911, but does not deny that if experiments are being conducted by inhumane persons it would be desirable, and, he believes, desired by scientific investigators, 5912–3.

PETER DAVID HANDYSIDE, M.D., F.R.S.E.

Lecturer on Anatomy at the University of Edinburgh, Examiner in Anatomy, Physiology, and Surgery at the Royal Colleges at Edinburgh, and at the University of St. Andrew's, &c., 5914–7.

Teaches anatomy during nine months of the year, 5918; took part in originating the Society for Prevention of Cruelty to Animals at Edinburgh in 1839, and was for three years its secretary; sees nothing in vivisection inconsistent with the object of that society, 5923, 5937.

Frogs.—Considers that frogs when subjected to painful experiments should certainly be anæsthetised, 5990–2, or have, as in Edinburgh, their heads cut off, 5986–9.

Anæsthetics.—Physiologists always use anæsthetics when possible, 5975, does so himself, 5957. All experiments at Edinburgh are performed as humanely as possible, 5928–9, 5931. Humanity of experimenters.

Never met with any man such a monster as to assert that he used anæsthetics not to save animals pain, but to save himself trouble, 5976.

Has had no experience with regard to curari, 5993.

Experiments for Demonstration.—Describes in detail the method of demonstrating the circulation of the blood Circulation of the blood in the web of a frog's foot under the microscope, and characterises it as a necessary and painless experiment, 5932–4.

Instances other desirable and painless experiments on newly killed frogs, and gives particulars as to the duration of mobility after death or severance from the living body; the tail of a large watch dog, for instance, exhibited irritability for 20 minutes after being cut off, 5930, 5936.

Advance of science.

Necessity for Experimentation.—Considers physiological and toxicological experiments essential for the advance of medical science, which he regards as still in its infancy, 5949; believes the profession generally is of the same opinion, 5949–50, 5952.

Great results of experiments.

Refers to the discovery of the anæsthetic use of chloroform, of Marshall-Hall's method of resuscitation from drowning, of the reproductive functions of the periosteum, to the advantages resulting from Sir Astley Cooper's, Sir David Barry's, Mr. Syme's, and Sir Benjamin Brodie's experiments, and to the practical bearing of his own observation on the portal circulation, and the parts played by the lacteals, the lymphatics, and the veins in the absorption of poisons, 5951.

Experiments by medical men on themselves.

Refers in detail to the various investigations and researches in which he has taken part, and argues that the advantages resulting from such experiments justify vivisection, while the fact that the lives of several eminent men have been all but sacrificed in making experiments on themselves, which might have been performed on the lower animals, shows its necessity, 5994; in short, experiment is the basis of medicine, 5951.

Would not be a "slur" on medical men.

Regulation of Vivisection.—Does not object to legislation, 5938; thinks there would be no more "slur" felt under legislation with regard to vivisection, than there is with regard to the clauses of the Anatomy Act prescribing three months imprisonment for infringing its provisions, 5962–3.

Licences.

Approves of licensing persons desirous of undertaking original experiments only, but teachers should be free to repeat painless fundamental experiments once before each of their classes, and also to perform private original experiments under anæsthesia, 5940; Considers that persons should be recommended by two laymen, such as justices of the peace, and their ability certified by a teacher of the profession, 5940, the laymen to represent the public in the matter, 5941–2; considers that laboratories should be also licensed or registered, 5940, 5942; believes that very few would apply for a licence: to a great extent popular excitement on the subject has no basis, 5948.

There should also be three professional inspectors with duties analogous to those of the inspector of anatomy, 5943–8.

Dr. Hoggan's theory of respiration.

Experimentation by Students.—Rather opposed than agreed with Dr. Hoggan in his theory, but advised him as an intelligent student of whose ability he had had many proofs, to test it by experiment, and offered him facilities for so doing, nothing was said about anæsthetics, it was taken for granted they would be used, 5919–27, 5981–5; at the time witness advised Mr. Hoggan to undertake these experiments, he had become so expert at anatomy that he had been made demonstrator, and witness had great confidence in his intelligence and ability, gives details, 5964–70; refers to other details connected with Mr. Hoggan's account of his conversations with witness, 5977.

Ability of Dr. Hoggan.

It is a matter of notoriety at Edinburgh that students showing scientific aptitude are recommended by their teachers, and rightly so, to embody the results in the theses which they present, 5971–3.

Experiments by students not frequent.

Dr. Hoggan is wrong in saying that "it was common for medical students to try private experiments" with the knowledge and concurrence of the lecturer, witness never knew of any such case, 5953–5; with the exception of Mr. Hoggan, believes that he never advised any student to experiment, 5978–80, during the whole of the 26 years during which he has taught, 5958–61; any student proposing to make a research involving an experiment on a live animal would certainly be told to go back to anatomy first, if he were not competent to experiment from ignorance of that subject, 5971, 5974.

MR. WILLIAM WILLIAMS.

Principal of the New Veterinary College at Edinburgh, 5995.

Experiments by students.

Neither witness, 5996–8, 6132, nor any of the professors knew anything of the operations referred to by Mr. Mills (*question* 4949), 6049–52, 6111, until his evidence was taken, 6109; has since ascertained from a groom who was present, 6064–9, 6110, that something of the kind took place, 5996–6000; gives details, but Mr. Mills' account of it is somewhat exaggerated, 6026–33. Gives particulars with regard to the maintenance of discipline and responsibility for the proper treatment of animals, 6045–52; cannot say how long this particular horse was kept and operated on, 6023.

Can only account for these experiments being unseen by supposing they took place during the Christmas holidays, 6016–22, 6130–1, when the professors are mostly away, 6150–3.

Are very unusual.

Such practices have not been going on for five years, 6007–8; it is the only case, witness believes, which has ever occurred, 6001–3, 6053–4. The experiments were entirely contrary to the rules, 6025, 6047–8. Such an operation as tenotomy is never permitted on a horse bought for dissection, 6102–6; had these experiments been discovered witness would probably have expelled the students concerned, 6042–4; the regulations, however, might be amended with the view of stopping such proceedings in future, 6004–6; knows nothing of the private experimentation and cat hunts referred to by Mr. Mills, 6144–5.

Cat hunts.

Students are compelled to dissect so many subjects, which they buy themselves, each session, 6038, 6112–4; the animals bought are treated with humanity, 6055, and as a rule are killed the same day, 6036; never performs experiments on cats or dogs, and believes they are never experimented on by the students, 6015.

Operations to acquire facility in bleeding.

The students are allowed to bleed from the jugular veins of animals bought for dissection, but that is all, 6024, 6034–41, 6115–6; does not think they are inclined to perform other experiments, 6141, or that they act otherwise than humanely towards animals, 6146. Except bleeding—which it is essential that students should be able to perform expeditiously, 6033, no experiments are undertaken for the sake of acquiring dexterity, 6149; bleeding being so painless and trivial an operation it is not done under superintendence, 6033, 6140–1.

Lithotomy under chloroform for demonstration.

When animals are bought for dissection lithotomy, under chloroform, 6119–24, is the only operation performed for demonstration on them whilst alive, 6009–14, 6125–9, 6082–3; after the lithotomy the animals are killed; in one instance, referred to by Mr. Mills, a horse was allowed to live some hours, gives particulars, 6142–3, to disabuse the students of the idea that it was impossible for horses to recover from the effects of chloroform, 6057–62; it did not suffer great agony, and was able to eat, 6063. In filling up the return sent to the Commission this experiment was from forgetfulness not included as an experiment for demonstration, 6084–6. Lithotomy is a very rare operation; it may save a horse's life, 6117–8.

Tenotomy.

Does not recollect ever performing tenotomy on a horse bought for dissection, 6078, 6096–101, 6107. The horse in which the jugular vein was blown was not, as Mr. Mills states, allowed to live for some time before being destroyed; it died in about 30 seconds, 6070–5. The object of the experiment was to show that allowing air to enter a vein was dangerous, and that blowing the jugular vein was a speedy means of destruction, 6076–7.

Blowing a vein.

With the exception of bleeding and lithotomy no experiments have been performed within the college for a great number of years, 6129.

Anæsthetics.—Anæsthetics are always used when lithotomy is performed experimentally, 6056; does not perform tenotomy under chloroform, the pain being so slight, 6079–81, 6091; sometimes chloroforms a horse for firing him, 6087, but giving horses chloroform is very difficult, they struggle so severely, 6088–90. Neurotomy being a short although painful operation, witness does not as a rule give chloroform for it, 6092–5.

Neurotomy.

Miscellaneous.—Has been told that the phrase, experiments for purposes of "causation," used by the examiners of the College of Veterinary Surgeons had reference to the doings at Alfort, but does not understand its meaning, 6133–9.

EDWARDS CRISP, M.D.

The results arrived at by vivisection are over-rated.

Vivisected animals for several years; gives particulars; believes the advantages resulting from the practice are much overrated, 6157. Gives instances of erroneous conclusions arrived at by this means, and of the opposite opinions based on these experiments; thinks these facts show the uselessness of much of the experimentation going on, and the desirability of some control, 6157–9; believes also that the pain after the operation is often dvocates estriction, ot abo-tion.

left out of consideration; vivisection should not, however, be stopped, 6157–8. Refers to some discoveries of his own which would not have been obtained if vivisection had been forbidden, 6158; thinks a committee should be formed at each hospital to control experiments and prevent useless ones, 6157; believes emulation is the prevailing incentive in vivisection, 6158; but admits that all vivisectors have thought that they were benefiting the human race, 6161. Refers to vivisection by eight students at once, at Alfort, and other experiments, 6157.

JAMES MADEN HOLT, M.P.

he Society or the bolition of ivisection.

Is a member of the committee of the Society for the Total Abolition of Vivisection, but has not attended any of its meetings, and has only communicated with other members by post, considers that the organization of the society is not at all satisfactory, 6162–71; and had he not been ill, would either have succeeded in reforming it or retired, 6287; it needs the appointment of an executive committee who can take active control and meet frequently, shall retire from the committee unless this is done, 6202–3.

ts defective rganiza-ion.

Believes the society was started in consequence of its being felt that the Society for the Prevention of Cruelty to Animals was somewhat slack in moving in the matter; believes the society is largely supported by the public, cannot give particulars, 6177–85, but will obtain them, 6241. Cannot say when the society was formed, joined it in March last from seeing its advertisement, it has no office, and he believes there has been no meeting of its members, nor as far as he knows of the committee; it has no paid officers to get up details with regard to vivisection, but a sort of prospectus of the objects of the society has been published, 6223–36, 6261–2, 6265.

Informal meetings.

Appointment of fficers.

Statements made on its behalf.

Has never attended or been summoned to any meetings of the committee, 6273–5; but there have been informal meetings of two members of the committee and Mr. Jesse, 6281–4. Cannot say how or by whom the president or committee was appointed, 6263–4; it may have been arranged by Mr. Jesse, 6280–1. Believes Mr. Jesse is treasurer and secretary; is not aware that any auditors have been appointed, 6268–70. Knows nothing of any authority having been given to Mr. Jesse to speak on behalf of the society. The letters which have appeared have been rather from Mr. Jesse personally than from the committee, 6237–40. Knows nothing of the letters laid by Mr. Jesse as "the society's representative" before the Commission as far as witness is aware, neither they, nor those published by Mr. Jesse, were written under the authority of the committee, 6276–9. Has himself given no formal sanction to any of the printed documents published by Mr. Jesse, 6285–6.

Entirely objects to the practice of vivisection.

Vivisection Unjustifiable.— Contends that no alleged benefit can afford sufficient sanction for the practice, 6204–6, 6252, inasmuch as it goes beyond the terms of the divine grant of animals for the use of man, and is contrary to the spirit of scriptural teaching, 6175, even if resulting in discoveries which would save thousands of human lives, 6209–11. Considers that the grant sanctions the infliction of pain on an animal in training it for instance, but not vivisection, 6207–8. Admits that the infliction of pain by the scientific investigator in the hope of benefiting human beings should be regarded with less aversion than that inflicted by the sportsman, but points out that in the latter case the animals are used for food, 6212–22.

Overloading an animal is illegal but not cutting him up alive.

Thinks it wrong that while a man may not overload a donkey, even if it be necessary to sustain the bodily life of his family, vivisection may be undertaken for the sake of finding means to heal them, especially as it is of very uncertain value, and medical men are not agreed on the point, 6175.

Thinks vivisection introduces non-natural conditions, and that the phenomena produced are therefore not to be relied on, 6176. Objects as a rule to inflicting pain on one animal in the hope of benefiting others as unjustifiable, but exceptional cases, such as those instanced, should be dealt with on their merits, 6254–60.

The three great discoveries ascribed to vivisection were made by anatomical research.

Refers to the three great discoveries alleged to have been made by vivisection; viz., the discovery of the circulation of the blood, of the double function of nerves of the spinal cord, and that of chloroform, and quotes observations by Harvey and Sir C. Bell to the effect that they made these discoveries by anatomical research, and only resorted to vivisection to demonstrate them to others; observes that the use of chloroform was discovered by Sir J. Simpson's experiments on himself, 6175–6. Has not, however, from illness, consulted either Bell's or

Harvey's works with regard to their real opinion with regard to vivisection, 6172–4, 6243–4, 6288–92.

Hardening effects of vivisection.

Thinks the practice of vivisection, even under anæsthetics, has a hardening effect on the community at large, and specially on medical men, would himself distrust a medical man who vivisected; gives and discusses reasons, 6293–9, and there are persons who will not employ such a one, 6176. Believes that the eminent medical men, who have stated before the Commission that they have practised vivisection, will in consequence not be consulted again by many of their former patients, 6193–5. Would not himself like to be treated by a man who had drawn his knowledge from vivisection, 6253; thinks no humane person would, 6176, even in case where a peculiar knowledge had been obtained, such as that of Dr. Brown-Séquard, with regard to diseases of the nervous system, 6319–22. Fears lest the practice of experimentation on suffering may lead to abuses, 6323. A man who would inflict great pain for eight or nine hours on a dog in order to test the action of a drug might make a similar experiment on man, 6324.

Vaccination.

Inclines to the belief that anatomical students grow somewhat callous in their treatment of dead bodies, 6299–300. Thinks studying books on vivisection likely to exert a hardening effect on the mind, and objects to such a method of medical education; but is not prepared to say that he would refuse to avail himself of the knowledge a medical man had obtained by studying such books, 6247–51; fears that vivisection and vaccination will get mixed up in the popular mind, and that considerable agitation will be the result, 6176; and that if the practice of vivisection is continued, that some of the privileges enjoyed by medical men will be called in question, 6176, 6302–3.

Non-scientific vivisection.

Does not think that it is justifiable to castrate lambs and spay sows for the sake of being able to fatten them more easily, and considers trapping rabbits, and thereby inflicting many hours suffering on them, very cruel, 6310–4. Does not approve of class legislation, but would like to see a measure dealing with the cutting of animals generally, whether for scientific or non-scientific purposes, 6199–201, 6304–9, 6315–7.

Legislation.

Is aware that the Society for the Prevention of Cruelty to Animals has sanctioned a bill tolerating vivisection under anæsthetics, but does not consider that sufficient, believing the experiments would not be conducted without pain, 6186–92, but that the law would be evaded; adverts to the temporary action of all, and doubtful effects of some, of the drugs regarded as anæsthetics; and observes that even if their effectual use were insured. in many experiments they could not be used, 6176.

From general reading, gathers that there is a considerable amount of consent that restrictions on vivisection for teaching purposes are desirable, 6301. If abolition is decided against, is not prepared at the moment to offer suggestions as to its restrictions, 6196–8.

MR. GEORGE HENRY LEWES.

Is not a member of the medical profession, but received a medical education and has studied physiology, especially during the last 20 years, for the most part in foreign laboratories, 6325–8, 6334, 6353.

Experiments on the domestic animals.

Has performed a great many experiments, 6354, nearly all relating to the nerves, 6365, mostly on frogs and other cold-blooded animals, 6357, and not more than a dozen rabbits and pigeons in a dozen years under anæsthetics, 6361–4; always kills the animal after the experiment if it is likely to suffer much pain, 6367. Could not himself bear to experiment on dogs or cats, 6360, 6379, rabbits would nearly always do instead, 6413–7.

Boiling a frog alive.

Very rarely performs an experiment causing pain, 6358, but instances one, 6386–92. Has not performed Goltz' experiment of boiling a frog alive, but has dipped a pithed frog into boiling water to disprove his conclusion, 6380–1.

Consideration for animal pain is much greater in England than abroad, especially than in Italy and Spain, 6372–3; does not think there is any danger of indifference increasing with the growth of experiment here, our national morality has been more cultivated in that direction, 6376.

Vivisection a necessity.

Useless experiments.

Vivisection is a painful necessity, 6330; wishes there were more thoughtful experimenters, especially in England, 6354–6, and fewer needless experiments, 6374; but is aware that a great deal of experiment is quite useless, and cannot possibly prove that which it attempts to prove, and that the disturbances caused by the experiment often invalidate the result, and that phenomena really due to one cause are ascribed to another, gives instances, 6330.

Experiment by students.

Doubts whether experiments on live animals are undertaken by incompetent persons, 6339. Experiment is very troublesome and tiresome, and students have not generally sufficient interest in it to undertake it; gives an instance of the lack of interest taken by students in scientific matters not coming into their examinations, 6339–41.

Untrained students should be made to feel the difficulty of getting satisfactory results rather than—except in the case of students of a really scientific bent—encouraged to experiment, 6341–2.

Useless repetition of experiments.

Before a painful experiment is undertaken it should be clear that a valuable result is probable, 6330, 6347; experiments to see what will happen are preposterous, 6348. Considers that there is much useless repetition all over Europe whenever a new experiment is published; repetition is justifiable only when there is doubt as to the result, 6330, even if the pain is small, 6345.

Experiments for demonstration.

Considers that experiments on live animals are objectionable in medical education, except in a few cases in which they must be seen, 6411–2.

Distinguishes between sensibility and the sensation of pain.

Sensation of Pain.—Thinks pain is quite as special a form of sensibility as hearing, seeing, or tasting, and may in some animals co-exist with other forms whilst in others it may be absent, 6383, 6396–400, 6403; refers to cases and conditions in the human being in which general sensibility exists without sensibility to pain; points out that savages feel pain less than civilized men; gives instances to show that animals with serious wounds cannot feel much pain, and argues that frogs, fishes, and reptiles, have no sensibility to pain, 6345–6, 6382–5; discusses the amount and kind of sensibility remaining in man after section of the spinal cord and under curari, 6405–9. Believes motor impulse essential to conscious sensation, 6370, 6410. Sensibility exists in all reflex

action, and in pithed frogs, but not the capacity to feel pain, 6393–402.

Thinks the amount of suffering caused by vivisection is very small, the pain is in the wounding not the wound, 6345.

Anæsthetics.—Gives frogs ether instead of chloroform, as the latter kills them, 6359; for warm-blooded animals uses ether, chloroform, or chloral, 6366, but never curari, which he regards, however, as a true anæsthetic, 6368–71.

One of the greatest experimenters abroad told witness that he always used chloroform, but simply for the sake of his own convenience, 6376–8.

Desire stopp abuses

Regulation of Vivisection.—Admits the right of society to restrict the right of experimenting on live animals to competent persons, 6331–4; and would be glad to see the present useless repetition of experiments all over Europe stopped, if it could be done without interfering with the prosecution of science, 6345; fears that real physiologists might be excluded if only those who were generally recognized as such were permitted to experiment, he should himself have been excluded by such a rule, although he believes there are very few medical men who have done more physiological work than he has himself, 6334.

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Does not know of any country having legislation, 6252, and thinks it is utterly impossible to legislate satisfactorily, on the subject, 6330; considers that vivisection, like literary criticism, is a benefit to society, and although both cause pain and are liable to abuse, thinks they can be controlled more effectually by educated public opinion than by legislation, 6334–8, 6349–50; restriction must come from professors inculcating a sense of responsibility both as regards animals and as regards science which is now encumbered with much useless lumber, 6345. If professors would explain to students the excessive difficulty of getting results, useless experiments would decrease, 6330, 6342.

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